

IX

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IN ALL ITS APPLICATIONS TO

PHARMACY, ARTS, AND MANUFACTURES.



WILLIAM CROOKES, F.R.S., &c.

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THE CHEMICAL NEWS.

VOLUME XXXIII.

EDITED BY WILLIAM CROOKES, F.R.S., &c.

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ON SOME FORMS OF SELENIUM, AND ON THE INFLUENCE OF LIGHT ON THE ELECTRICAL CONDUCTIVITY OF THIS ELEMENT.*

By HARRY N. DRAPER, F.C.S., and RICHARD J. MOSS, F.C.S.

IN a paper read before the Society of Telegraph Engineers† on February 12th, 1873, Mr. Willoughby Smith announced the remarkable fact, that a bar of crystalline selenium, through which a current of electricity passes, has its conductivity increased 15 to 100 per cent when the bar is exposed to light. The light from an ordinary gas-burner placed at a distance of several feet increased the conductivity 15 to 20 per cent. Mr. Smith satisfied himself that alterations in temperature in no way affected this result, by placing the selenium in water, in such a manner that the light from burning magnesium ribbon held some inches above the bar passed through about an inch of water before falling upon the selenium. Under these circumstances, the conductivity of the bar was found to increase more than two-thirds, returning to the normal conductivity when the light was withdrawn.

Lieutenant Sale, in a communication made to the Royal Society,‡ describes a series of experiments undertaken with the object of ascertaining the relative effect upon the electric resistance of selenium of the light in different parts of the spectrum. He found that in the solar spectrum the conductivity is least in the violet, and increases as the red is approached, attaining its maximum in a position just on the outside edge of the red rays at the red side. The conductivity in this position is greater than in diffuse daylight, but very considerably less than when the selenium is exposed to full sunlight. Mr. Sale observed that the effect of light is apparently instantaneous, but that the return in darkness to the normal resistance is not so rapid. He corroborates the statement of Mr. Willoughby Smith, already cited, that the varying resistance is in no way due to alteration of temperature of the selenium.§

Soon after the publication of Mr. Smith's observations, we undertook a series of experiments with the object of, if possible, determining the precise molecular state of selenium, which exhibited this phenomenon of diminished electrical resistance under the action of light, and the conditions necessary for its production.

It would here appear necessary to give a brief *resumé* of the state of our knowledge of the physical properties and relations of selenium. This is of the more importance

because little, if anything, has been added to that knowledge for nearly twenty years, and because the statements in some of the acknowledged text-books are not only insufficient, but often discordant with the results obtained by the *savants* to whom we owe all that up to this time has been done in relation to the subject.

As we would desire to avoid matter which is supplied by handbooks of chemistry, or details not directly bearing upon our investigation, it must be understood that we note here only those hitherto observed and not widely known characters of selenium which seem to us to be in intimate relation to the phenomena we have made the objects of experiment.

Selenium, discovered by Berzelius in 1817, was carefully studied by that chemist, and it is through his researches, and those of Regnault, Mitscherlich, and Hittorf, that we have almost all our knowledge of the physical characters of this element. It is upon their authority that the following statements are made.

Selenium may exist in several different forms:—

1. As a vitreous mass, with conchoidal fracture.
2. As a red amorphous powder precipitated from selenious acid or selenites by the action of reducing agents.
3. In the form of minute crystals deposited from its solution in bisulphide of carbon.
4. In crystals deposited from solutions of the alkaline selenides exposed to the air.
5. As a granular body resembling, almost completely, metallic cobalt or cast-iron, and obtained by the heating and slow cooling of either of the three first-mentioned forms.

The first three modifications, it should be mentioned, resemble one another in their physical and chemical relations, and must be regarded as different conditions of the same allotropic form of selenium.

It is here only necessary to speak of vitreous selenium and of its heat-produced allotropic modification, the granular variety, or as it has been felicitously called by Regnault, metallic selenium.

Vitreous selenium has no definite point of fusion. At temperatures exceeding 60° C. it softens, becoming gradually softer with increased heat, and being perfectly fluid at 250°. When rapidly cooled from this temperature, it returns to its original condition. At normal temperatures it may be kept without change of state for many years, and is probably under these conditions perfectly stable. It is, though very sparingly, soluble in bisulphide of carbon. In thin films, it appears by transmitted light of a beautiful ruby-red colour. Its specific gravity is, according to Schaffgotts, 4.276.

When this vitreous selenium is maintained for some time at any temperature between 94° and 200° C., and is then slowly cooled, it is found to have assumed a metallic ap-

* From the *Proceedings of the Royal Irish Academy*, vol. i., Ser. II. (Sci.).

† *Nature*, vol. vii., p. 203.

‡ *Proceedings of the Royal Society of London*, vol. xxi., p. 283.

§ No experiments are adduced in support of this statement.

pearance, and to have a grey granular fracture. It is now, we ourselves find, perfectly opaque to light, even in the thinnest films. Its specific gravity has increased to 4.796. When heated it does not soften, but at 217° fuses without taking any intermediate pasty condition. At 250° it is perfectly fluid, even when the mass is considerable; and when rapidly cooled returns, without any tendency to crystallise, to the vitreous, non-metallic modification. All that has up to the present been made known as to the electrical relations of selenium may be very shortly told.

Solid vitreous selenium cannot, according to Berzelius,* be rendered electrical by friction, but, on the contrary, Bondsdorff† states that when rubbed in very dry air it has this property.‡ Knox found that fused selenium conducted the current of a battery of sixty pairs. Hittorf§ found that granular selenium at normal temperatures conducted sufficient of the current of one Grove's element to deflect the astatic needle of a galvanometer having 200 convolutions 17° , and that when the selenium was heated to 210° in a small crucible the needle marked 80° . But when the temperature reached 217° (the point of fusion of granular selenium), the needle went back suddenly to 20° .

The action of light as probably effecting some change in the allotropy of selenium was not wholly unsuspected prior to Mr. Smith's observations. Gmelin mentions exposure to sunlight as a favourable condition for the precipitation of selenium from dilute solutions of selenious acid by sulphurous acid; and Hittorf, while noting the likelihood of such an influence, was unable to detect it, and was obliged to attribute the observed change of amorphous into crystalline selenium, while drying in sunlight, entirely to the effect of heat. In pursuing the line of research we have marked out for ourselves, we have been obliged to repeat much already published work, which with improved means of experiment has lost somewhat of its significance. We have thus encountered several apparently contradictory statements, some of which our experience has either failed to verify, or has placed in a new light; and we have been convinced that the properties of this remarkable substance are but imperfectly understood, and still present a wide field for investigation. Vitreous selenium is, we should say at the outset, apparently an absolute non-conductor of electricity. We have been unable to obtain any deflection of the very sensitive astatic needle of a high resistance galvanometer, when the thinnest films of selenium, of the continuity of which we could assure ourselves, are interposed in the circuit of ten Leclanché elements. The difficulty of producing very thin films of absolute continuity disposes us nevertheless to state our belief as to the complete non-conductivity of vitreous selenium with some reservation.

As might be expected from this character, selenium in the vitreous form becomes electric by friction. So easily indeed have we invariably obtained this result that we find it difficult to understand the contrary conclusion of Berzelius.

Hittorf, as has been stated, found that when vitreous selenium is converted into the granular form its electrical resistance diminishes directly with its temperature, but that when 217° is attained the resistance is suddenly and largely augmented. In repeating this experiment we have obtained results concordant with those of Hittorf, who appears, however, not to have exceeded the temperature of 217° . Going beyond this point, we find that the resistance diminishes up to the point of complete fusion of the selenium, being at its maximum at 250° . We have obtained also the remarkable result that when the vitreous selenium resulting from the rapid cooling of the completely fused granular form is quickly heated, it begins to conduct the current at a temperature between 165° and 175° , and that its resistance diminishes, not only up to

the extreme range of the mercurial thermometer, but so much beyond that it is still diminishing when, owing to the rapid volatilisation of the selenium, it has been found necessary to terminate the experiment.

It has hitherto been assumed that there is but one granular form of selenium, and that this is a conductor of electricity. We have, however, obtained a granular form in no way optically differing from granular selenium of comparatively low resistance, through a rod of which, 16 m.m. long and 3 m.m. diameter, the current of ten Leclanché cells does not in the least deflect the needle of our highly sensitive galvanometer. Nor does light, so far as our experiments have yet gone, diminish the resistance of this modification. We have, on the other hand, succeeded in obtaining bars of granular selenium through which the current of one Leclanché cell causes a very considerable deflection of the needle. This form is, we find, in its electric resistance almost unaffected by light. Between these two forms of granular selenium—the apparently non-conducting and the comparatively highly conducting—there is another of intermediate resistance. This modification is highly sensitive to light; its conductivity when in the form of flat bars increasing in direct sunlight 75 to 100 per cent, and in artificial light in ratios ranging from 10 to 50 per cent.

One form of granular selenium, as Hittorf first showed, has its resistance considerably diminished by heat: indeed, he says that could it be heated red hot its conductivity would not be inferior to that of the metals. Our own experiments confirm the diminution of conductivity by heat, but we have found, in at least one modification which we have produced, a body which so far conforms to the metallic type as to have its resistance strikingly increased by heat. We have made bars of selenium which when placed in the circuit of a battery and galvanometer, have shown a deflection of 48° , while upon completing the circuit of a nitric acid battery, the current of which heated a spiral platinum wire surrounding the bar of selenium, the needle gradually fell to 15° , as the temperature of the bar became greater.

For the present we refrain from comment upon these results; and although we have been engaged during many months in this investigation, we defer details of our experiments, and especially of the conditions under which the different electrically resisting and light-sensitive forms of selenium are obtained, until with larger experience we may hope to bring before the Academy results tending more closely to the solution of the questions we have proposed to ourselves.

ON PYROLOGY, OR ANALYSIS AND SYNTHESIS BY MEANS OF THE BLOWPIPE.*

By W. A. ROSS.

(Continued from vol. xxxii., p. 252.)

(7). It seems incredible, and yet is undoubtedly a fact, that there are English chemists—accomplished ones too—and even public analysts almost entirely ignorant of the analytical use of the blowpipe. To foreigners, especially Germans, it seems strange that we have not in England any *systematic* training, by means of lectures and practical teaching, in blowpipe analysis, on the pattern of the famous Freiberg University. Such a course might surely be instituted by a rich Government like ours without much difficulty, in Jermyn Street, under the direction of one of the old Freiberg students, as, for instance, Her Majesty's Inspector of Mines, within whose superintendence the students might make periodical visits to the most characteristic mines, which that gentleman knows so well: so that this reproach may be taken from us. It

* Gmelin, "Handbook," ii., p. 236

† Gmelin, *l.c.*

‡ Gmelin, *l.c.*

§ Poggendorf, *Annalen der Physik und Chemie*, Bd. lxxxiv., p. 214.

* The term "pyrological" ("pyrologischen Versuche") has been adopted by Professor Richter, of Freiberg, in a letter to the writer.

is not a sufficient reply to say that "there is a course of blowpipe instruction in almost every English chemical class or college." Every chemist who has been to Freiberg, or even to the American colleges, knows how inefficiently the blowpipe is taught, or rather, how effectually it is neglected in our otherwise excellent schools of chemistry. If this point be controverted, it will not be difficult to take one of our most generally received analytical textbooks, and show what extraordinary and incorrect statements are there made with regard to blowpipe analysis, nor does an inspection of some of the English pyrological instruments and apparatus at all reassure us. I have lately, incredible as it will seem, been shown an instrument as the *only* kind of blowpipe in use in a public laboratory in London, having an aperture at the jet, *at least a quarter of an inch in diameter*; and, to make matters worse, filled both with the blast and with aerated coal gas! I saw a youth blowing through such a machine as this by means of a mouthpiece cleverly extemporised with a glass funnel, and warned him that he would thus probably injure his lungs, a misfortune which would of course be attributed to "the blowpipe," instead of to the personal use of such a *tuyere* as this.

(8). It may be as well to mention here that blowpipes like that devised by Von Frick, having the operator's breath, or blast from any other source, and the gas for ignition conveyed by the *same* tube, are utterly useless for these purposes; that Bunsen burners are useless for these purposes; that Fletcher's most ingenious, and no doubt otherwise invaluable "hot blast blowpipe" is almost useless for these purposes, which are most erroneously supposed to be *always* best effected by the greatest heat. Let any student who really wishes to learn this science produce a blue pyrocone from a candle by a hand blower, and then with a mouth blowpipe held in the other hand, direct a blast *across* this blue pyrocone, so as to dissect it; he finds it to be a *solid* mass of blue flame. If he now perform the same operation on the pyrocone from a gas jet, the pyrocone is found to be slightly hollow, while the pyrocone from a Bunsen burner has only the merest thin shell of circumscribing ignited gaseous matter. It is obvious, therefore, that the effect of holding the fragment or paste of a mineral or other essay, in the middle of these three pyrocones, must be different in each case; yet how seldom in England do we see a candle or oil lamp used for important results which can be obtained *solely* by its use?

(9). It is obvious that in order to properly systematise the science of blowpipe analysis, we ought to reduce it to as close analogy as may be with the procedure in ordinary chemical analysis, which has been found to answer so well. I have, therefore, in my method discarded the preliminary use of the *salts* borax and microcosmic salt, and commenced attacking substances with the pyracids "Boric and phosphoric anhydride." *Boric acid*, indeed, contrary to the account found in most of our chemical works, dissolves before the blowpipe no oxide whatever completely, except those of the alkaline metals, and that of silver a little, but its reactions thus are a thousand times more valuable than if it really (as we are told) dissolved all those, while, by adding pyrologically a *very small* proportion of alkali (not quite 5 per cent) we obtain a still acid menstruum which is itself extremely soluble in water. *Phosphoric acid*, on the other hand, is, before the blowpipe, the most powerful unmixed solvent in the world, dissolving gold leaf to a bluish violet glass quite rapidly. In either case we can further, if we like, obtain an acidulated water solution, in which precipitates can be produced by alkalies, &c., just as in "the wet way," but this process requires to be worked out, and shall not therefore be further alluded to here.

(10). It is necessary, however, to lightly touch upon one part of the subject, not invidiously but conscientiously, before proceeding to details. However invaluable the application of the symbolical and algebraical process of ratiocination may be, assuming its groundwork of facts to

be correct, to what may be termed the metaphysical chemistry of modern times, it seems insufficient when absolutely and completely substituted for practical knowledge, and reasoning in mere English,* and, indeed, it is obvious that, in England at least, there is a tendency among our best writers, even on chemical metaphysics, to express their thoughts, not only in English, but in the very plainest English which has ever been in the possession of Her Majesty or her predecessors. There is no art so difficult of attainment as simplicity, and we may safely regard the value of an invention (or the description of one) in the inverse ratio to its complication. Whoever takes the trouble to compare the amazing hieroglyphical work of the communistical chemist Nacquet, translated into English the other day, with the luminous "essays" of the marvellous apothecary Scheele;† or Axiom V., Book I. of Newton's Optics, on the ratio between the sines of incidence and refraction in a ray of light, with the ordinary scientific account of the same simple law given in our modern mathematical or physical works, will soon see that the advantage in every essential particular remains with the adopters of expression by means of language. Besides, no one, not even the most eminent philosopher, can afford to discard the very great advantage of addressing himself to thousands—perhaps millions—of his fellow creatures, instead of to a few transcendentalists who cannot spread his opinions beyond their own clique.

REPORT ON THE DEVELOPMENT OF THE CHEMICAL ARTS DURING THE LAST TEN YEARS.‡

By Dr. A. W. HOFMANN.

(Continued from vol. xxxii., p. 286.)

THENARD, on bringing peroxide of hydrogen into contact with his tongue, in order to ascertain its taste, found that it was whitened. The cuticle was also blackened, and at the same time a violent itching was excited. Litmus paper without any previous reddening was at once decolourised, as was also turmeric paper.

In 1853 Chevreul|| undertook comparative experiments on the bleaching power of hydrogen peroxide. Its concentrated solution speedily turned syrup of violets green, oxygen being set at liberty. For the following experiments dilute colour-solutions were used—namely, syrup of violets, tincture of litmus, extract of peach-wood, and extract of logwood. The results were as follows:—

Time.	Violets.	Litmus.	Peach-wood.	Logwood.
10 mins.	Imperceptible.	Slight bleaching.	Change to rose.	—
24 hrs...	Complete bleaching.	Almost complete bleaching.	Turns yellow.	Turns yellow.
80 hrs...	Complete bleaching of all the solutions.			

Decolorisation is therefore effected less rapidly by peroxide of hydrogen than by chlorine. Tessié du Motay and Maréchal§ mention it as one of the agents which they propose for bleaching tissues, which, after treatment with permanganate of potash, they recommend to be steeped in a solution of peroxide of hydrogen. But it had been much earlier applied as a bleaching-agent by Thénard¶ himself for a particular purpose—namely, for restoring old

* The controversy at present proceeding between Dr. Frankland and Mr. Wanklyn in the CHEMICAL NEWS, with reference to water analysis, may be cited as an illustration of this point.

† Scheele seems to have been a German, not a Swede, settled as a common apothecary, at Köping, in Sweden.

‡ "Berichte über die Entwicklung der Chemischen Industrie während des Letzten Jahrzehends."

|| Chevreul, *Comptes Rendus*, lv., 735.

§ *Bull. Soc. d'Encouragement*, 1867, 472. Dingler, *Polyt. Journal* cxxx(2)iv., 526.

¶ Pélouze and Frémy, *Traité de Chimie*, 1861.

oil-paintings and drawings. White-lead in old paintings, which has become blackened by the gradual action of sulphuretted hydrogen, is converted into sulphate of lead by dilute solutions of peroxide of hydrogen, and thus restored to its primitive colour. A fine drawing by Raffaele, with superimposed white which had become spotted with black, was completely cleansed by a solution which contained at most five or six times its volume of available oxygen, and the paper did not suffer.

A peculiar, hitherto secret, application of this bleaching-agent has been recently made public by A. v. Schrötter.* During the last few years bottles labelled "Eau de Fontaine de Jouvence, golden," and containing about 140 c.c. of a colourless liquid, have been sold by perfumers in great cities. The price demanded is about 20 francs, and to them, as it appears, is due that offensive blonde shade of hair which holds an intermediate place between ash-grey and bright yellow, and attracts the attention of the spectators and the curiosity of observers by its *piquante* unnaturalness. According to Schrötter this secret nostrum is merely a solution of hydrogen peroxide made stable by copious dilution, and by addition of a small quantity of acid,—apparently nitric acid. According to Schrötter's careful examination it contained 6 volumes of available oxygen: 1000 grms. of the liquid would therefore contain 8.6 of available oxygen, or 18.3 of peroxide of hydrogen. As may be imagined, however, in case of an easily decomposable body, the bottles do not all contain solutions of equal strength. An examination conducted in the laboratory of the University of Berlin showed, in 1 volume of the solution, 9.4 to 9.8 vols. of available oxygen, corresponding to 13.6 grms. O, or 28.9 grms. H_2O_2 , per litre. A bottle costing 20 francs yields the purchaser 2.5 to 4 grms. of this substance in solution, and effects its purpose completely, though slowly, within four to six days, thus strikingly illustrating the great efficacy of peroxide of hydrogen. The name of the perfumer who understands how to speculate so successfully upon the purses of his fair contemporaries, and who deserves to be known to posterity, is E. H. Thiellay, of London.

ON AN INSTRUMENT FOR MEASURING THE DIRECT HEAT OF THE SUN.†

By Prof. BALFOUR STEWART, LL.D., F.R.S.

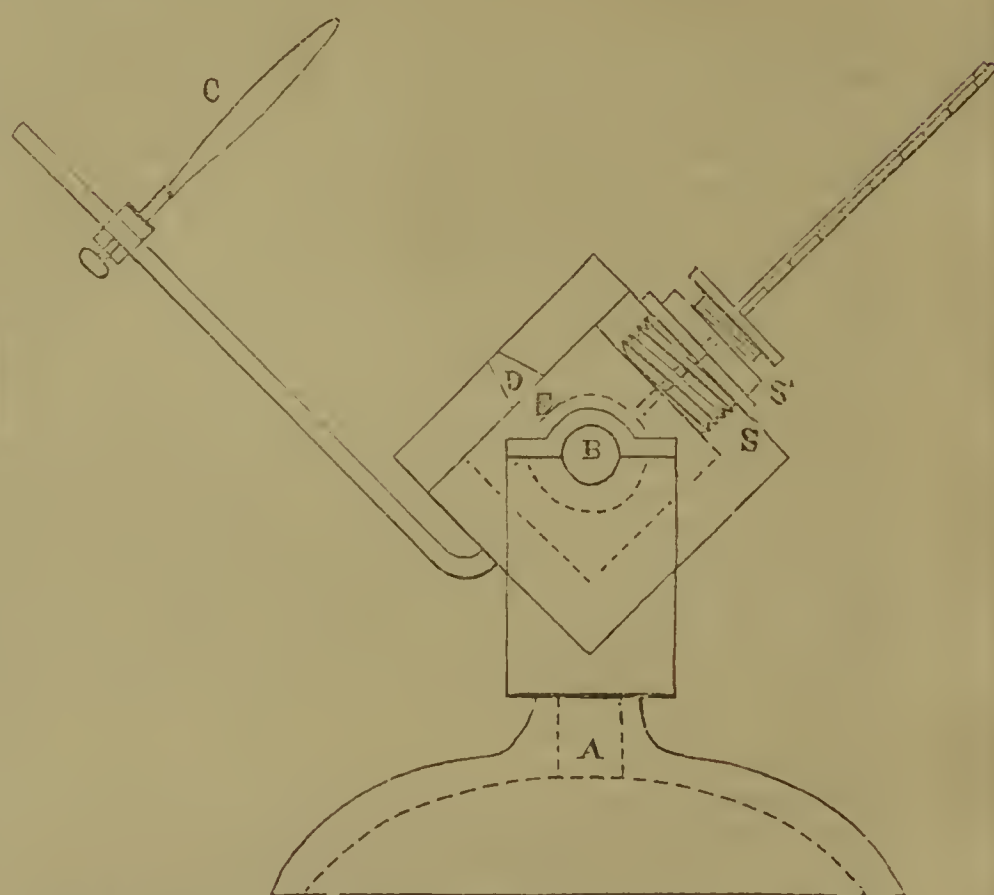
THE instrument generally employed for giving the radiant energy of the sun's rays acts upon the following principle. In the first place the instrument is sheltered from the sun but exposed to the clear sky, say for five minutes; let the heat so lost be termed r . Secondly, the instrument is turned to the sun for five minutes; let the heat so gained be termed R . Thirdly, the instrument being now hotter than it was in the first operation, is turned once more so as to be exposed to the clear sky for five minutes while it is shielded from the sun; let the heat so lost be termed r' . It thus appears that r denotes the heat lost by convection and radiation united when the instrument, before being heated by the sun, is exposed for five minutes to the clear sky, while r' denotes the heat lost by these same two operations by a similar exposure after the instrument has been heated by the sun; and it is assumed that the heat lost from these two causes during the time when the instrument is being heated by the sun will be a mean between r and r' , and hence that the whole effect of the sun's rays will be in reality—

$$R + \frac{r + r'}{2}.$$

Now although this assumption may in the average of a great number of experiments represent the truth, yet in

many individual cases it may be far from being true. It would therefore seem to be desirable to get rid of this uncertainty by constructing an instrument in which we are sure that the causes of variability are not allowed to operate.

These causes of variability I have attempted to get rid of in the following manner. With the help of Mr Jordan, mechanician at Owens College, the following instrument has been constructed. It consists of a large mercurial thermometer with its bulb in the middle of a cubical cast-iron chamber, this chamber being of such massive material that its temperature will remain sensibly constant for some time. The chamber with its thermometer has a motion in azimuth round a vertical axis, A, and also a motion in altitude round a horizontal axis, B. A three inch lens, C, of 12 inches focal length is attached by means of a rod to the cubical chamber so as to move with it. The nature of this attachment will be seen in the figure. Thus the whole instrument may be easily moved into such a position that the lens as well as the upper side of the chamber which is parallel to the plane of the lens may face the sun, and an image of the sun be thrown through a hole, D, in the side of the chamber upon the thermometer bulb, E.



The stem of the thermometer protrudes from the chamber as in the figure. A screw, S, somewhat larger in diameter than the bulb of the thermometer is made use of to attach the thermometer to its enclosure, and a smaller screw, S', pressing home upon india-rubber washers enables the thermometer to be properly adjusted and kept tight when in adjustment.

In the present instrument the internal diameter of the chamber is 2 inches, while the bulb of the thermometer is about $1\frac{1}{4}$ inches in diameter.

The scale of the thermometer is very open, more than an inch going to one degree. I have generally allowed the image of the sun given by the lens to heat the thermometer bulb for one minute, during which time an increase of temperature, not exceeding in any case two degrees, has been produced.

As far as principle is concerned there appears to be no objection to the present instrument, nevertheless it is open to a very serious practical objection. The scale being so very open, the stem comprehends only a few degrees; frequently, therefore, the temperature is such that the extremity of the mercurial column is either below or above the stem. Now the thermometer has a small upper chamber, and by means of a method of manipulation well known to those who work with thermometers, it

* Berl. Chem. Ges., 1874, 980.

† A Paper read before the Manchester Literary and Philosophical Society.

is possible to add to or take away from the main body of mercury in the bulb so as to keep the end of the mercurial column always in the stem. But experience has convinced me that for a thermometer with such a large bulb, frequent manipulation of this kind is not unattended with danger to the bulb. On this account the instrument in its present form is, I conceive, unsuited for steady work in an observatory from year to year.

It is, however, possible without any appreciable sacrifice of the scientific principle of the instrument, to alter it in such a manner as to remedy this defect. Without altering the size of the bulb, I should propose for a permanent instrument a stem, say 18 inches long, with a bore of such diameter that the stem should embrace a range of temperature between 20° F. and 92° F. Thus somewhat less than 5° will go to the inch. The stem might be protected from the risk of accident by an appropriate shield. Let such a thermometer be heated for two minutes and the size of the lens be somewhat increased. In this case a rise of something like 5° F. will be obtained, and this heating effect might very easily be estimated to one-hundredth of the whole, while the same thermometer would serve for all the temperatures likely to occur in these islands during the course of the year.

I ought to add that a pasteboard cover gilded on the outside is made to surround the chamber, and also that between the lens and the chamber there is a pasteboard shield with a hole in it to permit the full rays from the lens to pass—the object of this shield being to prevent rays from the sun or sky from reaching the instrument.

In such an instrument r or the change taking place in the thermometer before exposure to the sun will in all probability completely disappear, while r' will be extremely small. At any rate we may be quite certain that—

$$R + \frac{r+r'}{2}$$

will accurately represent the heating effect of the sun.

We may probably suppose that in the same instrument the lens (which must always be kept clean) will always stop the same or nearly the same proportion of the solar rays. But the lens of one instrument may not stop the same proportion as that of another instrument. This, however, is no objection if it be borne in mind that the instrument is a differential one. In practice there would be some standard instrument which would be retained at a central observatory, and all other instruments would, before being issued, be compared with it. It would be thus possible to compare together the indications of various instruments working in different places provided that these, before being issued, had their co-efficients determined at the central observatory.

LABORATORY NOTES.

By SERGIUS KERN, St. Petersburg.

(1). *On a Reagent for Uranium.*—With potassium ferrocyanide (K_4FeCy_6) a solution of a uranic salt yields a brown precipitate of uranium ferrocyanide. The precipitate obtained much resembles the precipitate of copper ferrocyanide, but may be distinguished by the solubility of the precipitates in hydrochloric acid, viz., the uranium ferrocyanide dissolves easily even in diluted hydrochloric acid: the corresponding copper salt is insoluble in acids. This reaction may be used for the separation of copper from uranium. The uranium ferrocyanide dissolved in hydrochloric acid with a few drops of nitric acid gives a *green colouration* after being boiled for some minutes: this reaction is proposed as a test for uranium salts.

(2). *On the Use of Cuprous Oxide.*—This compound is easily prepared by boiling a solution of copper sulphate with sugar and an excess of caustic potash. As the cuprous oxide (Cu_2O) obtained in the form of a red powder

is soluble in ammonia and absorbs readily free oxygen, it is proposed to substitute it for the expensive pyrogallic acid now used in laboratories for the absorption of oxygen. Pyrogallic acid must be very carefully preserved, an account of the great avidity of this substance for oxygen, whilst cuprous oxide may be easily conserved in a dry state and when necessary dissolved in ammonia. A solution of cuprous oxide in ammonia absorbing oxygen gas turns blue, owing to the formation of cupric oxide (CuO). The solution of cupric oxide obtained may be again converted into a colourless solution of cuprous oxide (Cu_2O) by placing in the liquor a clean copper wire. The formula $CuO + Cu = Cu_2O$ explains this reaction.

ANALYSIS OF "TELL-TALE SUGAR LIQUOR"

FROM THE

SAFES OF TWO VACUUM SUGAR PANS.

By G. C. STEWART, F.C.S.,

Chemist at the Cappelow Sugar Refinery, near Greenock.

ALL vacuum sugar pans (exceptional instances overlooked) are furnished with "tell-tale sugar liquor" safes for catching any "sugar liquor," &c., which might accidentally or peradventure "otherwise" boil over during the evaporating process in sugar refining.

These safes yield, when emptied, solutions which may vary in chemical composition according to a great variety of circumstances. First of all, the mechanical construction of the pans may have a great deal to do with this, as has also the position in which the safe itself is fixed. If the pan is low set and very short in the swan's neck, ten to one but that the "liquor" drawn from the safe of such a pan will be found upon analysis to be much richer in "sugar" and other organic matter than the "liquor" drawn from another pan high set and very lofty in the swan's neck.

Such is the case, and occasionally in sugar-boiling, when too much "salt" water is given to the condenser during the evaporating process, it not unfrequently happens that this excess of salt water finds exit by "more roads than down the Torricellian tube."

When such an accident occurs, the "liquor" drawn from the safe will be found upon analysis to be almost "salt water," and will actually taste salt.

By keeping up a continual examination of the "liquors" drawn from these safes day after day, a good idea will be formed by the chemist in charge of the sugar refinery as to how the pans are being handled by the pan-men.

The following two analyses of this "liquor" will be examined with curiosity by your numerous readers who take a direct interest in the literature of the subject.

100 parts by weight contain:—

	No. 1. Per cent.	No. 2. Per cent.
Crystallisable sugar	4.80	11.43
Fruit sugar	3.37	4.52
Extractive organic matter	1.76	1.54
Insoluble matter	0.56	0.08
Soluble salts	2.57	0.90
Iron	0.26	0.45
Copper	0.21	0.05
Water	86.47	81.03
	100.00	100.00

No. 1.—This "sugar-liquor" is from the safe of a modern vacuum sugar-pan recently constructed, high in the swan's neck, and of artistic mechanical design.

No. 2.—Is the same "liquor" drawn from the safe of an old-fashioned vacuum sugar pan, low set, short in the swan's neck, and as old as Howard.

NOTICES OF BOOKS.

Third Annual Report of the Board of Health of the City of Boston. 1875. Boston: Rockwell and Churchill.

THIS issue contains no small amount of important and interesting matter. There is a paper on the ventilation of schools by Dr. F. W. Draper, which takes up a subject hitherto neglected by sanitary reformers. Prof. W. R. Nichols took samples of air in 111 school-rooms and submitted them to analysis. The proportion of carbonic acid ranged from 3 volumes in 1000 to 0.57, with a general average of 1.18. The greatest amount compatible with a healthy condition of the atmosphere is fixed by Professor Pettenkofer at 0.7; the carbonic acid being regarded not so much as a dangerous body *per se*, but as the measure of contamination in an occupied room as arising from organic products thrown off from the lungs and skin. It need scarcely be said that an apartment where the carbonic acid rises to 3 parts in 1000 is a most unfit place for children to spend some five or six hours daily. The means employed, often the only means provided, to remedy this state of things is merely an additional evil. The windows are thrown open, and, as Dr. Draper puts it, "the inevitable wave of cold outside air sweeps over the uncovered heads of the children and a fresh accession of cases of bronchitis, or of more serious pulmonary affections, is the result." In the inspection of the "Chapman school, a room showed at the desk-level a temperature of 77°; three quarters-of-an-hour later the same room was revisited, when the thermometer indicated 61.7, a fall of 15.3. Between the two visits the teacher had 'aired' the room to some purpose; the air was pure enough, and the coughing and sneezing of the children gave warning that it was cold enough also. If such a sudden change should occur in the outside atmosphere it would be considered a fruitful cause of increased sickness in the community." We greatly fear that were similar investigations undertaken in England the results would be found not more satisfactory than those given in the report before us. It is at least certain that too many school-houses, instead of broad windows admitting those floods of light for which children instinctively crave, are provided with narrow, mediæval loop-holes, calculated to admit only a dim mysterious light, symbolical of anything but intelligence, and eminently calculated to make the pupils uneasy and fretful.

From the sanitary condition of schools we pass to a paper on the sewage question by Dr. W. L. Richardson, who appears to have swallowed the *ex parte* statements of certain English irrigationists without the time-honoured grain of salt recommended on such occasions. Thus he tells us that the existence of sewage farms, so far from being detrimental to the health of the vicinity, has actually a beneficial effect. As proof of this statement—and incidentally we must add, as a confirmation of the remark that statistics can prove anything—he quotes from Dr. Corfield the case of Norwood, where, thanks to the establishment of a sewage farm, the mortality has fallen from 18 to 12.07 per 1000. *Post hoc, ergo propter hoc?* "This improved death-rate is of course to be attributed to the increased vegetation produced by sewage irrigation." If luxuriant vegetation necessarily implies a low death-rate, what do we say to the Terai, the west coast of Africa, the tierras calientes of Mexico, to rice-fields and mangrove swamps? Admitting, for argument's sake, the Norwood case, we cannot overlook the facts detailed in the official report on Progress of India, for 1871 and 1872, by C. R. Markham, C.B. Here it appears that the epidemic of 1847 was more severe and more general in the irrigated districts than elsewhere. A medical committee of inspection recommended that irrigation should not be brought within 200 yards of the villages, and that the latter should be screened by a double row of trees, planted around the irrigated lands. If such are the effects of irrigation with clean water, applied, moreover, only when

needed by the crops, what must be the results of irrigation with sewage, carried on day by day? It must be clearly understood that if we pour any volatile liquid even upon the most perfect filter, evaporation takes place from the surface. If we keep the surface constantly moist, the evaporation is continuous, and if the liquid hold in solution any sewage gases these, too, must as by physical necessity be delivered into the atmosphere. Between a sewage farm and a polluted river there is simply this difference, that the former exposes a far broader evaporating surface. The vicinity of a sewage farm may here and there, for a few years, appear healthy. But just the same may be said of certain places where the utmost sanitary neglect prevails. We could point out villages where the only supply of water is either from shallow wells, separated from cesspools by only a few yards of chalk and gravel, or from roadside duck-ponds, receiving the drainage of cultivated lands and the ooziings of farmyard dung heaps. Yet the death-rate is low, and the inhabitants healthy. Are we not from time to time informed of the health and vigour of nightmen, scavengers, knackers, bone-boilers, and others who are constantly engaged among putrescent matter? Are their no hale old gentlemen who have all their lives quaffed the waters of some City pump, sparkling with carbonic acid due to decomposing animal substances, and whom no physician nor chemist can convince of its unhealthiness? Such cases as that of Norwood either prove nothing or a great deal too much. If a sewage farm is healthy, sanitary reform is a needless luxury.

As to the "astounding results"—which are slow in taking the form of hard cash,—the "enormous crops," and the "most luxuriant vegetation," the eyes of the public are gradually being opened. At first we were told that sewage irrigation would render the application of manure superfluous; then it appeared that the dung of the stock kept upon the farm must be added; and now we find that the green crops are to be ploughed in to enrich the soil! Dr. Richardson exaggerates when he says that precipitation processes allow *all* the soluble matters in the sewage to escape with the effluent. He forgets, too, that of the combined nitrogen present in the sewage, a large part, varying from a quarter to two-thirds, goes off in the drainage from the irrigation farms. So that precipitation and irrigation alike fail to secure all the valuable matter contained in sewage. That in India and other hot, dry countries irrigation is often the one thing wanted to convert a desert into a garden, we fully admit. But in England, in average seasons, it is quite as clearly the one thing which is not wanted.

The report further contains the yearly results of the chemical examination of articles of diet, medicines, &c., as executed by the able chemist to the Board, Dr. W. R. Nichols. Special notice is given of a class of quack medicines largely sold under the name of bitters, but containing large proportions of alcohol, in one case as much as 59 per cent.

CORRESPONDENCE.

SAMPLING OF MILK FOR ANALYSIS.

To the Editor of the Chemical News.

SIR,—Since the Sale of Food and Drugs Act came into operation, I have observed on several occasions that the mode of taking and dividing the samples of milk supplied to the analyst has an important influence on the comparative results of analyses made for the determination of the fat. I am sometimes called upon to analyse not only the sample left with me by the inspector, but also that left with the dealer, who is naturally anxious to know the result quickly instead of remaining in suspense, it may be, for weeks; and I have not unfrequently found,

under such circumstances, that the two analyses have differed in regard to fat to the extent of five or ten hundredths of a per cent. This difference I have traced to the method adopted by the inspectors in dividing the samples. The milk, when purchased, is put into a jug, and from this three bottles are filled. Many minutes often elapse between the making of the purchase and the subsequent dividing and sealing of the samples, during which time a partial separation of cream takes place, to an extent depending on the condition of the milk at the time, and, as a consequence, the bottle first filled will contain a milk somewhat richer in butter than the others. This difference would not have occurred in the divided samples obtained as they used to be under the old Act, for each sample was then taken in a bottle to the analyst, and by him divided after having been thoroughly mixed by shaking, which can be done in a bottle but not in a jug.

I have also found a want of accordance in the results of milk analyses arising from another and very different cause. I have been anxious to satisfy myself as to the usual amount of reduction in the proportion of fat which takes place in large vessels from which milk is being dipped out for sale both by retail and wholesale; and having had several experiments made in which milk has been dipped in successive quantities out of the tin churns containing about 16 gallons, and also out of the counterpanes or tureens containing about 10 gallons each, I have found that when such quantities of milk have, by successive dipplings, been divided into two parts, one consisting of what has been dipped out, and the other of what remained, on taking a sample from each as a milkman would take it, and estimating the fat, the mean of the two extremes has not coincided with the proportion of fat contained in the original milk. In fact, it is impossible, in dealing with large quantities of milk, as the milkmen do, and using the same vessels, to get concordant results of that description. I have always found the mean of the extremes to be below the quantity present in the original sample, if in taking the sample the measure or dipper be plunged in vertically to the depth of an inch or two below the surface, for in that case the richer stratum of milk and cream on the surface is broken and dispersed and the measure filled from the poorer portion beneath.

A milkman can easily supply a sample of milk from a counter-pan, that shall be either richer or poorer in fat than a fair average sample of the whole would be by varying the mode of taking it, but it would be impossible, in the usual mode of serving milk, to ensure that the sample shall represent the mean of the whole.

My object in communicating this note is to show that samples of milk taken in the usual way from the large vessels in which it is kept and conveyed for sale, hardly can, and generally does not, fairly represent an average of the bulk from which they are taken in regard to the proportion of fat present.—I am, &c.,

T. REDWOOD.

17, Bloomsbury-square,
December 28, 1875.

ESTIMATION OF MINUTE TRACES OF COPPER.

To the Editor of the Chemical News.

SIR,—Will you allow me to draw attention to the fact that a process for the estimation of minute traces of copper, after precipitation of iron with ammonia, exactly similar to that given by Mr. Carnelley in your issue of this week (vol. xxxii., p. 308), was proposed by me in the CHEMICAL NEWS, vol. xxxii., p. 3, in an article on the analysis of minium, of which an abstract appears in the *Journal of the Chemical Society* for December last. It is fair to own that the principle was suggested by Mr. Carnelley's process for the estimation of iron, and I frankly confess that, with the splendid opportunities of experiment and investigation offered by the laboratory of Owens College, he has treated the subject with a thoroughness and precision which were unattainable by me, owing

partly to want of leisure, and partly to the fact that the method was improvised under pressure of the sudden necessity for giving a complete analysis of a series of remarkably pure samples of red lead. He will find, however, on referring to my paper, that I have drawn attention to the disturbing influence of ammoniacal salts, which I have proposed to neutralise by the preparation of a comparison liquid from one-half of the solution to be examined.

It would appear that the usefulness of colorimetry, and also of judgment by turbidity, which may be provisionally termed "nephelometry," might be widely extended by the elaboration of a thoroughly reliable process for determining the exact equivalence of columns of liquid of different lengths by the correspondence of their colour or appearance as viewed from the ends; such devices as the preparation of comparison liquids, or the intentional addition of foreign salts, might then be entirely dispensed with as follows:—

Let two equal columns of the solution to be tested be taken, and let their exact measure be known; then let a different proportion of a standard solution of the ingredient to be determined be added to each—it will probably be convenient to add twice as much to one as to the other—and let the columns be made equivalent by shortening one of them. It will be evident that if the original solution was quite free from the substance sought, the comparative lengths of the columns will correspond with the proportions of standard solution added to each, or in the case supposed will be represented by the fraction $\frac{n}{2n} = \frac{1}{2}$; but, if the original solution did contain the substance to be determined, the value of the fraction will be altered, and the quantity present in the longer column, the bulk of which is known, may be determined by a simple equation.

Let $\frac{a}{b}$ be the fraction representing the comparative lengths of the columns, then the quantity of the substance in the longer one may be determined by the equation—

$$\frac{a(x + 2n)}{b} = x + n$$

where n , of course, represents the known quantity of ingredient added to the longer column by means of the standard solution.

The application of this principle may be rendered clearer by an example.

The difficulty of obtaining distilled water absolutely free from ammonia for use in Wanklyn's process of water analysis is well known, and it would be obviously very convenient to determine once for all the proportion of ammonia in a given sample; we might proceed thus: two columns measuring four ounces each are taken; to one of them is added standard ammonia solution equal to 0.0005 grain, to the other equal to 0.001 grain ammonia, the column of stronger tint is shortened, after Nesslerising, until the colour, viewed from above or below, by means of an appropriate apparatus, exactly corresponds; if the distilled water was pure one column will be exactly one-half the length of the other, if ammonia was present the fraction will be greater. Suppose it is $\frac{9}{14}$, then—

$$\frac{9(x + 0.001)}{14} = x + 0.0005$$

which will give $x = 0.0004$ grain.

The "appropriate apparatus" mentioned above is, so far as I am aware, still a thing of the future.—I am, &c.,

THOS. P. BLUNT, M.A. Oxon., F.C.S.

Shrewsbury, January 1, 1876.

COPPER IN BREAD.

To the Editor of the Chemical News.

SIR,—Dr. Edmunds in his contribution to the CHEMICAL NEWS of December 31 (vol. xxxii., p. 311), throws doubt

on the fact that copper has ever been used as an adulterant for bread, and suggests that the blue colour developed on bringing bread ashes to a red heat may have given rise to the idea. I have myself found copper some years since in Belgian bread by the wet process (though never in English), but believe that that adulteration no longer is continued at the present day. I cannot imagine how Dr. Edmunds can suppose that a *chemist* could mistake a *blue* colouration at a *bright red heat* to be due to the presence of copper.—I am, &c.,

ROWLAND J. ATCHERLEY.

January 1, 1876.

SODA TESTS.

To the Editor of the Chemical News.

SIR,—Amongst the points in connection with the commercial analysis of soda ash, which have not been touched upon by your correspondents, there is one of some importance, namely, that of filtering the solutions before testing.

It is evident that any matter, insoluble in boiling water, which neutralises test acid, causes the ash to appear by so much of more value than it really is to the consumer.

The following cases, not selected, but being simply the last three parcels of ash we have used, show what this may probably amount to in good ashes. The alkali in the well washed residue is reckoned at its equivalent of Na_2O ($\text{Na} = 23$).

	Na_2O in solution.	Na_2O in residue.	Total.	Teschemacher's certificate.
I.	53.82	0.40	54.22	54.40
II.	53.57	0.43	54.00	54.20
III.	50.18	0.93	51.11	51.20

The slight difference between Messrs. Teschemacher and Smith's analyses and my own is no doubt very easy of explanation.

Now as the ash is charged at so much the unit according to the percentage of soda, and in III. we have 0.93 useless, out of 51.11 there is a loss to the consumer of over $1\frac{3}{4}$ per cent. of the money value; or, if we compare the make of I. and II. with III., as there is a difference of 0.5 out of about 50, there is a loss of 1 per cent. on buying III. as compared with the others.—I am, &c.,

SAMUEL HALL.

East London Soap Works, Bow.
December 29, 1875.

SODA TESTS.

To the Editor of the Chemical News.

SIR,—It must strike any disinterested person who has perused your correspondence on the subject of commercial alkali testing, that the reputation of the analysts referred to is somewhat under a cloud, and that it is incumbent upon them as they value their commercial integrity to offer some reason for the discrepancies which have been laid to their charge.

It is no secret that for many years the "Liverpool test" has been regarded by all but those more immediately benefitted by it, as thoroughly anomalous; and now that the subject has been ventilated in your columns, if no explanation be forthcoming, there would seem to be no alternative but to suppose that it does not pay to expose its fallacies.

The tests must either be right or wrong. If the upholders of it believe the former, why do they not furnish grounds for their belief? If the latter, why do they not explain that it is simply a case of "trade custom," with which they do not consider it their business to interfere?

Some time ago I acted as manager of a Lancashire works, and had occasion to make many tests of soda-ash and caustic soda. And I need not add my results seldom or never corresponded with the Liverpool test of the same

samples. The analysts with whom I had to deal professed, I believe, to standardise their test acid on the 24 equivalent system; but, even adopting the same equivalent, I had to make it a rule to add 1 per cent to my results. The consequence was that I quickly found it to be quite unnecessary to send out 70 per cent caustic soda higher than 69 per cent, or 55 per cent soda-ash over 54 per cent of my test, feeling sure that it would turn up all right in Liverpool.

It is unpleasant to seem to impugn the commercial honesty of members of a profession which, of all others, appeals directly to the conscience, but neither is it agreeable to know that the possibility exists of purchasing ash by London, Glasgow, or Newcastle test and selling it at a profit by the Liverpool one.

Not merely, however, is the testing objectionable, the sampling is extremely lax. It used to be no uncommon thing to sample parcels of fifty to a hundred casks or drums by merely drawing small portions from two or perhaps three at most. I have in fact known cases where, from the sampler being in a hurry to catch a train, he has been content with sampling a single one.

So unsatisfactory indeed are both the sampling and analyses, that it is usual for manufacturers, when the first sampling and test does not accord with their expectations, to order a second or even a third sampling, till the test comes out of the required strength. I have an instance in my mind of a batch of some fifty casks of ash being sampled three successive times by the Liverpool chemist, and certified successively as 53 per cent, 52 per cent, and 54 per cent alkali; the latter being the strength at which the manufacturer had invoiced the parcel previously to its being despatched from the works. It is my impression that many of the brokers do not discourage this state of affairs, and that in numerous instances, so long as a certificate of some sort is forthcoming to send out to America with the invoice, they do not care whether it represents the true strength or not.

But as many thousands of tons of alkali are annually shipped from Liverpool, and sold entirely on the Liverpool test, how important the matter is will at once appear. And it is therefore devoutly to be hoped that the present state of affairs may speedily have an end.—I am, &c.,

JOHN MORRISON.

Pottery Lane, Forth Banks, Newcastle-on-Tyne.
December 28, 1875.

LIVERPOOL SODA TESTS.

To the Editor of the Chemical News.

SIR,—I have read in the CHEMICAL NEWS the address of Mr. John Pattinson to the Tyne Chemical Society, in which he makes some remarks respecting Liverpool soda tests. I have also read the correspondence on the same subject published in your paper.

As one Liverpool chemist, engaged to a considerable extent in the examination of alkali products, I must emphatically deny that my test differs materially from what Mr. Pattinson terms the "Tyne test," or that it ranges higher than the ordinary commercial test, which is, as Mr. Pattinson states, "based on the assumption of the old chemists that pure carbonate of soda contains $59\frac{1}{2}$ per cent of soda" (the old equivalent of soda being taken as 32 and that of carbonate of soda 54).

Some seven years ago I had a long correspondence with Mr. Pattinson respecting soda tests, when the subject was fully and fairly discussed between us. As a result of this correspondence, and of numerous discussions with other persons familiar with the origin and working of the commercial soda assay, I then arranged a method of working, which I have ever since employed, and which translates the percentage of soda ascertained on the basis of the equivalent more generally accepted by modern chemists (carbonate of soda 53 containing soda 31) into the commercial standard, in such a way as to obtain

figures that are essentially the same as those given by Mr. Pattinson himself in his paper published in the CHEMICAL NEWS of March 5th, 1869.

The practice of reporting the commercial assay in whole degrees, here adopted and thoroughly understood, and which I follow, may sometimes make my report *half a per cent higher* than Mr. Pattinson's, and at other times *half a per cent less*, but this is the very outside difference, and in actual practice it is rarely so wide a difference can occur. This practice, which I am informed is not confined to Liverpool, is, as I view it, the interpretation of results, obtained on chemical data, and by chemical methods to meet a well-known and well-understood trade custom. All soda-ash and caustic soda rules are here based on whole degrees only. Any portion of a degree below a half is not paid for, but above a half is paid for as a whole number.

Whilst working by the method I have alluded to, and testing for some of the oldest and largest firms in the alkali trade, buyers as well as sellers, and my reports sent not only to numerous places in the United Kingdom, but to very many places abroad, I can safely say that for fully six years past I have not received one single intimation that my reports have been *too high*, but I have had occasional complaints that my results were *too low*, and in one or two cases business has been withdrawn from me on the distinct statement that such was the case.

I should be sorry, sir, to make such an apparently egotistical statement did I not feel that as a "Liverpool chemist" I am included in the remarks of Mr. Pattinson, and therefore called upon to defend myself from the imputation, which apparently includes *all* Liverpool chemists who test alkali products of reporting higher figures than are warranted by following the "ordinary commercial standard."

Mr. Pattinson mentions a case in which a sample of caustic soda, tested in Liverpool, was certified by a Liverpool chemist to contain 60 per cent of alkali. The "*identical sample*," he says, tested in his laboratory was found to contain only 57 per cent. He adds that "no notice was taken by the Liverpool chemist of the request to the buyer that the sample should be re-examined." Inasmuch as I make it an invariable practice, although using every precaution to ensure accuracy in my work, to fully consider any objection that may be raised to my analyses and assays, and fully sift all possible sources of error, I am able to say that the case referred to by Mr. Pattinson has not been brought to my notice. I therefore, without hesitation, disclaim any connection with the transaction. But just a word or two in reference to it. It would be interesting to know whether or not it was a sample of "cream" caustic soda, and, if so, whether it was the very same identical bottle sample, or a sample drawn from the identical *bulk*. I can well understand a considerable difference in samples of cream caustic soda drawn from the same bulk, although I will not go so far as to say the difference would extend to three per cent. Different strengths of cream caustic soda may be drawn *from the same drum*. It is an alkali product that should be sampled with extreme care, in order to arrive at the average strength of the bulk.

And this leads me to remark that one of the most constant sources of disagreement in the reports of different chemists lies in the sampling operation. The drawing of samples is a most important matter, requiring the most scrupulous care, whereas it is very frequently conducted in a most careless and incomplete manner, and then differences in tests must result. To compare the results of different chemists, tests of portion of the very same sample must be insisted on.

A further source of difference lies in difference of manipulation, but on this point I would say that having tested alkali products, either in alkali manufactories, or as a professional analyst, for nearly twenty years, I cannot state any reason for the wide differences referred to by Mr.

Pattinson, provided that ordinary care is taken in the manipulation. Some slight differences will occur, even with the most careful analysts, but provided the manipulation does not infringe any chemical principles, or allow of wide discrepancies in consequence of the introduction of inconvenient and complicate details, the testing of soda-ash is, as Mr. Pattinson says, "so simple an operation that there need be no difficulty in two chemists agreeing within a quarter per cent." I would add, *the samples being identical*.

With regard to the letter of Mr. George Warner, F.C.S., I can say that I believe he will not object to my stating that I have several times exchanged samples with him, and that our results have closely agreed.

One word in conclusion. It is undoubtedly inconvenient that the commercial standard differs from the chemical standard now generally accepted by chemists, but manufacturers and buyers based their trade custom on the statements of the older chemists, and it is inconvenient to be continually changing a trade standard. Still, the difference between the chemical and the trade standard being known, it is possible that sales based on the chemical standard may gradually be made until that standard only is adopted. But in this case buyers must expect to pay a corresponding increase in price. I very often make tests based only on the 31 equivalent of soda for sellers and buyers who distinctly understand on what basis they are working, and I believe that such tests are satisfactory to both parties as well as to myself.

Apologising for the length of my communication, I am, &c.,

A. NORMAN TATE.

7 and 9, Hackins Hey, Liverpool.
December 30, 1875.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Seances de l'Academie des Sciences. No. 23, December 6, 1875.

Constitution of the Phosphates.—MM. Berthelot and Louguine.—In this memoir the authors examine the formation of an insoluble phosphate, that of baryta; they undertake an alkalimetric study of phosphoric acid; and, finally, they seek to define the displacements and reciprocal distribution of an alkaline base among phosphoric acid and the nitric, hydrochloric, and acetic acids. They conclude that phosphoric acid is not a tri-basic acid of the same kind as citric acid, as the third equivalent of a soluble base is separated from phosphoric acid by the feeblest actions, and even by dilution. With ammonia it happens that this third basic equivalent does not combine with phosphoric acid, or if it combines at first it does not remain definitely united to the acid, but is gradually separated spontaneously and completely. Neither is phosphoric acid a bibasic acid in the same sense as are the sulphuric, oxalic, or tartaric acids. The second base, as alkalimetric operations show, is not neutralised by phosphoric acid, and is entirely separated by the hydrochloric and nitric acids, and gives indications of division even with acetic acid. In short, the three equivalents of base united in the phosphates considered as normal are combined in different and unequal manners. Phosphoric acid must be regarded as a monobasic acid of a mixed function.

Colouring Matter of the Fruits of Mahonia, and the Characters of the Wine which may be obtained from them by Fermentation.—I. Pierre.—Upon cotton the juice of this fruit gives fugitive red stains, which with ammonia give a variety of unstable greens. The wine

obtained from the berries is admitted to have a disagreeable flavour, but the author suggests that it might be used for giving a facitious colour to inferior wines.

Note on a Process of Magnetisation called Double Touch.—M. J. M. Gaugain.—Not adapted for abstraction.

Temperature of Elevated Regions of the Atmosphere.—M. D. Mendeleef.—It appears that in the upper strata of the atmosphere there exists a source of heat, for the observed heat is always higher than the calculated temperature. This source of heat is doubtless found in the aqueous vapours of the atmosphere.

Transparence of Flames and of the Atmosphere, and on the Visibility of Scintillating Fires.—M. E. Allard.—The nature of this paper appears sufficiently from the title.

Distribution of Magnetism in Circular or Elliptical Discs of Steel.—M. E. Duter.—The author's results are that the sum total of magnetism diffused over circles or ellipses is proportional to the surface. This magnetism may be considered as distributed on threads which affect a hyperbolic form. The non-transverse axes of these hyperboles are the axes of symmetry of magnets perpendicular to the neutral line.

Certain Properties of Gallium.—M. Lecoq de Boisbaudran.—Already inserted in full.

Hydrated Derivative of Cellulose.—M. Aimé Girard.—The compound examined by the author consists of—

Carbon	42.1
Hydrogen	6.4
Oxygen	51.5

The equivalent of water fixed in this substance resists desiccation. The substance, for which the author proposes the name hydro-cellulose, is characterised by its extreme friability.

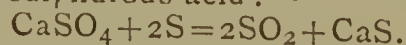
Constitution of the Albumenoids.—M. P. Schützenberger.—The author finds that the differences of constitution of the proteic matters are of the second order, and that they all contain a common nucleus, around which are grouped variable proportions of less important groups, such as urea, oxamid, tyrosin, and the glutamic acid of gluten.

—
Bulletin de la Societe Chimique de Paris,
No. 11, December 5, 1875.

Reply to the Objections of M. A. Gautier, relative to the Part played by Carbonic Acid in the Spontaneous Coagulation of Blood.—M. E. Mathieu and V. Urbain.—The authors maintain that carbonic acid is the cause of the spontaneous coagulation of blood, and during life the fibrin dissolved in the plasma does not coagulate, because the acid gas, in the same manner as oxygen, is combined with the red globules.

Constitution of Benzin.—MM. A. Rilliet and E. Ador.—A hypothetical paper, not adapted for abstraction.

Extraction of Sulphur.—C. F. Sestini.—Having observed, in the Romagna, a workman carefully picking out crystals of gypsum mixed with sulphur ore, before its introduction into the *doppione* (a furnace receiving a double row of retorts), he was told that in the furnace gypsum destroyed the sulphur. On investigating the reactions ensuing when gypsum and sulphur are heated together, he found that at 130° the gypsum lost all its water and became anhydrous. At higher temperatures, up to 444°, the sulphur reduced the sulphate of lime to a sulphide, and escaped as sulphurous acid:—



Does the Mould developed on Organic Bodies assimilate the Nitrogen of the Atmosphere?—MM. F. Sestini and J. del Torre.—The authors having reviewed the conclusions of Boussingault, Boischode, Selmi, and others, and having repeated their experiments under varied conditions, express the opinion that the slight increase of nitrogen found after the growth of *Penicillium* was due

rather to the absorption of atmospheric ammonia than the direct assimilation of free nitrogen.

The Jaborandi (*Polycarpus pinnatus*).—M. E. Hardy.—The essential oil of the *Polycarpus*, as regards its boiling-point and certain of its characters, resembles the iso-terebenthenes, and is exceedingly analogous to the oil of citron.

Solution of Platinum by Sulphuric Acid in the Process of Concentration.—M. A. Scheurer-Kestner.—Already noticed.

Nitronaphthyl-Sulphurous Acids and their Derivatives.—M. P. T. Cleve.—The author's object was to examine the acid which Laurent obtained by the action of fuming sulphuric acid upon nitro-naphthalin, to compare it with those obtained by the action of nitric acid upon α and β naphthyl-monosulphurous acids, to compare these acids, and to transform them into the corresponding di-oxy-naphthalines and dicarbonated acids.

Bisulphate of Lithia.—M. H. Lescœur.—This salt is obtained by the use of monohydrated sulphuric acid. It forms deliquescent crystals, which when dried consist of LiOSO_3 51.87 per cent, free sulphuric acid 46.85 per cent. Theory requires sulphate of lithia 52.88 per cent, to 47.12 per cent of sulphuric acid. Its melting-point is about 120°.

Biacetate of Thallium.—M. H. Lescœur.—This salt is obtained by exposing a solution of acetate of thallium in monohydrated acetic acid to spontaneous evaporation. Its formula is $\text{TlO}, \text{C}_4\text{H}_3\text{O}_3, \text{C}_4\text{H}_4\text{O}_4$. It contains 64.82 per cent of thallic oxide, and 18.84 per cent of free acid. The salt effloresces slightly in dry air, and melts at about 64°.

Biacetate of Lithia.—M. H. Lescœur.—The solution of neutral acetate of lithia in glacial acetic acid deposits, on spontaneous evaporation, crystals in the form of a hopper. Its formula is $\text{LiO}, \text{C}_4\text{H}_3\text{O}_3, \text{C}_4\text{H}_4\text{O}_4$, and it contains 11.54 per cent of lithia, and 49.05 of acetic acid. It is deliquescent, and melts at 99°. If heated suddenly in a small platinum capsule it gives off vapours of monohydrated acetic acid, which take fire and burn.

Part played by Carbonic Acid in the Phenomenon of Coagulation.—Dr. F. Glenard.—The author infers, from the experiment described, that carbonic acid plays no part in the coagulation of blood.

Transformation of Starch by the Action of Diastase, and the Production of a New Saccharine Matter.—M. A. Petit.—If at a constant temperature of 50° a gramme of diastase is allowed to act for some hours upon a kilo. of starch paste at 1-10, the liquid, after filtration and prolonged ebullition,—in order to stop the action of the diastase,—contains, besides maltose capable of fermentation and of reducing Fehling's liquid, another fermentable sugar, without action upon Fehling's test, even after ebullition for five minutes in contact with sulphuric acid at 1-100.

New Alcoholometric Formula.—M. C. Delavaud.—Not adapted for abstraction.

Correspondence from St. Petersburg Nov. 10th, Session of the Chemical Society Sept. 11 to 13.—The minutes of the Session contain a very interesting paper by M. N. Zalomanoff, on the absorptive power of soils. M. Beilstein announces that in the action of a mixture of hydrochloric acid and chlorate of potassa upon benzoic acid there is formed, besides, β -dichlorobenzoic acid and the ordinary α -dichlorobenzoic acid. M. Menschoutkine, on behalf of M. Lissenko, handed in a note on the memoir of M. Jatzoukowitz on the action of oxygen on coals and paraffins. M. Menschoutkine also announced, on behalf of MM. Tscheque and Steiner, that the action of monochloroacetic ether upon xanthogenate of potash gives rise to xanthogenacetic ether.

The same issue contains a memoir, by M. Beilstein, on the dichlorobenzoic acids.

THE CHEMICAL NEWS.

VOL. XXXII. No. 842.

ON THE ESTIMATION OF VERY SMALL QUANTITIES OF LEAD AND COPPER.*

By M. M. PATTISON MUIR, F.R.S.E.,
Assistant Lecturer on Chemistry, Owens College.

As I have lately been occupied with experiments upon the action of saline solutions upon lead and copper, which involved the measurement of very small quantities of these metals, I thought it might be well to test the accuracy and delicacy of the method employed.

The method itself is in no way new, being that described by Wanklyn in his book on "Water Analysis." The depth of colour produced by the addition of sulphuretted hydrogen water to a known volume of the liquid under examination is compared with the colour produced, by the same means, in an equal volume of water, to which a known amount of lead or copper, in solution, has been added. In comparing the colour of the liquid under examination with the standard liquid, I find it preferable to employ stout glass tubes, holding about 100 c.c., and having a diameter of about 1.5 c.m., rather than white porcelain dishes as recommended by Wanklyn. The contents of the tubes are thoroughly mixed by means of glass tubes on the ends of which bulbs have been blown. (See Thorpe, "On a Method of Estimating Nitric Acid, &c." *Journ. Chem. Soc.* [2], xi., 547.)

Wanklyn recommends the use of standard solutions, 1 c.c. of which is equal to 1 m.grm. of copper or of lead: he employs 70 c.c. of the water to be tested. If, therefore, the colour produced on adding sulphuretted hydrogen water to 70 c.c. of the liquid under examination is found to be equal to that produced by the addition of the same reagent to 70 c.c. of distilled water to which 1 c.c. of the standard has been added, we shall have 1 grain per gallon of lead in the water. But 1-10th of a grain of lead per gallon is generally considered hurtful when present in a drinking water; to estimate this we should require to use only 0.1 c.c. of the standard: a very small error in reading the burette measurements would introduce a comparatively large error in the result. Thus, in the case of a water containing 1-10th grain of lead per gallon, an error in reading of 0.05 c.c. would introduce an error in the quantity of lead equal to one-half of the total quantity to be estimated. The first point, therefore, to investigate appeared to be *the strength of the standard solutions*. I shall describe the experiments made with copper.

Standard used, 1 c.c. = 1 m.grm. copper.

Expt.	Taken.	Found.
No. 1 ..	5.0 m.grms. per litre	4 m.grms. per litre.
" 2 ..	2.5 " "	2 " "

Standard used, 1 c.c. = 0.1 m.grm. copper.

Expt.	Taken.	Found.
No. 3 ..	5.0 m.grms. per litre	4.8 m.grms. per litre.
" 4 ..	2.5 " "	2.4 " "
" 5 ..	0.5 " "	0.5 " "

In each case 50 c.c. of liquid was used.

Similar results were obtained with lead solutions.

The use of a standard, 1 c.c. of which is equal to 0.1 m.grm. of copper or of lead enables more accurate and

more delicate results to be obtained than the use of a stronger standard does.

The second point to be determined was *the limits of accuracy of the method*, and first as to the lower limit.

From the experiments with copper already detailed it will be seen that 0.5 m.grm. of copper per litre could be estimated by using 50 c.c. of the liquid.

Standard used, 1 c.c. = 0.1 m.grm. copper.

Solution contained 0.25 m.grm. of copper per litre? Details as before.

Added 0.3 c.c. standard.	Colour too dark.
" 0.2 c.c. "	" rather too dark.
" 0.1 c.c. "	" equal to the other.

Expt.	Taken.	Found.
No. 6 ..	0.25 m.grm. per litre	0.20 m.grm. per litre.

In this experiment it was very difficult to determine the exact point at which the colours were the same, as the intensity of colouration produced was very slight. A further addition of 0.05 c.c. of the standard could hardly be said to produce a noticeable change in the depth of colour.

I think, therefore, that 0.5 m.grm. of copper per litre = 0.035 grain per gallon, is the smallest quantity which can be accurately estimated by this process when working with 50 c.c. of the liquid under examination.

The amount of lead which can be estimated with accuracy is less minute than the amount of copper.

Standard used, 1 c.c. = 0.1 m.grm. lead.

Expt.	Taken.	Found.
No. 7 ..	0.25 m.grm. per litre	No colouration.
" 8 ..	0.50 " "	0.4 m.grm. per litre.
" 9 ..	0.75 " "	0.6 " "
" 10 ..	1.00 " "	1.0 " "

1 m.grm. of lead per litre = 0.07 grain per gallon, is, therefore, the smallest quantity which can be accurately estimated by this process when working with 50 c.c. of the liquid under examination.

By the evaporation of 1 litre of water to 50 c.c., a quantity of copper so small as 0.025 m.grm. per litre, or of lead equal to 0.05 m.grm. per litre, can be estimated by this process. In other words, the process will estimate 1 part of copper in 2,000,000 parts of water, or 1 part of lead in 1,000,000 parts of water.

Secondly, as to the upper limit.

Standard used, 1 c.c. = 0.1 m.grm. copper.

Expt.	Taken.	Found.
No. 11 ..	20 m.grms. per litre	20.4 m.grms. per litre.
" 12 ..	10 " "	10.0 " "
" 13 ..	25 " "	24 to 28 " "
" 14 ..	30 " "	28 to 32 " "

20 m.grms. of copper per litre = 1.4 grains per gallon is the largest quantity which can be estimated by this method when working with 50 c.c. of the liquid under examination.

With lead the following results were obtained:—

Expt.	Taken.	Found.
No. 15 ..	10 m.grms. per litre	10 m.grms. per litre.
" 16 ..	12 " "	12 to 15 m.grms. p. lit.
" 17 ..	15 " "	Colour too dark to allow of estimation.

10 m.grms. of lead = 0.7 grain per gallon is therefore the largest quantity which can be estimated by this method when working with 50 c.c. of liquid.

In making these determinations I found that the colours of the liquids might be compared *immediately* after the addition of sulphuretted hydrogen. The colours did not become intensified on standing.

* A Paper read before the Manchester Literary and Philosophical Society.

I also found that it was immaterial whether the standard was added before the sulphuretted hydrogen water or *vice versa*. Thus there is no need, if the colour of the standard be too light, to prepare a fresh standard, as must be done in Nesslerising. It is only necessary to add another measured quantity to the liquid which already contains sulphuretted hydrogen.

The addition of one or two drops of dilute hydrochloric or nitric acid in no way affected the accuracy or delicacy of the estimation of copper. In the case of lead, a drop of hydrochloric acid caused a faint turbidity (especially in estimating large quantities of the metal), which interfered materially with the results. If an acid must be added, acetic acid is, I think, the best.

When working with 50 c.c. of liquid, so small a quantity as 0.5 m.grm. of copper, or 1 m.grm. of lead per litre, may be estimated by this process. If it is required to estimate smaller quantities than these, the liquid must be concentrated by evaporation. If the amount of copper exceed 20 m.grms., or of lead exceed 10 m.grms. per litre, a smaller quantity of the liquid than 50 c.c. must be used.

ON SOME RECENT DISCOVERIES OF FIELDS OF IRON ORE IN THE SOUTH OF RUSSIA.

By SERGIUS KERN, St. Petersburg.

THESE new fields of beautiful iron ore are situated partly in the Verchni-Dnieprovsky district of the Ekaterinoslaw Government, partly in the Elizavetgradsky district of the Cherson Government; iron ore is found here on the rivers Saksagane and Ingouletz, near the village Krivoy-Rog. About twelve miles from this place on the river Saksagane, near the village Tchervonnaia-Balka, large quantities of red hematite are found. Immense layers of hematite, 100 feet thick, are situated near the river Ingouletz and the village Doubovaia-Balka. The best layers of iron ore are the following:—

1. On the left side of Ingouletz and on the right of Saksagane between the villages Krivoy-Rog and Doubovaia-Balka is a layer of ironstone 120 feet thick; the results of the analyses are:—

	Per cent.
Oxide of iron	68.40
Manganese	0.50
Silica	22.10
Alumina	3.00
Lost from ignition	6.00
	100.00

2. On the left side of the river Saksagane a layer of red hematite 42 feet thick; gives 62.5 per cent of oxide of iron.

3. On the same river Saksagane exist layers of magnetic iron ore and of brown hematite; a sample of the latter analysed gave the following average composition:—

	Per cent.
Oxide of iron	73.40
Manganese	1.30
Silica	18.03
Alumina	2.25
Lost from ignition	5.02
	100.00

It is estimated that the new fields of iron ores contain altogether 90,000,000 tons of ore. There is a railway going to be constructed to these mines, and some blast-furnaces are to be erected.

REPORT

ON THE DEVELOPMENT OF THE CHEMICAL ARTS DURING THE LAST TEN YEARS.*

By Dr. A. W. HOFMANN.

(Continued from p. 3.)

PERHAPS Mr. E. H. Thiellay has unintentionally become the founder of a manufacture of peroxide of hydrogen which may have worthier applications in the future. Perhaps he may not be the first or the only hair-bleacher, as appears from the following document. What von Schrötter revealed to the public was previously known to the initiated. Thus it appears from a letter of M. Schering, of the Council of Commerce, dated Berlin, July 3, 1874, in which occurs the following passage:—

"The bleaching of hair, feathers, &c., by means of peroxide of hydrogen has been found practicable, the greatest difficulty being the ready decomposability of the material. In England and France it is prepared and sold for this purpose in quantity under the names 'Golden Hair Water' and 'Auricome.' In my establishment it is often enquired after for the same purpose." Mr. J. Williams, of the firm Hopkin and Williams, of London, makes a similar statement in letters of July 20 and 27, 1874. By peculiar precautions, however, he is able to prepare permanent solutions of peroxide of hydrogen containing 10 to 20 volumes of available oxygen (3 to 6 per cent by weight of H_2O_2). The weaker solution, which is said to keep for months without change, is sold at 8s. per kilo., in larger quantities at 6s. The stronger solution is sold at double the price. The amount of oxygen in Thiellay's solution, as determined in the Berlin laboratory, agrees tolerably closely with the weaker of these preparations, and this may possibly be its true origin. As the bottle when the determination was made had been opened four weeks previously; as it was only half full and was merely provided with a common cork, the permanence of the solution may be considered sufficient for most purposes.

Peroxide of hydrogen would not be the first body whose industrial application commenced with trifles and gradually reached an unimagined extension. Nitrate of silver served first the vanity of the world as a hair-dye long before its applications in photography. Schrötter* very rightly expresses the wish that peroxide of hydrogen might be generally accessible at a moderate price. Böttger,† and previously Geiger,‡ recommend its introduction into the pharmacopœia. That for medicinal purposes it is preferable to oxygen, ozone, or ozone-water (!) is manifest. Whilst ozone only bleaches ivory in the strongest sunshine of summer, there is no doubt but that peroxide of hydrogen would answer the same purpose even in the absence of light.

Progress in the Artificial Production of Cold and Ice. By Dr. H. MEIDINGER, Professor in Carlsruhe.

Concentrated cold in the form of ice acquires day by day a higher importance for industrial as well as for domestic purposes. Brewing on the Bavarian system, the preparation of "Lager-beer," which, amongst us in Germany at least, has nearly superseded all other kinds of beer, depends upon the prolonged maintenance of a temperature bordering upon freezing-point. The confectioner has no other practical means of producing a degree of cold from -12° to -18° , as required in the preparation of ice-creams. The physician often employs the cold of

* "Berichte über die Entwicklung der Chemischen Industrie Während des Letzten Jahrzehends."

† Von Schrötter, see above.

‡ Böttger, *Polytech. Notizblatt.*, 1873, 13.

|| Geiger, *Lehrbuch der Pharmacie* Aufl. bearb. v. Liebig, i., 213.

ice both externally and internally as an absolutely indispensable remedy. The butcher and the hotel-keeper can scarcely dispense with this means of preserving meat. In the domestic sphere ice has become formally established, at least in large cities, where it can always be obtained at a cheap rate, and to those who have become accustomed to its use it appears a necessary agent for preserving food and cooling beverages during the warm season. In chemical manufactures ice has also found various applications in the crystallisation of salts, or, to speak in more general terms, in the separation of dissolved substances by means of cold. In proportion to the growing consumption we see increasing quantities of ice stored up every winter. An extensive system of transportation has been arranged for conveying ice from the more northern and colder parts of the earth to regions nearer the equator. North America especially ships ice in astonishing quantities in all directions, even to Central and South America, to the West Indies, and to India. Ice from Norway is sent to England and the German ports on the North Sea. In mild seasons, such as 1862-63 and 1872-73, ice from the glaciers of the Alps was sent down the Rhine in entire trains.

Science has shown, however, how to prepare this important requisite artificially. The first attempts at the manufacture of ice on the large scale took place between 1850-60; but this branch of industry has since been much extended. Even in regions where the winter is, as a rule, cold enough to permit ice to be stored up in quantity, *e.g.*, in Germany, it has often been found remunerative to construct machinery for its artificial preparation, or, in general terms, for the production of cold. Manufacturing establishments of this kind may be seen in various places in full activity, and after the mild winter of 1872-73 the demand for machine-made ice could scarcely be met.

The London Exhibition of 1862 introduced the ether and ammonia ice machines. A third system has since been added, the air ice machine, which has not yet reached perfection, since peculiar difficulties interfere with its practical execution. The theories of these machines have been already explained, so that there is no difference of opinion as to their capabilities and their relative merits. A series of proposals have also been made for the production of cold by other agencies, which have hitherto produced little or no practical result. We will endeavour to describe the development which the question has taken in all its branches down to the present day.

(To be continued.)

THE ROYAL SOCIETY.

AWARD OF MEDALS TO DR. HOFMANN AND MR. CROOKES.

IN presenting the Copley Medal to Dr. Hofmann and a Royal Medal to Mr. Crookes at the Anniversary Meeting of the Royal Society, November 30, 1875, Dr. Hooker, the President, said:—

“The Copley Medal has been awarded to Professor August Wilhelm Hofmann, F.R.S., of Berlin, for his numerous contributions to the science of chemistry, and especially for his Researches on the Derivatives of Ammonia.

“The researches of Dr. A. W. Hofmann, from first to last, are related by a strict logical connection, from which (although in various side-paths he has made truly interesting discoveries) he has never essentially deviated. Indeed these researches may be considered as constituting one great and prolonged research on the organic bases theoretically and experimentally considered. It is not, however, to be imagined that because, to a certain extent, limited in its range, this work is of a special or technical

order. The subject covers a large area, and is calculated to lead the investigator to the consideration of the most important chemical problems.

“The memoirs of Dr. Hofmann in reference to the organic bases fall under several heads:—(1) The researches on Aniline and the Organic Bases contained in Coal-tar. These researches are mainly included in the period between 1843 and 1850. (2) The investigations on the molecular constitution of the organic bases derived by the substitution of the Alcohol radicals in the molecule of Ammonia (1850-51). (3) The Phosphorus Bases and the Diatomic Ammonias (1857-60). (4) The investigations on Rosaniline and the various Colouring-matters derived from Coal-tar (1860-70).

“In the course of the aniline investigations Hofmann made an important contribution to the unitary theory of chemistry. Dumas had shown that the essential chemical properties of acetic acid were not altered by the substitution in the acid-molecule of chlorine for hydrogen; but no organic base had yet been discovered derived from another base by a similar process. Fritzsche, indeed, had made a bromine derivation of aniline, in which three atoms of hydrogen were replaced by bromine; but the substance thus formed was a neutral (not basic) body. It occurred to Hofmann that the substitution had here gone too far, and that for this reason the basic properties of aniline had disappeared. Consequently, by an ingenious process (devised for the experiment), the treatment of chlorisatin by the hydrate of potash, he prepared mono-chlor-aniline—aniline, that is, in which one atom of hydrogen was replaced by chlorine. This body was a base, like aniline itself. Hofmann established its basic character by the preparation of many of its salts (*Liebig's Annalen*, vol. liii., p. 1, 1845).

“At the date when Hofmann's paper on the molecular constitution of the volatile organic bases was presented to the Royal Society (December, 1849), Wurtz had just prepared, by a striking experiment, the primary monamines of the alcohol radicals—that is, a system of bases in which one-third of the hydrogen of the ammonia was replaced by the hydrocarbon, the existence of which (it may be noticed) had been predicted by Liebig as a consequence of his views as to the composition of the organic bases. The experiments of Hofmann were in the same direction as those of Wurtz, but of far more general application. By the action of the iodides of the alcohol radicals upon ammonia, Hofmann replaced successively the three atoms of hydrogen which its molecule contains by these hydrocarbons—a method fraught with important results, both theoretical and practical, with which all chemists are familiar.

“Among other consequences, this method placed the theory of types on a solid experimental foundation, which served as the basis of its future development. A critical test was given by which the constitution of any given “ammonia” could be ascertained, the number of derivatives of the “ammonia” when subjected to the action of iodide of ethyl corresponding to the number of atoms of hydrogen which it still retained unreplaced by hydrocarbon. Hofmann applied this test to aniline, and demonstrated, by the successive formation of ethyl-aniline and diethyl-aniline, that this base belonged to the primary monamine class, containing two atoms of hydrogen unreplaced by hydrocarbon. To trace out, however slightly, the consequences of this method would be a serious task indeed. It is sufficient to say that the views at present held by chemists as to the molecular constitution of water, according to which water is regarded, like ammonia, as a typical molecule, but containing two atoms of hydrogen (instead of, as in the other case, three) replaceable by the alcohol radicals, are based upon experimental evidence for which the very same instrument of research, the iodide of ethyl, and methods in all respects analogous to those by which Hofmann thus established the constitution of aniline were employed.

According to these views triethyl-aniline represented

ammonia in which all possible substitutions of this order had been effected. However, Hofmann applied his test to this substance, and obtained a most remarkable result. A molecule of iodide of ethyl combines with a molecule of this substance, giving rise to the iodide of a molecular group, behaving like potassium or sodium, "a true organic metal in all its bearings." The ground of this assertion is, that this iodide of tetrethyl-ammonium, when treated with freshly precipitated oxide of silver, is decomposed with the formation of an oxide of the "organic metal" possessing the main features of potash. This train of investigation has not as yet been adequately pursued; but the fundamental importance of this experiment, in reference to our knowledge of the elemental bodies, cannot be doubted.

"The first memoir on the phosphorus bases, in which the existence of a class of phosphorus bases analogous to the compound ammonias was experimentally established, was published in conjunction with M. Cahours. In three subsequent memoirs these compounds were further investigated with the determination of the differences by which they were discriminated from their nitrogen analogues. We have here presented to us the first example of a diatomic base formed by the action of bromide of ethylene or triethyl-phosphine, in which reaction (as in the similar case of the diatomic alcohols) Hofmann successfully established the formation of an intermediate monatomic bromide, the reaction taking place by two distinct stages. In the third of these memoirs a new class of compound bases is brought before us, containing both nitrogen and phosphorus. Subsequently, in a series of somewhat less elaborate memoirs, the conception of the typical character of ammonia is greatly expanded by the examination of the derivatives of two and three molecules of ammonia.

"As to the researches in reference to the colouring-matter derived from coal-tar, it is only necessary to observe that his inquiries initiated and rendered possible what is now a vast branch of industry. In reference to rosaniline itself (the key of the system), he established the fact, long unknown, that this substance was not an aniline compound at all, but derived from the combination of aniline with toluidine.

"I may lastly mention, as a fitting conclusion to this series of discoveries, an investigation of very wide interest, which has resulted in the construction of the normal cyanides (α) of the monatomic hydrocarbons, formed by the action of chloroform on the primary monamines, which, together with water, are resolved into formic acid and the base whence they are derived; while their isomers (class B), the nitrites, under similar conditions yield ammonia and their corresponding acids. Hofmann has also established the existence of a new class of cyanates (β) of the same monatomic hydrocarbons which, together with water, are resolved into ammonia and their corresponding alcohols, the original class (α) being resolved under the same circumstances into carbonic acid and the primary monamines, as in the experiment of Wurtz before referred to, the whole investigation standing in intimate connection with Hofmann's previous work.

"To estimate the value of these results it is necessary to go through the vast mass of experimental evidence from which they are deduced, which constitutes a body of complete and exact information in reference to one general subject not easily paralleled in the history of chemistry.

"A Royal Medal has been awarded to Mr. William Crookes, F.R.S., for his various chemical and physical researches, more especially for his discovery of thallium, his investigation of its compounds, and determination of its atomic weight; and for his discovery of the repulsion referable to radiation.

"In the year 1861 Mr. Crookes communicated the discovery of a new element, which he had detected by means of a green line exhibited in the spectrum of certain selenium residues which he was examining for tellurium.

The element had as yet been found in extremely small quantities only; but yet he was able to prove chemically as well as optically that it was a new substance, which, from some of its reactions, he thought most probably belonged to the sulphur group of elements, and to which he gave the name thallium.

"The investigation of this new substance was eagerly followed out; and by inquiries in various directions he succeeded in obtaining it in larger quantities, though his supply was still most inconveniently small, rendering it necessary to work the same material over and over again, instead of carrying on the investigation of two or more of its compounds simultaneously. This investigation, which had led him to perceive that the new body was a metal, which he isolated, was in progress when (in the summer of 1862) he was induced to send in a preliminary notice of his investigations to the Royal Society, instead of waiting, as he had intended, till the research was complete, in consequence of the publication by M. Lamy (who had been fortunate enough to meet with a comparatively fertile source of the new substance) of a paper in which he described several of its compounds and announced its metallic nature. The complete memoir was presented to the Royal Society early in 1863, and is published in the *Philosophical Transactions* for that year. It contains, among other things, a provisional determination of the atomic weight, giving a mean result of 203.96.

"The atomic weight of an element being a constant of fundamental importance, Mr. Crookes spared no pains in an accurate determination of that of thallium. This research, which extended over several years, is described in a highly elaborate memoir which was presented to the Royal Society in 1872, and is published in the *Philosophical Transactions* for the following year. The various weighings which the investigations required were performed and reduced with the most scrupulous regard to minute accuracy, as the utmost care was taken in the preparation of the chemicals. Though only one method was employed, namely that of determining the weight of nitrate of thallium obtained from a known weight of the metal, the care with which the whole process was conducted, and the near agreement of the different experiments, are such as to lead us to regard the final mean 203.642 as most probably very near the truth.

"In the course of the weighings which were executed in this research, Mr. Crookes noticed some minute anomalies depending on temperature which did not seem referable to currents of convection. This led him into a train of experimental research which resulted in the discovery of an action of radiation, which, whatever be its real nature (a point still involved in mystery) and whether the action be direct or indirect, all who have seen or followed the experiments must allow to be most remarkable. An account of this research was published in the *Philosophical Transactions* for 1874.

"When delicate beams of straw and other materials, carrying pith balls or disks at their ends, were mounted so as to turn as freely as possible within glass vessels from which the air could be exhausted, it was found that at atmospheric pressure the approach of a hot body produced a movement usually indicative of an attraction, real or apparent, between the ball and the body, and that of a cold body a reverse movement. In these motions, however, it is evident that currents of convection play a leading part; nor does it appear by any means certain that the actions may not be due to these and other well-known causes. The same thing continues when the air is partially exhausted, until, at a certain high rarefaction varying according to circumstances, the motions cease, or nearly so. But on passing this, a new and unexpected phenomenon is revealed, which is exhibited in perfection in chemical vacua, and in the best vacua produced by the Sprengel pump with the improvements which Mr. Crookes has introduced into it. The suspended body is now repelled with striking energy when a source of radiant heat or light is presented to it, or even, if the radiation be

powerful, when it is held at some distance, or when the sun's rays concentrated by a lens are thrown upon it. The action has more recently been exhibited by Mr. Crookes in an exceedingly striking form by means of a horizontal four-armed fly delicately mounted on a sharp point, and carrying at the ends of the arms pith disks in vertical planes passing through the arms, the disks being blackened on one face, on the same side for all. The motion depends in this case on a differential action on the black and white faces, the black being repelled.

"It is the mystery attending this phenomenon that gives it its great importance. There is evidently some action going on with which we are not at present acquainted; and there is no saying what a thorough investigation into the cause of the phenomenon may lead to."

PROCEEDINGS OF SOCIETIES.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, December 14th, 1875.

EDWARD SCHUNCK, Ph.D., F.R.S. &c., President, in the Chair.

PROF. SCHORLEMMER exhibited a sample of peat from lagoons in the Sierra Madre, in Mexico. It is very dense and not readily inflammable, giving very little flame, but when once red-hot it burns completely, without requiring much draught, to a perfectly white ash containing much calcium carbonate and a little sodium sulphide, which is derived from glauber salt which the peat contains.

"Chemical Notes," by M. M. PATTISON MUIR, F.R.S.E., Assistant Lecturer on Chemistry, Owens College.

I. On the Solubility of Potassium Perchlorate in Water.

Having a small quantity of pure potassium perchlorate at my disposal, I thought it might be interesting to determine the solubility of this salt in water at different temperatures.

The apparatus employed was similar to that described by Hannay (*Journ. Chem. Soc.* [2], xii., 203).

The salt was placed in a small test-tube to which a thermometer was strapped, the whole being surrounded with ice or water maintained at the proper temperature.

The following were the results obtained:—

A. Temperature 0° C.

Weight of liquid in the bulb, 4.722 grms.

Weight of residue on evaporation, 0.0333 grm.

Weight of distilled water contained in the bulb at 0°, 4.7575 grms.

Weight of bulb itself, 5.3954 grms.

Hence the specific gravity of an aqueous solution of this salt saturated at 0° equals 1.0005; the percentage of salt in solution is 0.705; and the solubility of the salt is 1 part in 142.9 parts of water.

B. Temperature 25° C.

Weight of liquid in bulb, 4.7418 grms.

Weight of residue on evaporation, 0.0907 grm.

Other weights as before.

Specific gravity of aqueous solution saturated at 25°, 1.0123.

Percentage of salt in solution, 1.92.

Solubility, 1 part in 52.5 parts of water.

C. Temperature 50° C.

Weight of liquid in bulb, 4.798.

Weight of residue on evaporation, 0.243.

Specific gravity of aqueous solution saturated at 50°, 1.0181.

Percentage of salt in solution, 5.07.

Solubility, 1 part in 15.5 parts of water.

D. Temperature 100° C.

Weight of liquid in bulb, 4.9965.

Weight of residue on evaporation, 0.7870.

Specific gravity of aqueous solution saturated at 100°, 1.06603.

Percentage of salt in solution, 15.76.

Solubility, 1 part in 5.04 of water.

For each rise of 25° the solubility and the percentage of salt in solution increase in round numbers threefold.

I may add that I found Hannay's apparatus exceedingly accurate and serviceable.

II. On Basic Bismuth Perchlorate.

If metallic bismuth be heated with an aqueous solution of perchloric acid, it is slowly converted into a white non-crystalline mass. This substance is insoluble in water: when thoroughly washed and dried between folds of blotting-paper it presents the appearance of a bulky, pure white powder which it is difficult to obtain equally divided as the particles tend to gather together and form small more or less compact masses. This substance yields the following numbers on analysis:—

(a) 0.3640 grm. gave 0.2675 grm. Bi_2O_3 = 0.2382 grm. Bi.
(b) 0.4173 " " 0.2980 " " = 0.2670 " "
(c) 0.4500 " " 0.3231 " " = 0.2900 " "

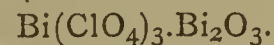
Calculated for $\text{BiO} \cdot \text{ClO}_4$.

Found.

I. II. III.

Bismuth 210 .. 64.52 65.44 63.98 64.44

These numbers agree very well with those required by the formula $\text{BiO} \cdot \text{ClO}_4$, or it may be written—



Basic bismuth perchlorate is almost perfectly insoluble in water even at 100°: it is very readily dissolved by hydro-

Date, 1875.	Weather.	Place and Time.	Temp.	Barom.	Wind.	Vols. of $\frac{1}{2}\text{CO}_2$ per 10,000 of Air.
Aug. 2.	Fine, but cloudy.	In boat, $\frac{1}{2}$ mile from shore; 12 noon.	16.5°	767 m.m.	W. by S.	3.87
" 4.	Clear, cloudless sky. Sunset.	On shore, 8 p.m.	21°	760 "	W. by N.	3.88
" 14.	Fine, fresh breeze.	200 yds. from shore, 3 p.m.	21°	760 "	S.W.	3.34
" 18.	Fine, very clear; very heavy rain during preceding night.	On shore, 8.30 a.m.	16°	759 "	N.W.	3.40
" 21.	Fine, very clear; rain during morning.	In boat, $\frac{1}{4}$ mile from shore; 2.30 p.m.	17.5°	767 "	N.W.	3.84
Sept. 3.	Fine, showers during pre- ceding days.	300 yards from shore.	16°	759 "	N.W.	4.01

Mean = 3.72 vols. CO_2 per 10,000 of air.

chloric or nitric acid; less readily by sulphuric acid; at a red-heat it is decomposed with formation of bismuth chloride which is slowly volatilised.

III. On the Amount of Carbon Dioxide in the Air of Sea-Coast Places.

Thorpe (*Journ. Chem. Soc.* [2], v., 189) has shown that the air over the ocean contains less carbon dioxide than air over the land, the mean numbers being 3.0 and 4.04 vols. per 10,000 of air respectively.

During the long vacation I interested myself with a few experiments upon the air of the sea-coast, with a view to determine whether it inclined, as regards carbon dioxide, to sea air or to land air.

The samples of air were collected at Ardrossan, a small town on the Firth of Clyde, where the river is almost entirely merged in the open sea.

The estimations were conducted in accordance with Pettenkofer's method.

The air of such a place as Ardrossan, although it be situated almost in the open sea, is not therefore influenced by the sea, so far as the carbon dioxide is concerned, but contains almost the same amount of that gas as is found in land air.

CORRESPONDENCE.

"IN ABSENTIA" DEGREES.

To the Editor of the Chemical News.

SIR,—Some attention having been recently directed to the importance of judging men rather by what comes before than by the letters which follow their names, it may possibly interest some of your readers to know that—even in the matter of degrees—whilst some men "achieve greatness," others have "greatness thrust upon them."

About five years ago I wrote to an individual who advertised himself as willing to assist gentlemen in obtaining Ph.D. and other distinctions *in absentia*, asking him to give me some particulars of this mysterious process, and was informed that for a small fee "a suitable Literary petition in Latin would be written and submitted, with my biography, &c., and his best recommendations to the Senatus Academicus of any University I might select." My curiosity having been satisfied, I did not reply to this or any subsequent communication, but since that date Dr. — has been one of my most constant and disinterested correspondents. I have now in my possession twenty-eight letters, circulars, cards, &c. (the last to hand a few mornings since), and many others have been accidentally destroyed. In one of these he takes considerable pains to explain to me, a countryman, the exact situation of his abode, that I may have no difficulty in consulting the oracle. In another he says—"Next week being Whit week I have no doubt you will be in London; please call on me." In another I am informed that, "since he last wrote, the Deans of several Colleges and Universities have sent to several gentlemen their diplomas, and they are much pleased with them, as they are very handsome documents." Having waited long enough for the vision of these "handsome documents" to produce a due effect on my mind, he wrote—"I have the honour to inform you that the Senatus Academicus of the University (*sic*) has instructed me to state that the degree you applied for will be granted you on receipt of the fees." This overwhelming condescension on the part of the "Senatus Academicus" not meeting with a suitably eager and grateful response, he again wrote—"I am afraid, from your silence, that the fees are the obstacle to your promotion, and, as I am very anxious that you should receive your degree at once, I pray you to express to me, in confidence, the arrangement you would desire." No comment on the

above is necessary. We need not be surprised that Doctors differ.—I am, &c.,

F. BADEN BENDER.

7, Exchange Street, Manchester,
January 6, 1876.

A CORRECTION.

To the Editor of the Chemical News.

SIR,—In my "Note on the Chalybeate Water at Sellafield, near Whitehaven" (*CHEMICAL NEWS*, vol. xxxii., p. 309), I regret to find that I have made an error in copying the analysis of the clayey earth (therein alluded to) from my laboratory notes. I will therefore thank you to kindly give publicity to this letter, as a correction.

The tabular analysis of the "clayey earth" should have read as follows, as it appears in my notes taken at the time of analysing:—

Silica	40.24
Alumina	38.62
Ferrous oxide	10.73
Ferric oxide	0.70
Manganous oxide	2.00
Calcium carbonate	5.30
Calcium sulphate	2.05
Loss or undetermined	0.36

100.00

—I am, &c.,

WILLIAM H. WATSON.

Braystones, near Whitehaven,
January 11, 1876.

PEROXIDE OF HYDROGEN AS A HAIR DYE, OR BLEACHER.

To the Editor of the Chemical News.

SIR,—In the admirable "Report on the Development of the Chemical Arts during the last ten years," by Professor Hofmann, he states, in your impression of the 7th inst. (*CHEMICAL NEWS*, vol. xxxiii., p. 3), that the expensive liquid used to impart a golden hue to the hair was, with respect to its composition, a secret until 1874, when Schrötter analysed it and proved it to be a solution of peroxide of hydrogen. In my Report on Public Health (*Dublin Medical Journal*, November, 1869), I stated that "a specimen of colourless fluid, used at the present time for the purpose of giving dark hair the fashionable, *i.e.*, yellow colour, was submitted to us for analysis. Very much to our surprise we found it to consist solely of solution of peroxide of hydrogen, or oxygenated water. Curiously enough, chlorine, instead of bleaching hair, rather darkens its hue." In my "Manual of Public Health," page 249, I give a formula for a yellow dye (H_2O_2) for the hair. My discovery in 1857, that urea is as effective as ammonia in furnishing nitrogen to plants, has not prevented one French and two German chemists from making the same discovery in later years.—I am, &c.,

CHARLES A. CAMERON.

Royal College of Surgeons, Dublin,
January 10, 1876.

PERIODIC LAW SHOWING EXISTENCE OF A SIMPLE RELATION AMONG THE ELEMENTS.

To the Editor of the Chemical News.

SIR,—With reference to the paper by M. Mendeleeff, translated from the *Comptes Rendus* of November 22nd 1875, which appeared in the *CHEMICAL NEWS*, vol. xxxii., p. 293, I have to make the following remarks:—

1st. The periodic law showing the existence of a simple relation among the elements when arranged in the natural order of their atomic weights was first published by me in the CHEMICAL NEWS for August 20th, 1864, (vol. x., p. 94). I again called attention to it in the same journal for August 18th and 25th, 1865, (vol. xii., pp. 83 and 94), and on March 1st, 1866, read a paper on the subject before the Chemical Society. All this was some years before M. Mendeleeff had printed anything upon the question.

2nd. With regard to the application of such a law to indicate the gaps which exist in the system of the known elements, I may mention that in the CHEMICAL NEWS for July 30th, 1864 (vol. x., p. 59), I pointed out that in the group of elements containing carbon, silicium, titanium, and tin, there was an element wanting having an atomic weight of 73; being, in fact, the eka-silicium of M. Mendeleeff.—I am, &c.,

JOHN A. R. NEWLANDS, F.C.S.

Amsterdam, January, 3, 1876.

SODA TESTS.

To the Editor of the Chemical News.

SIR,—The correspondence which has recently appeared in the CHEMICAL NEWS fully confirms the accuracy of my remarks on "Liverpool soda tests" made in my inaugural address to the Newcastle Chemical Society lately reprinted in your Journal. Will you allow me to state, in reply to Mr. A. Norman Tate's candid and courteous letter, of last week, that I have no doubt that there are many chemists in Liverpool who always carefully and fairly test samples of soda-ash and caustic soda submitted to them, and that I am quite satisfied he is one of these. He is not the chemist referred to as certifying a sample of caustic soda contained 60 per cent of water which only contained 57.0 per cent. In further reply to his inquiries, allow me to state that the said sample was a sample of "cream" caustic soda, and that the portion tested by me was from the identical bottle sample tested by the other chemist. But this is not a solitary case. There are hundreds and perhaps thousands of similar cases in which the Liverpool test has been from one to three per cent above the proper amount—cases well known to many persons in the trade, and well known to several analytical chemists, who have long considered this state of things as one of the greatest disgraces to the profession. For obvious reasons it would be unwise to mention names.

I must courteously but emphatically state that I think Mr. Tate is in the wrong in dealing with the fractions of percentages in the way he describes. I have always understood that in the alkali trade no fractions, not even 0.9, are charged or paid for; and this will not be considered very unreasonable or hard upon the seller when it is remembered that the whole commercial test gives the latter the advantage of about 0.7 of a per cent in a 52 per cent alkali. But whether this be the custom or not, I maintain that no chemist ought to give a certificate that a sample contains 53 per cent of soda when he knows that it only contains 52.5 per cent. It appears to me that the proper thing to do is to state that the sample contains 52.5 per cent of soda and leave the buyer and seller to arrange how the fraction has to be dealt with.

With regard to the letter of Mr. Samuel Hall, I have to say that every chemist who tests soda-ash for buyer and seller, filters, or ought to filter, the portion soluble in water from the insoluble portion before the test acid is added. If this be neglected, the carbonate of lime in the insoluble portion will neutralise the test acid and be reckoned as soda, and thus a door for a very serious fraud would be opened. It is the custom in some works, in order to save time and trouble, to test the sample without filtering, and make a deduction for the amount of carbonate of lime which, from experience, the sample is

known to contain. Mr. Hall is, I think, mistaken in assuming that the difference between his tests and Messrs. Teschemacher and Smith's arises from his filtering the samples and these gentlemen not doing so. I happen to know that Messrs. Teschemacher and Smith, as might be expected, always filter their samples of soda-ash before testing. The differences pointed out are just about equal to those which would arise from using test acid based on the equivalent of sodium = 23 and test acid based on the old equivalent of sodium, 24 (the English commercial test).

In conclusion, let me remind those chemists who test soda with acid made on the correct chemical equivalent of soda that in converting their results into the English commercial test 31 parts of soda by the theoretically correct test are equal to 31.41 parts of soda by the commercial test, and not 32 parts as has been sometimes erroneously assumed by chemists. This error was pointed out by me in a letter which appeared in the CHEMICAL NEWS of the 27th of June, 1870.—I am, &c.,

JOHN PATTINSON.

75, The Side, Newcastle-on-Tyne,
January 10, 1876.

SODA TESTS.

To the Editor of the Chemical News.

SIR,—Soap-makers are greatly indebted to you for the space you have given to your correspondents under the above heading. Whether it will end in testing soda-ash chemically, or retaining the present system of merely a *commercial examination*, and also whether the equivalent shall be 32 or 31, remains yet to be seen. I protested in my former letter against our having to pay, as soap-makers, the price of soda for that which is of no value to us; and I can confirm, by my own experience, the statement of Mr. Hall, in your impression of last week (vol. xxxiii., p. 8). Some twelve years since, I elicited from a London commercial analyst, in reply to a question, that the soda-ash sent for certificate was tested with all its impurities in it; therefore, whatever quantity of test acid was used, represented real soda: "*we have no time to go beyond that*," he said.

Again, there is, as I have heard, a difference of opinion among the analysts as to the colour of litmus test-paper at the point of saturation, some insisting on full redness: Faraday said the delicate tint of the inside peel of an onion is the proper one.—I am, &c.,

A SOAP MAKER.

Southwark, January 11, 1876.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Seances de l'Academie des Sciences. No. 23, December 6, 1875.

Distribution of Magnetism in the Interior of Magnets.—MM. Trève and Durassier.—In this paper, which cannot be intelligibly abstracted without the accompanying diagrams, we find the interesting observation that those parts of a magnet where its force is greatest are less readily attacked by acids than its neutral or central parts.

Panification in the United States, and on the Properties of the Hop as a Ferment.—M. Sacc.—The author holds that there exists in the hop a ferment more powerful than that which exists in beer-yeast. It is soluble

in water, and has the property—unique among ferments—of resisting the action of boiling water.

Influence of the Defoliation of the Beet on the Yield of Sugar.—M. B. Corenwinder.—The author finds that defoliation reduces the gross amount of the crop, causes a notable amount of the sugar to disappear, and induces the plant to take up a further amount of saline matter from the soil, thus deteriorating the yield of sugar, both in quantity and quality.

No. 24, December 13, 1875.

Laws of Magnetic Influence.—M. J. Jamin.—Incapable of useful abstraction.

Theory of Refining Glass.—M. E. Fremy.—The difficult portion of the manufacture of glass is the process of refining, the object of which is to render the mass homogeneous, and expel as far as possible the bubbles of gas which are produced in abundance at the moment when the glass is formed. The nature of this gas is not exactly known, but it is evidently due to the action of reducing agents upon the sulphate of soda found in excess in the glass. This excess of sulphate of soda is destroyed by various methods, but chiefly by the use of sticks. At the instant when the sulphate of soda is thus submitted to the action of an organic body, the formation of sulphide is proved by the yellow colouration which the glass assumes, but which disappears afterwards from the action of oxygen. It is curious to point out here a certain analogy between refining glass and refining copper. In the former case the excess of the sulphate of soda, which is the agent of vitrification, is destroyed by wood. In copper refining oxygen is the agent of purification for the metal: but the excess of this gas forms protoxide of copper, which dissolves in the metal, and renders it brittle. The refining of copper, like that of glass, is therefore completed by making use of wood, which decomposes the oxide of copper, and restores to the metal all the useful properties which the oxygen had caused it to lose.

Heat of Solution of Precipitates and other Bodies Sparingly Soluble.—M. Berthelot.—Reserved for insertion in full.

Researches on the Sulphines.—M. A. Cahours.—The author investigates the mutual action of bromide of acetyl with sulphide of methyl; of iodide of acetyl with sulphide of methyl; of bromide of cyanogen and sulphide of methyl; iodide of methyl and sulphocyanide of methyl; and iodide of ethyl with sulphide of ethyl.

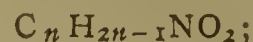
Temperature of Elevated Regions of the Atmosphere.—M. D. Mendeleef.—(Second memoir). The author gives a demonstration of his theory concerning the excess of temperature in the upper strata of the atmosphere.

Destruction of Vegetable Matter mixed with Wool.—MM. J. Barral and Salvétat.—The authors point out that the fragments of vegetable matter mixed with Australian and South American wools have proved a great inconvenience to manufacturers. The chemical process for their removal is based upon the fact that vegetable matter treated with sulphuric acid at 4° to 5° B., and then heated in a stove to 125° to 140°, is disintegrated. This method was patented by M. Frézon. M. Joly, instead of sulphuric acid, uses a solution of the hydrochlorate of alumina, stoving afterwards at a rather higher temperature. M. Chevreul has shown that in this process the hydrochlorate of alumina acts by its specific properties, and not by the liberation of hydrochloric acid.

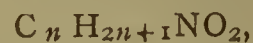
Researches on the Constitution of Fibroin and of Silk.—MM. P. Schützenberger and A. Bourgeois.—The authors arrive at the results that fibroin is distinguished from albumen by the almost entire absence, amongst the products of its hydration of acids of the series—



by a much smaller proportion of amidic acids of the acrylic series—



and by the fact that the amidic acids of the series—



which form its principal mass, are the inferior homologues ($n=2, 3, 4$) of those which prevail in the albumenoids ($n=6, 5, 4$). Silk yields more ammonia, oxalic, carbonic, and acetic acids than fibroin.

Comparative Study of the so-called Instantaneous Electric Efflux, and of the Continuous Current in the case of Unipolar Excitement.—M. A. Chauvet.—This paper requires the accompanying diagrams.

Action of Flames in Presence of Electric Bodies.—M. Douliot.—All the phenomena presented are such as might have been foreseen if for the flame there were substituted a conductor insulated and armed with points in all directions.

Sulphocyanates of Acid Radicals.—M. P. Miquel.—By heating together 78 parts chloride of acetyl and 161 parts sulphocyanide of lead, there are formed chloride of lead and sulphocyanate of acetyl. This compound passes over at 135° without traces of decomposition.

Saccharification of Amylaceous Matters.—M. Bondonneau.—The author holds that each amylaceous molecule, before arriving at the final state of glucose, passes successively through the following stages:—

	Rotatory Power.	Action of I.	Action of Absolute Alcohol.
Amylogen ..	216	blue	insoluble
Dextrin, α ..	186	red	"
Dextrin, β ..	176	colourless	"
Dextrin, γ ..	164	"	soluble
Glucose ..	52	"	"

Influence of Defoliation on the Weight and Saccharine Richness of Beet-Root.—MM. P. Champion and Pellet.—The authors dispute the theory of M. Violette, but they nevertheless admit his practical conclusion that the removal of leaves reduces both the gross weight of the crop and the percentage of sugar.

Justus Liebig's *Annalen der Chemie*,
Band 179, Heft 1 & 2.

On Certain Methyl- and Benzyl-Selenium Compounds.—C. Loring Jackson.—The compounds examined are the monoselenide of methyl $(CH_3)_2Se$, the selenide-nitrate of methyl, the selenide-chloride $(CH_3)_2SeCl_2$, the corresponding bromine compound $(CH_3)_2SeBr_2$, and the iodine $(CH_3)_2SeI_2$, and the methyl-selenide-platinum chloride $[(CH_3)_2Se]_2PtCl_4$. Among the benzyl compounds described are the benzyl monoselenide $(C_6H_5CH_2)_2Se$, the nitrate, chloride, and double-platinum compound $[(C_6H_5CH_2)_2Se]_2PtCl_4$. The author has also examined benzyl-diselenide $(C_6H_5CH_2)_2Se_2$, benzyl-selenious acid $(C_6H_5CH_2)SeOOH$, with its ammonium, silver, sodium, barium, and lead salts, and certain selenines.

Products of the Action of Chlorine and Bromine upon the Aldehyd of the Ethyl Series.—A. Pinner.—The first effect of chlorine upon aldehyd, along with the formation of hydrochloric acid and of chlorine, substitution-products of aldehyd, consists in its polymerisation. Among the resulting bodies is croton-chloral, the properties, decompositions, and products of which are described at some length. The author has made a variety of experiments with the view of elucidating the constitution of croton-chloral, but all with a negative result.

Products of the Action of Hydrocyanic Acid upon Chloral and Croton-Chloral.—A. Pinner and C. Bischoff.—The authors describe, in this paper, chloral cyanhydrate

trichlorolactic acid, trichlorolactic-ethyl ether, croton-chloral cyanhydrate and trichlorangelactic acid.

Contributions to the History of Diethyl-Urea.—Victor von Zotta.—An examination of the action of nitrous acid upon diethyl-urea.

On Isomeric Sulpho-Salicylic Acids.—Ira Remsen.—The author obtains two isomeric sulpho-salicylic acids.

Communications from the Laboratory of the University of Freiburg, in Breisgau.—These consist of a paper on the decompositions which the sulphocyanide of ammonium undergoes at elevated temperatures; a memoir on melamin; contributions to the history of sulphurea; a note on chloro- and brom-sulphurea; a paper on brom-ethyl sulphurea, and one on the thiopruissamic acids.

Constitution of Mesitylen.—A. Ladenburg.—A hypothetical paper, not adapted for useful abstraction.

On Diphenylen Disulphide.—C. Graebe.—This substance, $C_{12}H_8S_2$, crystallises in colourless shining prisms, fuses at 154° to 155° , sublimes in needles and boils at 360° . It is insoluble in water, very sparingly soluble in cold—but more freely in boiling—alcohol. Benzol, ether, and bisulphide of carbon dissolve it readily. In concentrated sulphuric acid it dissolves slowly in the cold, but more quickly if heated, giving an intense violet colour.

On Rosolic Acid.—C. Graebe and H. Caro.—The authors have subjected the behaviour of rosaniline with nitrous acid to a new examination. Their paper will be inserted in full at our earliest convenience.

Communications from the University Laboratory of Prof. A. Fleischer, at Klausenburg.—These consist of a memoir, by Prof. Fleischer, on dithiocyanic acid, and a paper, by the same author, on certain double salts of sulphocyanide of ammonium.

Action of Chlorine upon Pyrogallol.—J. Stenhouse and C. E. Groves.

Crystallographic Properties of Mairogallol.—W. J. Lewis.—These two papers are translated from the English.

Remark on F. Kopfer's Dissertation, "The Action of Mineral Acids upon Chloride of Lime."—W. Walters.—The author controverts Kopfer's statement that chloride of lime, on treatment with minute quantities of acid, yields only monoxide of chlorine.

On Glutaminic Acid.—J. Habermann.—An account of some of the salts of this acid.

Bulletin de la Société d'Encouragement pour l'Industrie Nationale. No. 24, December, 1875.

Report Presented by M. Baude on behalf of the Committee of the Economic Arts on a Process for the Analysis of Cements, and on the Adjunct for Moderating the Rapidity of Setting, by M. Ducourneau.—The process for the analysis of cements herein referred to is not described. It consists in a series of decantations.

Description of the Tubular Electro-Magnet of M. Camacho.—This description of the Camacho electro-magnet is accompanied with engravings.

Les Mondes. Vol. xxxviii., No. 16. December 16, 1875.

Death from the Sting of a Bee.—At Crèvecœur, in September last, a young woman of the age of twenty-four died in a few minutes from the sting of a single bee on the left cheek. The deceased had been strong and healthy, but having on a former occasion been stung by a bee she had remained for several hours apparently dead.

The Goux Earth Closet System.—This system is in successful operation in the town of Amiens and in

its vicinity, and the manure gives very satisfactory results.

M. Reimann's Farber Zeitung, No 47, 1875.

This issue contains an account of the meeting of the Berlin Association of Dyers. Dr. Reimann read a very interesting paper on the manufacture and application of eosine. There are receipts for dyeing an aniline-violet on mixed garments, for a brown, light brown, cinnamon and light olive on wool and woollen yarn, and for a fast black on cotton yarn.

No. 2, 1876.

Wool-Dyeing with Methyl-Green.—The process which has been found satisfactory in a number of establishments consists in boiling the wool for fifteen minutes in a solution of hyposulphite of soda, 3 grms. to 600 grms. water, and when the wool is thoroughly penetrated with the liquid adding 2 grms. sulphuric acid. The bath after taking out the wool must be perfectly clear, and the vessel employed must not consist of copper, lead, or iron. If the hyposulphite of soda is employed too strong the wool does not "handle" well and the green appears somewhat flat. No trace of a metallic compound must be present. The dyeing is then performed in an aqueous solution of methyl-green. If a yellowish tone is required picric acid and acetate of zinc are mixed in the proportions of 600 grms. water, 0.07 gm. picric acid, and 0.06 gm. acetate of zinc. After dyeing yellow in this beck, a little acetate of soda is added, and the dyeing is then completed with 0.2 gm. methyl-green.

MISCELLANEOUS.

Mineralogical Society of Great Britain and Ireland.—It has been arranged to hold the first General Meeting, for election of Officers, adoption of Rules, &c., at the rooms of the Scientific Club, 7, Savile Row, London, on Thursday, February 3rd, at 12 noon. The chair will be taken by Mr. H. C. Sorby, F.R.S., &c.

Death of Mr. S. T. Davenport.—We regret to have to announce the death of Mr. S. T. Davenport, well known as an active and energetic officer of the Society of Arts. Mr. Davenport's connection with the Society had lasted for thirty-three years, and it was in great part to his unceasing and zealous efforts that the present prosperity of the Institution is due. To very many of the members he was well known, and all who take an active interest in the Society of Arts will feel his loss severely. Mr. Davenport's death was very sudden; it took place on Friday last, after an illness of only four days.

Metropolis Gas Supply.—Dr. Letheby, the chief Gas Examiner appointed by the Board of Trade, has recently reported—to the Metropolitan Board of Works and the Corporation of the City—on the illuminating power and chemical quality of the gas supplied to London by the Chartered, the Imperial, and the South Metropolitan Gas Companies. In all there are nine testing-places, where the gas from the several works of these companies is tested every evening, between the hours of 5 and 10 o'clock, and the following are the average results for the quarter:—The illuminating power of the Chartered Gas, at Beckton, has been equal to the light of 16.91 standard sperm candles; at Friendly Place, Mile End, it has been equal to 16.63 candles; and at Ladbroke Grove to 16.93 candles. That of the Imperial Company, at Carlyle Square, Chelsea, has been equal to 16.44 candles; at Camden Street, Camden Town, to 16.28; at Graham Road, Dalston, to 15.70 candles; and at Bruce Terrace, Bow, to 16.22 candles: while that of the South Metropolitan Company, at Hill Street, Peckham, has been equal to 16.25 candles.

In the case of the Cannel gas of the Chartered Company the illuminating power has been equal to 21.02 candles. In all cases this illuminating power is obtained by burning the gas at the rate of 5 cubic feet an hour, from suitable burners, as prescribed by the Referees appointed by the Board of Trade. On four occasions during the quarter the gas of the Chartered Company, at Beckton, was a little below the parliamentary standard. As regards purity, Dr. Letheby reports that sulphuretted hydrogen was, with two exceptions, at all times absent from the gas of all the companies. The two exceptions were in the case of the gas at Beckton, when, from accidental causes, there was a trace of this impurity present. The average proportions of sulphur in the gas have been as follows:—Beckton, 12.67 grains per 100 cubic feet; Friendly Place, 10.59 grains; Ladbroke Grove, 12.99; Millbank, 17.12; Chelsea, 19.38; Camden Town, 17.91; Graham Road, 13.77; Bruce Terrace, 11.45; and Hill Street, Peckham, 21.99 grains. The proportion of this impurity has been in excess of the prescribed amount (25 grains per 100 cubic feet) on four occasions in the Imperial Gas, at Camden Street, and on fourteen occasions in that of the South Metropolitan Gas. Dr. Letheby states that the excess of impurity in the Imperial Company's gas was accidental. The amount of ammonia in the gas of all the companies has been constantly below the prescribed quantity of $2\frac{1}{2}$ grains per 100 cubic feet of gas. In most cases it has been considerably less than 1 grain per 100 cubic feet.

Important Decision versus the Employment of the "Lactometer" used by the Board of Health, of New York City, as a Means of determining Adulteration of Milk.—On September 16th, 1875, a German milk dealer was convicted, in one of the lower Courts, of "the misdemeanour of keeping and exposing for sale adulterated milk." The prosecution relied entirely upon the fact that milk found in the shop of the accused, when tested by the lactometer, failed to attain the standard which is said to denote pure milk. Prof. Doremus, as expert for the defence, demonstrated by experiments that the employment of the lactometer enabled the fraudulent vendors of milk in the country to remove much of the cream, and also to add water to the half-skimmed milk. He also testified that milk from certain cows, during certain seasons of the year, would yield a pure milk liable to condemnation when criticised by this untrustworthy instrument; that he visited, in the month of September, a celebrated milk and butter district (Orange County, New York State), and obtained twelve specimens of milk from as many pasture-fed cows, five of which samples were of less gravity than the standard adopted by the Board of Health. Chemical analysis showed their richness in butter to be the cause of their want of buoyant power. Dr. Doremus also referred to various French, German, and English authors, as substantiating his views, and quoted the sweeping denunciation of the lactometer by Mr. J. Alfred Wanklyn, in his work on "Milk Analysis." Justice John R. Brady, in reversing the judgment of the lower Court, said, that if—by tests made by scientific men—the lactometer or lactodensimeter furnishes only questionable evidence of adulteration, it should not be regarded as sufficient to warrant a conviction.

PATENTS.

ABRIDGMENTS OF PROVISIONAL AND COMPLETE SPECIFICATIONS.

A new or improved mode and means of preventing the deposition of sediment and incrustation in steam boilers. G. Nimmo, Glasgow. November 23, 1874.—No. 4018. This said invention consists in placing within the water in the boiler bags, pads, or "sweeps," made of cotton-waste, hemp, rope, or other soft material (or pieces of wood covered with a soft material), which, when saturated, possess a specific gravity rather heavier than the water; so that these, by the ebullition or boiling of the water, and by their own shape or configuration, rise and

fall in and agitate the water, so as to keep the sediment in motion and prevent it from settling or depositing in the boiler, and so prevent incrustation. When the boiler is to be cooled down, the sediment is blown or drawn off through a pipe and cock for that purpose. When used for either single or double flued Cornish boilers, or multiple tubular boilers, the bags, pads, or "sweeps" are made sufficiently thin to pass between the flues and the shell, or between the tubes themselves, so as to brush and clean them.

Improvements in the manufacture of air-gas for lighting and heating purposes, and in the apparatus connected therewith. C. F. Schüssler, Hamburg. November 23, 1874.—No. 4021. The novelty of the invention consists in the production of a heating and illuminating gas by the construction of a chamber containing tubes or channels in which hydrocarbon liquid is caused to flow and impregnate atmospheric air, which is fed thereto under pressure of an arrangement of bellows worked automatically.

Improved compounds or combinations of ingredients for waterproofing filaments, threads, fabrics, and fibrous materials generally, also for coating walls and other bodies. A. Wilkinson, High Street, Marylebone, Middlesex. November 23, 1874.—No. 4026. This relates to forming a composition of ingredients, such as bisulphuret of carbon, benzine, sulphuret of zinc, ammonia mixed with naphtha, india-rubber, gutta-percha, or other like gum, for the purposes stated in the title. In some cases an exterior coating, composed of sugar of lead and linseed oil mixed with whiting and litharge, is applied, and before being completely dry an extra coating of the first mentioned composition is applied to render the materials completely waterproof.

Certain improvements in the manufacture of acetate of soda, and in the processes employed therefor. H. B. Condy, Battersea, Surrey. November 24, 1874.—No. 4037. The novelty of the invention consists in precipitating the organic impurities existing in acetate of lime, and the completion of the decomposition of the solutions of acetate of lime or sulphate of lime (which is dissolved to a small extent) by means of carbonate of soda. The combination of such processes enable me to produce good merchantable acetate of soda by one crystallisation.

Improvements in refining saltpetre, and in apparatus for the same. G. Haycraft, Faversham, Kent. November 25, 1874.—No. 4046. My invention relates, first, to the arrangement of the vessels in which the several stages of the refining process are performed, in such a manner that the saltpetre will be caused to pass by gravitation alone from vessel to vessel, whereby hand labour and the use of pumps for lifting the saltpetre from one level to another is dispensed with. Also to the method of boiling the rough saltpetre by introducing steam among the mass, with or without the use of a steam coil. Also to the method of recovering saltpetre from the mother-liquor which drains from the refined saltpetre. Also to the method of cooling the saltpetre during crystallisation, as well as to the construction of the boiling and reducing vats, the agitators, and the arrangement of gearing for driving the same.

Improvements in automatic chemical telegraphs, comprising transmitting and receiving instruments, circuits, and apparatus connected therewith, part of the said improvements being also applicable to other telegraphs. A. M. Clark, Chancery Lane, Middlesex. (A communication from W. E. Sawyer, Washington, Columbia, U.S.A.) November 26, 1874.—No. 4063. The present invention relates to a new and improved system of automatic chemical telegraphs, comprising transmitting and receiving instruments, circuits, and apparatus connected therewith.

Improvements in bleaching wool, textile fabrics, and fibres. T. N. Palmer, Lansdowne Road, Dalston, Middlesex. (A communication from L. P. H. P. Balna, Rheims, France.) November 28, 1874.—No. 4087. I use a bath composed of bicarbonate of soda and subcarbonate of magnesia, into which air is introduced by pressure. I put into an apparatus the above mentioned matters with the addition of sulphuric acid; the gas which is emitted from this apparatus saturates the bath, and in order to continue the application I saturate the same bath with sulphurous gas, accompanied by the introduction of the oxygen of the surrounding air. I also use chloride of lime in the water saturated with sulphurous gas, always with the addition of the oxygen of the air. I reserve to myself the use of all the means producing sulphurous, carbonic, and oxygen gas applied to the bleaching of the above mentioned materials.

A new or improved ink-pencil. J. L. Petit, Birmingham, Warwick. November 28, 1874.—No. 4090. This new or improved ink-pencil consists of aniline dyes, or mixtures thereof, thoroughly incorporated with finely powdered plumbago, or black, or deep coloured chalk, together with gum-water or other adhesive material soluble in water. The paste produced may be made into sticks, and dried, and be used as a crayon or chalk, or be enclosed in cedar as an ordinary black-lead, or be made into fine rods to be used as an ever-pointed pencil. The writing produced by the use of the ink-pencil may be copied by pressing moistened paper upon it. The original writing is thereby made more legible as the moisture dissolves more or less of the aniline compound in the composition, and the dissolved matter dyes the vegetable fibre, and deepens the colour of the writing. Several copies of the writing may be taken. The ink-pencil may be produced in different colours.

Improvements in the method of and apparatus for extracting the juice and crystallisable matter from sugar cane, and after manipulation of the same. G. W. Risien, Demerara. (A communication from William Russell, of Demerara.) November 30, 1874.—No. 4094. The features of novelty which constitute this invention consist in passing the cane stalks through two mills spaced about thirty feet apart and connected by a chamber. In this chamber works an endless band or carrier, and the chamber is provided with two tiers of pipes, through

which a continuous supply of hot water, or steam, or cane juice passes in a spray and saturates the expressed cane stalks or begass on passage of same from mill to mill. The hot water, or hot water and steam, is supplied from the cane juice heaters used in the manufacture of sugar, and the juice from the second mill may also be used for saturation by passing it through a juice heater and then to the supply pipes for saturating the begass before mentioned.

Improvements in apparatus employed in the washing and cleansing of wool and other fibres. J. Clough, Manchester Road, Bradford. November 30, 1874.—No. 4095. In conveying wool or other fibrous material (after being washed) from the washing bowl or trough to the squeezing rollers, a carrying fork is employed, operated by crank motion with balance wheel on the end of the shaft, and worked in a curved plate, the curve of which is suited to the movement the fork is desired to describe; this curve may be flatter or quicker according to the distance the squeezing rollers may be from that part of the bowl or trough where the wool is lifted by the last washing fork and placed upon the curved plate. The curve of this plate begins at the point where the wool is leaving the sud or wash liquor and continues upwards as far as is necessary to give the wool sufficient fall down an incline leading to the nip of the rollers, and so to pass it through the rollers. This curved plate has two sides or guards to prevent the wool from falling off it when being taken forward by the carrying folk to the rollers. Stationary holders are placed at the foot and on the top of the curved plate to retain the wool, when lifted from the bowl or trough and placed upon it, until the carrying folk working in the curve comes and takes such wool forward to the top of the inclined plane, and from whence it slides down to the nip of the rollers. There are holders called ratch tooth holders (by the teeth taking the form of ratch teeth). The head of the carrying fork which works in the curve is fixed upon a square part of the lower end of the "stalk," and is movable up and down it; an adjustable steel spring, being placed on the stalk above the head of the fork, gives way by compression according to the thickness of the body of wool which it may for the time be carrying, and by which it readily suits either a larger or a smaller amount of material under operation. The prongs of this fork are formed slightly rounded at the front part, running out conically to the back part.

Disincrusting and preventing the incrustations in steam boilers. J. B. Deluy, Naples, Italy. November 30, 1874.—No. 4108. My invention is an improvement on all the discoveries made until now; it is the result of my studies, and consists in a powder made out of one only vegetable produce pulverised, which I introduce in the boiler through the manhole when the supply of water is completed. My disincrusting powder, which I name "Disincrustant Marseillais," is neither a compound or a mixture, and it disincrusts, cleanses, clears out all deposit by spontaneous action. By the ebullition the incrustation is dissolved and goes out through the tap purger.

NOTES AND QUERIES.

Terra Alba.—Is the article called terra alba made *entirely* from the ground alabaster of Derbyshire, or is there another kind of terra alba made or *manufactured* at the Salt or Acid Works of Newcastle or elsewhere in England? Assuming that it is also made at the Salt Works as well as at Derbyshire, is the former better in any way, and what is the price per ton laid down at Liverpool? Please give present market prices for the Derbyshire, and that which is manufactured at salt works or elsewhere. There is another article called pearl hardening which I do not wish confounded with the other, but would like the present market price of this also.—THOMAS MANNING.

MEETINGS FOR THE WEEK.

- MONDAY, Jan. 17th.—Medical, 8.
London Institution, 5.
- TUESDAY, 18th.—Civil Engineers, 8.
Zoological, 8.30.
Royal Institution, 3. "On the Classification of the Vertebrated Animals," by Prof. Garrod.
- WEDNESDAY, Jan. 18th.—Meteorological, 7. (Anniversary).
Society of Arts, 8.
Geological, 8. "On some Unicellular Algæ Parasitic within Silurian and Tertiary Corals, with a Notice of their Presence in Calceola sandalina," by Prof. P. Martin Duncan, F.R.S., V.P.G.S. "How Anglesey Became an Island," by Prof. A. C. Ramsay, LL.D., F.R.S. "On the Influence of Various Substances in Accelerating the Precipitation of Clay Dissolved in Water," by William Ramsay.
- THURSDAY, 20th.—Royal, 8.30.
Royal Institution, 3. "On the Chemistry of the Non-Metallic Elements," by Prof. Gladstone.
Chemical, 8. "On Narcotine, Cotarnine, and Hydro-cotarnine," by G. H. Beckett and Dr. Wright. "On a Method of Estimating Bismuth Volumetrically," by M. M. Pattison Muir.
London Institution, 7.
- FRIDAY, 21st.—Royal Institution, 8. Weekly Evening Meeting. "The Optical Department of the Atmosphere in relation to the Phenomena of Putrefaction," by Professor Tyndall, 9.
- SATURDAY, 22nd.—Royal Institution, 3. "On Excavations in Asia Minor," by R. P. Pullan.

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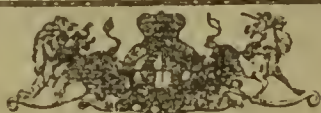
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THE CHEMICAL NEWS.

VOL. XXXII. No. 843.

THE NEW PHASE OF ELECTRIC FORCE.

PROFESSOR E. J. HOUSTON has kindly forwarded to us an early proof of an article he has contributed to the January number of the *Journal of the Franklin Institute*. Before, however, referring at length to his experiments we will reproduce from the *Scientific American* three diagrams of the apparatus used by Mr. Edison during his experiments.

Mr. Edison and his assistants were experimenting with a vibrator magnet consisting of a bar of steel fastened at one end, and made to vibrate by means of a magnet when they saw a spark coming from the core of the magnet so bright that they suspected something more than induction.

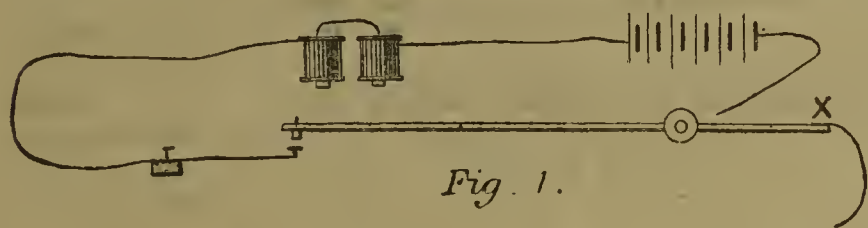


Fig. 1.

The vibrator and battery were then placed on insulated stands, and the wire connected with *x* (Fig. 1) was carried over to the stove about 20 feet distant. On rubbing the end of the wire against the stove, splendid sparks were observed. With the wire permanently connected with the stove, sparks could be drawn from any part of the stove with a piece of metal held in the hand. Again, while the

An experiment was made with the apparatus figured in Fig. 2 to show that the force in action is not induced electricity. All the parts are insulated except the gas fixture. A is the battery; B, a common telegraphic key; C, an electro-magnet; D, a bar of cadmium (or other metal, cadmium being the best) supported by an insulated stand; E is a mirror galvanometer; F, the gas pipe; G, a dark box enclosing pencils with graphite points (common lead-pencils). The unknown current passes from the bar of cadmium through the galvanometer without causing the slightest deflection, and—notwithstanding the gas

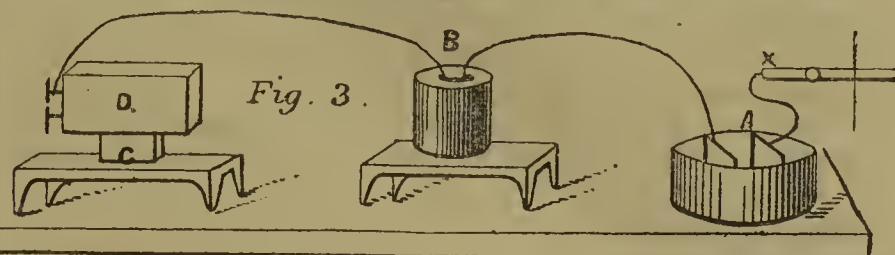


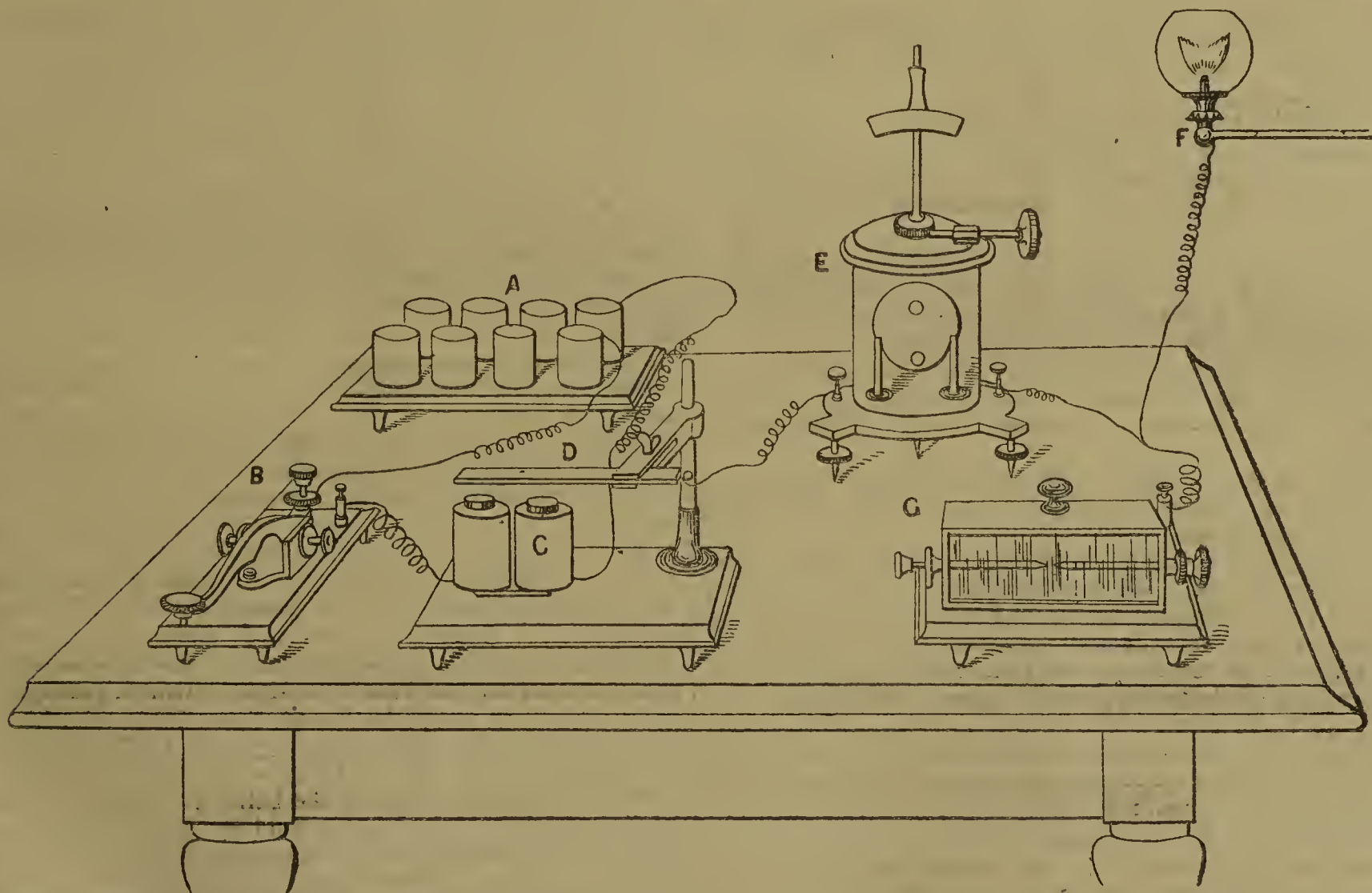
Fig. 3.

pipe connection, which would drain the wire of induced electricity, if there were any—bright sparks are visible between the graphite points in response to the motion of the telegraphic key.

Standing on an insulated stool, the experimenters draw sparks from the following arrangement (Fig. 3), in which *x* is the end of the vibrator (which, as well as the battery, is insulated); A, a secondary battery; B, a 200 ohm coil or copper wire; C is a block of iron, and D, a condenser, all well insulated except A, which is of glass, and stands on the table.

In another experiment a glass rod four feet long with a piece of carbon fixed to one end, was well rubbed with a silk handkerchief over a hot stove, and the carbon point presented to the apparatus, the other end of the rod being held in the hand with the handkerchief; sparks were drawn, yet the galvanometer chemical paper, the sense of

FIG. 2.



vibrator was in action, a block of iron was placed near *x*, but not touching the bar, nor connected with it in any way except by the wood of the base through the table, and sparks could be drawn from the iron. These experiments were made to show that the supposed new force is not amenable to the laws of voltaic or static electricity.

shock in the tongue, and a delicate gold leaf electroscope, were not in the least affected by the mysterious current.

On December 16 Mr. Edison brought his discovery before the Polytechnic Club of the American Institute. The editor of the *Scientific American*, who was present, says that Dr. Beard gave a very lucid account of the

phenomena, observed by himself as well as by Mr. Edison; and he pointed out in what particulars the new spark is similar to some forms of electricity, and in what it appears to differ from the various known forms of that force. Like a true scientist, he pointed out that only such phenomena as every competent experimenter is able to verify at any time are worth consideration; and he spoke of the sources of illusion and delusion which misled Reichenbach, and afterwards others who asserted that they had verified his alleged discovery of the so-called odic force.

In 1871 Professor Houston published the description of some experiments* which bear, both in method and results, a partial resemblance to those by Mr. Edison. The original experiments, made with a Ruhmkorff induction coil, capable of throwing the induced spark six inches in free air, have now been repeated by Professors Houston and Elihu Thomson. The results are thus described by Professor Houston:—

The induction coil was worked by means of an electro-poin battery of ten cells coupled for an intensity of ten. The elements of each cell consist of a single plate of zinc placed between two plates of carbon. The available surface in each cell is about three by six inches. One pole of the battery was placed in metallic connection with a gas-pipe and the other in similar connection with a large insulated conductor. On working the interrupting break-piece a torrent of characteristic white sparks of condensed electricity passed between the platinum points of the coil. Under these circumstances, that is, while the discharges were occurring between the points, sparks could be drawn from all metallic objects in the same room with the coil, or in adjoining rooms. The sparks were especially noticeable when metallic objects were approached to the gas- or water-pipes of the building, or to metallic surfaces in connection therewith, as in the case of the pneumatic trough, steam-engines, and boiler before mentioned. As already implied, actual contact between the gas- and water-pipes and one of the wires leading from the coil, was unnecessary, as distinct sparks were afforded by a stove in the same room, and from another in an adjoining room.

In order to test the suspected similarity between these sparks and those described by Mr. Edison, we submitted them to the tests proposed by him. We made a number of experiments and obtained the following general results, viz.—

First. The gold leaves of a delicate electroscope did not diverge on being brought into contact with metallic objects yielding the sparks, although in every case the sparks could be seen at the point of contact.

Second. The needle of a delicate astatic galvanometer was not sensibly deflected by the sparks, on an apparent current being caused to traverse the coils of the instrument.

Third. A small shred of cotton-wool was not sensibly attracted or repelled by objects from which the spark might be obtained.

Fourth. The so-called retroaction of the spark was distinctly observed. On looping a wire back on itself, a decided spark was seen at the point of contact made by the end of the wire with any portion of the wire itself.

All the above results are in strict accordance with the known laws of electricity, as will appear hereafter.

To still further compare these results with those obtained by Mr. Edison, we dispensed with the use of the induction coil, and employed an apparatus similar to that described by him, viz.,: An electro-magnet in connection with the battery already mentioned, the current of which was rendered intermittent by means of an ordinary interrupter. With this arrangement, a wire in contact with the core of the magnet yielded sparks having all the properties described by Mr. Edison.

From a careful reading of the published accounts of Mr.

Edison and Dr. Beard, it appears that the alleged discovery of a new force is based on their failure to obtain from the sparks indications of electrical charges or currents; or in other words, in the apparent absence of electric polarity. All the effects noticed, however, are readily explainable by reference to the presence of an instantaneous outgoing current, immediately followed by an incoming one, with the complete re-establishment of electrical equilibrium. When we bear in mind the enormous velocity of electrical currents of this character, probably some hundred thousand miles per second, we can readily understand that the flow and reversion of the current would take place in an exceedingly small fraction of a second; a space of time sufficient, were the current merely direct, to produce any decided divergence of the leaves of the electroscope or the needle of the galvanometer. *The presence of the inverse current, immediately following the direct current, would absolutely and necessarily prevent the exhibition of electrical polarity as exhibited in the motion of the electroscopes, galvanometers, and of similar instruments.*

It is doubtless due to the fact that the direct and inverse currents are opposite in their effects, and therefore produce instantaneous electrical equilibrium, that Mr. Edison failed to obtain the characteristic twitching of frogs' legs or the discolouration of iodised paper; for, although these results would unquestionably follow an electric current in one direction, their presence would be masked by the opposite effects produced by the instantaneously following inverse current.

The possibility of the existence of the direct and inverse currents, as above described, may be questioned; but when we bear in mind that the sparks can only be obtained by the interruption of the battery current, and that it is necessary to pass the battery current through a long coil of wire, conditions in every way favourable to the production of instantaneous induced or extra currents, the direct and the inverse currents follow as a matter of necessity; for the induced or extra currents in the coil of wire necessarily produce, in the core of the magnet and the metallic wire in connection therewith, an electrical current in one direction, instantaneously followed on the cessation of the induced or extra current, by a current in the opposite direction for the re-establishment of the electric equilibrium in the cores of the magnet.

It is a fact well known to all versed in electrical science that the induced current produced at the moment of making contact with the interrupter flows in the opposite direction to that produced on breaking the contact. This fact would in itself, exclusive of the above explanation, be sufficient to account for the production of inverse currents in the core of the magnet, when the interruption of the battery current was sufficiently rapid. Dr. Beard admits that the phenomena may be referred to a somewhat similar explanation, but the value he attaches to his supposition may be judged from his subsequent adoption of the term "apolic force" as a preferable term to "etheric force."

It is matter of surprise to us that both Mr. Edison and Dr. Beard endeavoured by careful insulation to eliminate in the apparatus employed by them the effects of induced electricity or induction, since it is a recognised fact in electrical science that the more perfect the insulation the more decided the effects of induction, of which, perhaps, no better instance could be found than the care taken to thoroughly insulate the secondary coils in the induction on Ruhmkorff apparatus. In view of the above considerations, we feel warranted in the belief that all the phenomena noticed by Mr. Edison and Dr. Beard are explainable by the presence of inverse electrical currents of considerable quantity, but comparatively small intensity, instantaneously produced at the making or breaking of the battery circuit.

There was noticed during the progress of our experiments with the induction coil the following curious phenomenon, which appears to us favourable to the explanations

* *Journal of the Franklin Institute*, June, 1871.

we have adduced. One of the poles of the coil was connected with a gas-pipe, and the other with an insulated conductor of considerable surface in the room containing the Ruhmkorff core. This room is in connection by a telegraph wire with the chemical laboratory on the floor below, and with an earth circuited station, D, in another building, about 500 feet distant in a direct line. On the interrupter of the coil being worked, a peculiar clicking sound was heard, by both of us in the line wire in the chemical laboratory. The operator at D was requested to observe whether any unusual phenomena were noticed at his instrument. He at once telegraphed to us that a distinct "tinkling sound" was heard, which did not vary whether the ordinary battery current of the line was opened or closed. Since he did not know what to expect, his confirmation of our observation was very satisfactory. The production of the sound is probably referable to a rapid succession of molecular changes produced in the wire by the sudden reversion of its electrical states.

Central High School, Philadelphia, December 11, 1875.

REPORT ON THE DEVELOPMENT OF THE CHEMICAL ARTS DURING THE LAST TEN YEARS.*

By Dr. A. W. HOFMANN.

(Continued from p. 13.)

THE science of physics reveals three procedures by which a reduction of temperature can be effected, ice being the result if the cooling is sufficiently intense and is applied to water. The methods in question are:—The solution of solids (salts); the spontaneous evaporation of liquids; and the expansion of æriform bodies. Each of these methods has met with practical applications; the first-mentioned or solution-process for reducing the temperature of small mass in simple apparatus not acting continuously; the two others, evaporation and expansion, for the uninterrupted production of ice in complicated machines.

I. Cold obtained by Solution.

Every mixture of substances in proportion as it produces, during solution, the greatest depression of temperature in its own mass is called a freezing mixture. Various mixtures of this kind have long been known, and may be found described in all text-books of physics. The best known and most commonly applied both in domestic and technical affairs pre-supposes the presence of ice. It consists of 3 parts of ice and 1 of common salt, which dissolve each other, whilst the temperature falls to -21° , the freezing-point of a concentrated solution of chloride of sodium. The solution of a part only of the mixture is requisite to produce this low temperature in the entire mass. Not till heat penetrates from without into the mass does a further melting take place, the temperature remaining the same. Consequently the above degree of cold may be kept up till all the ice has been melted with the salt. It is necessary, however, to keep the mixture continually agitated. This snow and salt-freezing mixture is used in preparing ice-creams, for which a temperature of about -12° is required. As the essential point here is the congelation of water and the other substances present may be neglected, as far, at least, as their specific and latent heat is concerned, it is easy to calculate what weight of ice-cream may be prepared with a known quantity of freezing mixture.

The freezing apparatus of the confectioners consists of a tin vessel for receiving the ingredients, placed in a larger bath of wood or tinned copper. The interval is filled with ice and salt, which are constantly stirred that the mutual contact of the two may be perfect. If this is

neglected the salt, after a portion of the solution has been formed, sinks to the bottom and ceases to act upon the ice. Since about 1865 a freezing apparatus for domestic use has been introduced from Paris arranged as follows:—A cylindrical pewter vessel with double sides is fitted in the middle of a jacket with two pivots, which rest upon two supports fixed in a block of wood. One of the pivots is prolonged so as to form a handle which serves to keep the cylinder in continual rotation. The two plane ends of the cylinder are disks of wood, which are pressed upon the cylinder by a peculiar arrangement, india-rubber rings being used to preserve complete tightness. The interval between the double sides of the cylinder is filled with a bad conductor of heat. A cone of pewter is introduced into the interior, and can be opened on one side to receive the materials for the ice-cream; the annular interstice is filled with salt and ice, which are introduced from the other side. The lid is put on, and the handle is turned for about five minutes. The lid of the cream receiver is then taken off, and the matter which has become deposited on its inner sides is scraped off with a spatula and stirred up with the still unfrozen residue to a butter-like consistence. The apparatus is closed again, turned for five minutes, opened again, and the contents stirred up as before—an operation which is repeated a third time. In a quarter of an hour the ice-cream is ready. The apparatus acts satisfactorily, but it is troublesome and rather costly.

(To be continued.)

A STUDY OF HYDROCARBONS.

No. I.—THE CONSTITUTION OF ALIZARIN.

By S. E. PHILLIPS.

THERE are two methods by which we may approach this great subject:—One is to grapple with the minutiae of details, and thence to try and reduce the interminable chaos to some ordinal principles of classification; and thus it is that acids are grouped according to some ideas of monatomic, diatomic, and polyatomic peculiarities; and the same with alcohols; and between these are more neutral groupings of aldehyds, ketones, quinons, paraffins, olefines, &c., and each series with their corresponding primary, secondary, tertiary, and other subdivisions, while an immense cloud of hypothesis darkens the whole proceeding, and graphic, constitutional, and other efforts of atomic disposition are freely put in requisition, as if to reduce the void to some elements of classification.

This system may be profitably studied in the recent and very valuable works of Armstrong and Schorlemmer.

Another, an older and simpler way, with far less of pretension and hypothesis, is to subordinate very much of all this minutiae, and to aim at some general grasp of the whole principle of "radical" behaviours; so that, upon this solid basis, the minutiae of fact and experiment may supervene, to fill in the rugged outline, and stamp it with that quality of simplicity and beauty which—in all departments of human enquiry—is the polar star which both impels and rewards the highest efforts of human labour.

Nor let it be supposed that this aspect looks with coldness upon the Herculean efforts which are so ably summarised in the works referred to, which are published at a popular price out of all proportion to their intrinsic worth; nor should it be thought that we would lessen by one tittle these great efforts to ascertain the meaning of those isomeric or allotropic differences which now multiply at every step. But we do think it is high time that some safe guiding principle should be selected to harmonise the past and fructify the future.

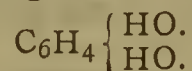
The artificial production of the beautiful "Turkey-red" may well be looked upon as one of the proudest triumphs of modern chemistry; but to one who has closely watched the teeming memoirs and public lectures devoted to its

* "Berichte über die Entwicklung der Chemischen Industrie Während des Letzten Jahrzehends."

elucidation, it becomes a painful picture of the really backward state of science which tolerates such vast efforts with so little of fruition in the direction so much desiderated.

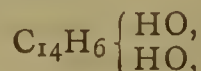
A vague idea, very widely prevalent, considers alizarin as diatomic and dibasic, or that in the quinone genesis H_2 are replaced by the divalent group (O_2), &c.

A diatomic phenol is said to be a combination of a hydrocarbon like marsh-gas with 2 atoms of hydroxyl—

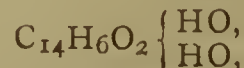


So Perkin, Schunck, and others believe in analogous type representations!

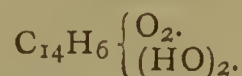
Anthraquinon is therefore—



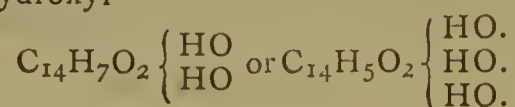
and alizarin—



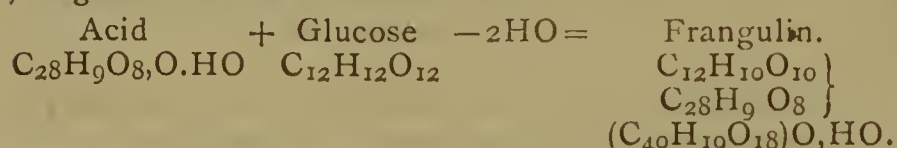
or, after Graebe,



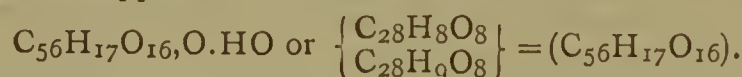
M. Faust has been engaged for some years past upon a very analogous substance called frangulinic acid: at one time he is busy trying to find out whether it has 2 or 3 atoms of hydroxyl—



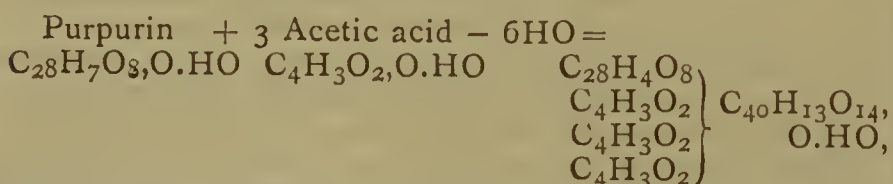
It seems clear, first of all, that the acid by deoxidation yields anthracen, thus establishing a kinship with alizarin; then we find the glucoside from the bark of *Rhamnus frangula* contains the elements of—



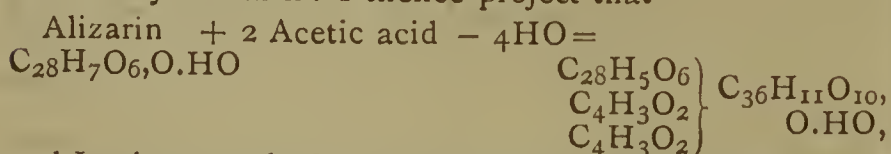
Why not digest this acid with ammonia, under the light of a thousand analogies, and obtain the amide ($C_{28}H_9O_8$) H_2N , or similarly with aniline, to obtain the corresponding anilide: a very strong corroboration is found in the tendency of the acid to condensation when the diacid appears to contain the elements of—



Alizarin and purpurin, in all probability, have a similar glucoside origin, but the derivation in that sense is not so clearly made out. Perkin has obtained a triacetyl purpurin which contains the following elements:—



and a similar tribenzoyl compound, and speaks of a similar diacetyl alizarin: I thence project that—



and I subsequently find that analysis gives $C_{36}H_{12}O_{12}$!

Why not, therefore, obtain the amides, anilides, or sulpho-salts of these bodies, and thus determine for ever whether these bodies are mono- or bibasic?

The presumption is irresistible that the alizarin amide would contain $C_{28}H_7O_6.H_2N$, and similarly with purpurin. That such would easily and almost certainly result is evidenced by a wide range of collateral facts connected with the following hydrocarbons:—

Benzol Series.	Naphthol Series.	Anthracen Series.
$C_{12}H_5.H$	$C_{20}H_7.H$	$C_{28}H_9.H$
$C_{12}H_5.O.HO$	$C_{20}H_7.O.HO$	$C_{28}H_7.O.HO$
$(C_{12}H_5O_2).O.HO$	$(C_{20}H_7O_2).O.HO$	$(C_{28}H_7O_2).O.HO$
$C_{12}H_5O_4.O.HO$	$C_{20}H_7O_4.O.HO$	$C_{28}H_7O_4.O.HO$
	$C_{20}H_7O_6.O.HO$	$C_{28}H_7O_6.O.HO$

The No. 1 of the anthracen series is not anthracen, and it is more usual in lists of this character to lead off with the hydrocarbon of the next higher series, as if it were at all necessary to eliminate 2H before inserting the 2O. This first step, however, in the oxy-progression is almost wholly ignored, and a quinon body, No. 3, is supposed to result from the elimination of H_2 and their replacement by the divalent group (O_2), &c.

Such lists can be projected in several ways, according to the object in view, and we offer the following:—(See Table below).

No. 1 gives the hydrides of ethyl, phenyl, toluyl, &c., and many intermediates might be added, as also chrysen and others beyond.

No. 2 gives aldehyd, quinon aldehyd, benzoic aldehyd, and the aldehyds of naphthaquinon and anthraquinon. That a similarity of type and general properties may here subsist all through there can be no question.

No. 3 gives acetic, collic, and benzoic acids, naphthaquinon, and anthraquinon. With a greater knowledge of minutiae than we at all pretend to in this superficial survey, it is quite feasible that the terms collic and benzoic acid, as associated with the two quinons, would have to be replaced by quinon and salicyl, or some other isomers. We use these terms merely as representative of their types, and claim for the series all the regularity and interest attaching to No. 2.

No. 4 gives glycollic acid, an oxy-quinon body, represented by chloranilic acid, oxy-benzoic acid, oxy-naphthaquinon, and oxy-anthraquinon.

No. 5 gives glyoxylic acid, dioxy-benzoic acid, naphthazarin, and lizaric acid or alizarin.

No. 6 gives gallic acid, trioxo-naphthaquinon, and purpurin.

No. 7 gives oxy-gallic acid, tetraoxy-naphthaquinon, and oxy-purpurin or rufiopin.

Now, why is it that some of these isomers are regarded as comparatively neutral or aldehydic bodies, while others of the same composition are more definitely acid? Many have little hesitation in responding to this enquiry; but it is discouraging to observe how variously the atoms are arranged, according to the particular views entertained. Our position is one of far less pretension, and if we greatly extend the "hydrate" type it is only to express or classify a very wide series of relationship.

There are two directions in which we should look for isomeric differences of individual properties:—

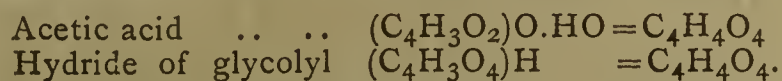
(1.) A difference in the constitution of the radical: thus benzoic acid and salicylic aldehyd may both conform to certain general reactions, and so far establish their equal claim to the type $C_{14}H_5O_2.O.HO$, or any other which may better represent the generic similarity of those reactions. It is a matter of almost absolute knowledge that such radicals can have a different genesis, or can be built up of different materials, yet retaining the same proportion of constituent atoms as a whole.

	Ethyl.	Phenyl.	Toluyl.	Naphthyl.	Anthracyl.
1.	$C_4H_5.H$	$C_{12}H_5.H$	$C_{14}H_7.H$	$C_{20}H_7.H$	$C_{28}H_9.H$
2.	$C_4H_3.O.HO$	$C_{12}H_3.O.HO$	$C_{14}H_5.O.HO$	$C_{20}H_5.O.HO$	$C_{28}H_7.O.HO$
3.	$C_4H_3O_2.O.HO$	$C_{12}H_3O_2.O.HO$	$C_{14}H_5O_2.O.HO$	$C_{20}H_5O_2.O.HO$	$C_{28}H_7O_2.O.HO$
4.	$C_4H_3O_4.O.HO$	$C_{12}H_3O_4.O.HO$	$C_{14}H_5O_4.O.HO$	$C_{20}H_5O_4.O.HO$	$C_{28}H_7O_4.O.HO$
5.	$C_4H_3O_6.O.HO$	$C_{12}H_3O_6.O.HO(?)$	$C_{14}H_5O_6.O.HO$	$C_{20}H_5O_6.O.HO$	$C_{28}H_7O_6.O.HO$
6.			$C_{14}H_5O_8.O.HO$	$C_{20}H_5O_8.O.HO$	$C_{28}H_7O_8.O.HO$
7.			$C_{14}H_5O_{10}.O.HO$	$C_{20}H_5O_{10}.O.HO$	$C_{28}H_7O_{10}.O.HO$

(2.) There may be a difference of type, which further knowledge may be able to exemplify, and hence the confusion and mistake we have pointed out between chloroacetic acid and chloride of glycolyl.

When chemists use the *true* chloroacetic acid they get a real acetyl substitution; when they use the isomer, still calling it by the same name, and not perceiving the distinction, they obtain a glycolyl substitution!

Now as whenever we have a chloride we may almost certainly have a hydride,—therefore, replacing the Cl by H, we have as a typical case the two isomers in question:—



And it is more than probable that in this way many of these bodies may vary isomerically—first, in their radicals, and secondly, in their types.

(To be continued.)

ON THE NECESSITY FOR ORGANISATION AMONGST CHEMISTS, FOR THE PURPOSE OF ENHANCING THEIR PROFESSIONAL STATUS.

By C. R. ALDER WRIGHT, D.Sc.

THE remarks which have from time to time appeared in the CHEMICAL NEWS, calling attention to the daily encroachments made on the province of the professional chemist by medical men, engineers, and members of other professions, and to the numerous disabilities and inconveniences to which chemists are subjected for want of a proper mutual organisation whereby their status—in the eyes of the public generally—may be properly fixed, appear to be the expression of a very general feeling that, unless some steps are speedily taken to ensure that all persons who profess to be analytical and consulting chemists are duly trained and properly qualified for such positions, professional chemistry will soon become an occupation of such low standing in the eyes of the public that few properly educated men will embrace it as a profession; and thus a great injury will be inflicted on British chemical science, inasmuch as, under the present order of things, a scientific man devoted to chemistry can only maintain himself by combining with his investigation-work more or less of tuition or of business chemistry—a state of matters to be deplored, but which does not seem likely to be altered at present in this country.

At the present moment professional chemical work (apart from science teaching and lecturing, and from scientific research) is carried on by two classes of practitioners, viz., by men who have spent years of time and considerable sums of money in perfecting their technical as well as scientific knowledge, so as to become qualified to act as thorough analytical and consulting chemists: of these many combine tuition with professional chemistry, holding chairs and lectureships in the different schools and colleges of the country, whilst a considerable proportion manage to snatch some time from the hard work of their multifarious bread-winning duties for the carrying out of original researches; secondly, by a much less completely trained and specially qualified class of men—in many cases belonging rather to the artisan than to the highly-educated classes: frequently such pseudo-chemists commence as subordinate assistants and bottle-washers in the laboratories of chemical works and factories, in which places they may certainly acquire some knowledge of the more or less rough-and-ready testing processes in use for the examination of a few materials and products, but necessarily do not obtain the thorough systematic instruction necessary to enable them to examine with accuracy the innumerable substances that may be brought to them in general practice. Sometimes they are science teachers who, being possessed of a small smattering of laboratory

knowledge, endeavour to supplement their by no means too great earnings as teachers, by undertaking professional chemical work, for which as a rule their experience and skill are scarcely sufficient. Occasionally they are medical men possessing only the small modicum of chemical knowledge gained during their studies in the medical school, and often wholly innocent of any notion whatever how general quantitative work should be conducted, although they may have picked up some acquaintance with the methods to be adopted in the examination of some few articles of food, of water, or air, &c.

These semi-chemists inflict a double injury on the legitimate practitioners. In the first place, although they continue to go on tolerably smoothly when routine analyses or work of ordinary occurrence is in hand, yet as soon as anything out of the common run crops up, or if a quantitative determination requiring more than ordinary skill is required, they are at sea: in many cases they get out of the difficulty by sending the analysis on to some one more skilled than themselves; in others, they make the attempt, and produce a result which often is satisfactory enough to the unfortunate client, but which is, as likely as not, perfectly fallacious: occasionally their results are checked by more competent chemists, when woeful discrepancies come to light. Cases of this sort are familiar to nearly every chemist, but the unfortunate result is that the general public—being unable to decide between the qualified man and the semi-educated empiric—throws discredit on both, and on the chemical profession generally. In the second place, these quasi-chemists, not having to recoup themselves for the time and money spent in acquiring a thorough knowledge of their profession, are frequently willing to do work for fees absurdly low: the general public finding that Mr. Smith or Jones, F.C.S., will do analyses at a certain figure, expects the competent man to do the same; and if the latter does not come down to the fees for which his bastard professional brother will work, he loses his clients and his income altogether.

It does not require demonstration that if any one professes to do analytical work for fees which cannot possibly remunerate a properly qualified man for the expense of his education, he must slur over the work in some way: accurate methods—requiring time, labour, and above all skill—are discarded in favour of rough and rapid tests; the work is done at race-horse speed, and without due care and attention; it is even alleged that occasionally the work is not done at all, the results being simply the offspring of the analyst's imagination! The sad results of the rapid increase in the number of imperfectly skilled persons attempting to practise as chemists are a rapidly growing distrust of chemists generally and their analyses in the eyes of the public, and a consequent diminution in the status of the professional chemist, together with an increasing difficulty in making an income out of chemistry as a profession by thoroughly skilled persons: this means, in many cases, that time which would otherwise be devoted to valuable scientific researches must instead be given to bread-winning. Moreover, the general public is frequently defrauded by being supplied with incorrect analyses and fallacious tests.

A good illustration of the lowness of the position chemists now occupy in the eyes of the public, as compared with members of other professions, is afforded by the amounts of the salaries offered to Public Analysts as compared with those of the Medical Officers of Health in the same districts. Frequently the Public Analyst is paid at a lower rate than a Nuisance Inspector, Surveyor of Roads, or even the Parish Beadle: the wonder is, accordingly, not that the best known names in the chemical world are—for the most part—conspicuous by their absence from the list of Public Analysts, but rather that there should be really a considerable percentage of skilled chemists amongst them. The bare fact that so many thoroughly trained chemists are willing to take posts as a rule much underpaid is a sufficient proof of the difficulty experienced in making a decent living out of professional chemistry.

The remedy which naturally suggests itself for these injuries to the public, and these and various other grievances of professional chemists, is that these latter should unite together, forming an *Association* or *Guild*, and that they should obtain a Charter enabling them to do as is done in what is virtually the Guild of Medical and Surgical Practitioners; *i.e.*, to *grant licenses to practise to duly qualified persons only*. The possession of the licentiateship of the Guild would then be a fair guarantee to the public of the efficiency of the owner; whilst non-licentiated practitioners should be debarred from the power of enforcing, by law, payment for the work which they are presumably incompetent to perform (just as in the medical profession): neither the certificate of analysis nor the evidence of experts, as such, should be admissible in any court of law in the United Kingdom. Probably a special Act would be requisite to enforce these conditions.

The complaints now frequently made—and in some cases with but too much truth—of the incompetence of many so-called chemists and analysts, and the slovenliness and unreliability of much of their work, would in all probability cease altogether when such an Association was once formed—especially when it was known that for proved professional misconduct a licentiate's name would be erased from the rolls of the Guild (much as a Solicitor can be struck off the Rolls, or as a Counsel may be disbarred, or a theological practitioner inhibited from preaching, or unfrocked). A most salutary check on unconscientious work would be established if any client who found analyses of the same sample, by different licentiates, to differ unjustifiably, should have the right of bringing the matter before the Council of the Guild, who should then appoint a referee to examine into the case, the expenses of so doing to be paid by that analyst who should be found to be in the wrong.

When once fairly inaugurated, the Guild might perhaps advantageously consider the propriety of fixing a tariff of fees for ordinary commercial analyses, &c., and of promulgating official methods of analysis. Of course the formation of such a tariff of fees would in no way interfere with the right of each specialist or man distinguished in science to charge his own higher fees for reference cases or other special services; whilst no interference need necessarily be thus caused with the present system of analysts contracting with manufacturers, &c., for series of analyses and valuations of their products for the purpose of guiding and controlling their manufactures. Each certificate of analysis by which goods are bought and sold, on which an action or a prosecution is based, &c., might, however, be advantageously paid for according to a fixed tariff.

It is at once noticeable that an organisation for business purposes amongst chemists bears no very intimate relationship either to purely scientific research or to science teaching. A man may be an admirable theoretician, a good lecturer or teacher, without being more than an indifferent analyst, whilst he may be wholly unskilled in manufacturing and business chemical details; or conversely, a man be a thorough chemical engineer, an admirable adviser in reference to certain manufactures that he has specially studied, &c., without being able to teach theoretical chemistry, or without being capable of conducting a purely scientific research. It will, however, be generally conceded that the man who can originate, plan out, and work out to a successful conclusion, an experimental research, either in pure or in applied chemistry, must possess the analytical knowledge and manipulative skill requisite in an analyst, whilst his mental capacity must be at least equal to the demands likely to be made upon it whilst exercising the vocation of a consulting chemist.

Manifestly, too, such an organisation of professional chemists would occupy ground quite different from that taken up by the Chemical Society; the first would have reference to chemistry solely as a *business*, as a means of livelihood; whilst the second looks on chemistry only as

a *science*; only duly qualified persons would have the privilege of being registered on the rolls of the Guild; whilst, as is well known, any one who takes an interest in chemical science, whether connected with it as an experimental discoverer, as a druggist, science teacher or lecturer, or as a professional chemist, or whether unconnected altogether with chemistry in his occupation but taking an amateur interest therein, is ordinarily admitted to the Fellowship of the Chemical Society, provided (as in a recent case in point) there is no special reason to the contrary; and there is a great deal to be said in favour of this mode of proceeding.

Just as a man may be L.S.A. or M.R.C.P., and may practise medicine as a means of support, and may further by researches (say in pure physiology) acquire world-wide fame; so a consulting chemist might be a licentiate of the proposed Guild, maintaining himself by professional chemistry, and might also by his investigations in pure chemistry reach the highest pinnacle of distinction. The two connections in which physiology and abstract chemistry would stand to the two individuals respectively would have to each other only this indirect relationship, that the man whose reputation for scientific work is great is for that reason not unlikely to have lucrative private practice flowing in to him, although this result would not necessarily be brought about. The Chemical Society is associated with the one kind of connection, the proposed Guild with the other.

The question immediately arises, in what way can the practical difficulties be overcome that lie in the way of sorting the present race of chemists and pseudo chemists into these two classes, and of enforcing on the latter of the acquisition of more skill and special training than they at present possess before admitting them to the licentiateship? Possibly something like the following scheme might be practicable if only the chemists of higher standing would be willing to exert themselves a little for the purpose of bringing about the result so much to be desired, *viz.*, the exaltation of their professional status by excluding incompetent members. A general meeting of chemical practitioners might be convened at which a committee of selection might be appointed to draw up a preliminary list of professional chemists whose status as such, or whose published investigations, demonstrate their fitness for immediate registration as duly qualified chemists; gentlemen desirous of such registration being invited by advertisement or otherwise to fill up forms stating the grounds on which they base their claim for registration, such as the place and duration of their studies, the length of time in which they have been engaged in professional pursuits, and the character of these, the nature of their published researches, &c., &c. At another general meeting this list might be amended and adopted, and from its contents a constructive committee appointed to prepare draughts of the charter to be applied for, and of the bye-laws and rules and regulations of the Guild. Finally, at another meeting these draughts might be discussed, amended, and adopted.

As regards the admission to the licentiateship of the Guild, when this is once fairly inaugurated, all candidates unable to satisfy the original committee of their fitness for immediate registration and all subsequent applicants might be subjected to examinations of such a character as to test their fitness for the licentiateship, *practical quantitative work forming an essential part of such an examination*. Whether two successive examinations (like the Primary and Final of the College of Surgeons) should ultimately be adopted, and whether proficiency up to a certain standard in the subjects of ordinary English education, elementary mathematics, physics, French, or German, &c., should be insisted on as necessary qualifications for one aspiring to become a member of a recognised profession, and similar questions, are points to be decided by the constructive committee of the Guild. If two successive examinations were thought desirable, manifestly those who had passed the first examination would

have advantages over others who had not done so in competitions for assistantships and the like; whilst on the other hand there would be no reason why non-qualified and non-examined assistants should not be engaged in commercial and manufacturing laboratories for the purpose of doing routine mechanical work (*e.g.*, filtrations) just as at present; only such would be unable to give valid certificates of analysis for brokers' purposes or for legal proceedings until they had, by passing the examinations, shown that they had qualified themselves for promotion to the higher grades.

A most important question connected with any proposition for organisation amongst chemists is, in what way are the necessary preliminary expenses and the cost of maintenance of the Guild to be defrayed? As regards the first, by charging a registration fee (say five guineas) to each chemist who is licentiated by the Guild from its very commencement, a sum would probably be obtained sufficient to meet the expenses upon the obtaining of a charter, &c.; subsequently, examination fees might be charged in addition to all requiring examination before admission, and if necessary an annual subscription required from each licentiate; possibly it might be practicable to collect a revenue by requiring that each licentiate shall affix to every certificate of analysis given by him for the purpose of buying or selling goods or of being made evidence in a court of law, &c., a stamp (value say one shilling) to be issued by the Guild; no certificate to be admissible without such stamp being affixed, or the value of the stamp might be a fixed percentage on the analytical fee. Such a mode of collecting revenue finds its parallel in the legal organisation of this country.

The scheme thus roughly sketched out is not in any way put forward as a perfect one; but if by drawing out the ideas of chemists and promoting discussion on the subject, it should help to pave the way for a more suitable plan of organisation, its object will have been fulfilled.

PROCEEDINGS OF SOCIETIES.

PHYSICAL SOCIETY.

January 15th, 1876.

Professor GLADSTONE, F.R.S., President, in the Chair.

THE following candidates were elected Members of the Society:—Sir David Lionel Salomons, Bart., Arthur R. Granville, and Capt. Abney, R.E.

Prof. WOODWARD, of the Midland Institute, Birmingham, exhibited and described a novel form of apparatus for showing either the longitudinal motion of sound-waves, or the transverse vibration of those of light. It consists, essentially, of a series of balls suspended in a horizontal line by strings. These balls rest against a series of transverse equidistant partitions in a wedge-shaped horizontal trough, which can be raised and depressed parallel to itself. If, while a ball is placed against each partition, the frame be drawn aside in the plane in which the balls hang, and then slowly depressed horizontally, the balls will be successively liberated, the order in which this takes place being regulated by the heights of the partitions. As these gradually increase from one end to the other, the appearance presented is that of a series of condensations and rarefactions, as in the ordinary acoustic wave. If the frame be drawn aside, parallel to itself prior to depressing it, the balls will rest against one side of the trough, and can be liberated in succession, causing them to oscillate in planes parallel to themselves. By this means a vibration of the particles is set up, resembling that of polarised light.

Prof. GUTHRIE suggested that Mr. Woodward should devise a similar apparatus for exhibiting stationary waves.

Prof. WOODWARD promised to remember the suggestion, and stated that he had endeavoured to adapt the apparatus

to circular and elliptic wave motion, but experienced considerable difficulty.

Mr. LOCKYER then made a communication on "*Some Recent Methods of Spectroscopy*." At the outset he mentioned that he brought these processes forward in the hope that others present might be induced to take up some branch of the work. The first subject of which he treated was the photographing of the solar and metallic spectra. Mr. Rutherford, of New York, who has produced some of the finest photographs of spectra extant, has shown that to obtain clear photographs the smallest possible portion of the surface of the prism should be employed. An excellent method for ensuring this is to focus the light on the slit by means of a common opera-glass (as large as possible), which should reduce the beam of parallel rays incident on the first prism to not more than a quarter of an inch in diameter. Mr. Lockyer exhibited the 4-prism spectroscope employed by himself, to which a camera about 4 reet long is adapted. By this apparatus a large series of comparisons has been obtained between the sun and the metals, the slit employed being provided with five slides, so that the spectra can be accurately arranged side by side. It is advisable always to observe the image of the electric arc when comparing the spectra of metals with that of the sun rather than direct light. It is also found very advantageous to place the poles of the lamp at right angles to the slit, as by this means the spectra are more sharply defined than when observed in the ordinary manner; and, further, the appearances presented by the spectrum of a metal are comparable with those observed in the solar spectrum during, say, a magnesium storm. In the photograph comparing the spectra of aluminium and calcium it is noticeable that certain lines are common to the two, but those which are thick in the aluminium spectrum are thin in that of calcium, and *vice versa*. This depends on the quantities of impurity present. It has thus been shown that there are no proper coincident lines in the spectra of any two simple substances, and that there is no substance spectroscopically pure. The relation between the lengths of the non-continuous lines, and the amounts of metals employed to produce the spectrum, convinced Mr. Lockyer that it would be possible to employ the spectroscope for quantitative analysis. The earlier experiments in this direction were then referred to, as well as those in which Mr. Lockyer has recently been engaged in conjunction with Mr. W. Chandler Roberts, of the Royal Mint, with a view to ascertain how far it is possible to detect small differences of composition in gold-copper alloys, such as that used for the coinage. The method employed consists in measuring, by means of a micrometer in the eye-piece of a 4-prism spectroscope, the relative lengths of certain gold and copper lines when the image of an induction-coil spark passing from the alloy under examination is focussed on the slit. Although the results obtained have not been uniformly comparable, and therefore reliable, it is nevertheless certain that a difference of composition as minute as the 1-10,000th part is recognisable by this means. Another branch of spectroscopic research, to which Mr. Lockyer alluded, was the study of the absorption-spectra of metals when they are not subjected to so violent an action as that of the electric arc. Observations of this nature have been made at low temperatures by Roscoe and Schuster and by Mr. Lockyer, and at the highest temperatures produced by the oxy-hydrogen blowpipe by the latter in conjunction with Mr. Roberts. These experiments, which have been fully described in the *Proceedings of the Royal Society*, show that the absorption-spectra of metals may be divided into five classes, which, for any particular metal, depend on the amount of heat applied. They also show that in passing from the liquid to the most perfect gaseous state, vapours are composed of molecules of different orders of complexity; and, second, this complexity is diminished by the dissociating action of heat, each molecular simplification being marked by a distinctive spectrum.

The PRESIDENT enquired whether the indium line, to

which Mr. Lockyer had referred, and by means of which the metal was originally discovered, was absolutely identical with the hydrogen line.

Prof. McLEOD asked if Mr. Lockyer had found that the incandescence of the air made any difference in the character of the spectra, and drew attention to the advantage of a small lens placed in front of the slit.

Mr. WOODWARD enquired whether any mechanical means were adopted for ensuring that the lamp gave a constant light while in the horizontal position.

Dr. GUTHRIE referred to the spectrum observed when light traverses the vapours resulting from the action of copper on nitric acid. He wished to know whether the number of bands observed stands in any relation to the number of possible oxides of nitrogen at a given temperature. Or must one oxide of nitrogen be considered as being capable, at that temperature, of giving bright and dark bands according to the way in which the light acts on it.

Mr. LOCKYER, in reply to the President's question, said that, so far, no difference has been observed between the refrangibility of the hydrogen line and that of indium. He is anxious to ascertain whether any occluded hydrogen exists in the metal. Little or nothing is known as to the subject referred to in Dr. Guthrie's question. The use of the electric lamp eliminates all difficulty with reference to air lines, as its "atom-shaking" power is not sufficient to break into lines the spectra of nitrogen and oxygen. It was found necessary to make all the adjustments referred to by Mr. Woodward entirely by hand.

CORRESPONDENCE.

NAPIER'S MANUAL OF DYEING.

To the Editor of the Chemical News.

SIR,—It is now over twenty years since the old edition of Napier's "Manual of Dyeing" was published, it bearing date 1853. If any practical dyer were asked if any change had taken place in the dye-house in that time I can imagine the answer would be "Yes. Equal to almost a revolution. Many colours are obtained by quite different methods now, and many more are new altogether." Allowing, then, that such changes have taken place, we young dyers naturally look to those who profess to be leaders to give us information respecting them. For though the old processes may still dye a given shade, yet if more modern ones have been adopted by the trade we expect to be informed of them, because they are to us of much more importance than the old ones. Likewise in the chemical department; it is all very well to know the methods in use twenty or thirty years ago, but it is much more useful to know the processes in use at present.

I have said this much because I think our author has failed in this very important, and I may say essential, point. Especially is this the case in his receipts, many of which are both bad and costly as compared with those in use at present.

The first thing, however, that attracts my attention is a statement on page 11 to the following effect, viz.,—"The following are the effects upon a few of the more ordinary colours when exposed to the sun for a few days, often in a few hours:—

Dark indigo-blue becomes light.
Aniline-blue becomes light gray.
Yellow by fustic becomes straw-colour.
Yellow by chrome becomes greenish brown.
Red by madder becomes lighter.
Red by cochineal becomes paler.
Red by safflower becomes white, yellow tint.
Red by magenta becomes very light.
Green by indigo and chrome becomes light and dirty.
Green by vegetable yellow becomes dull and pale.
Black by logwood becomes brown.

When these colours are exposed wet the effects are much sooner produced."

If these effects were produced very much sooner few people except the dyers would see the colours at all.

Now that many of the changes do take place in time I readily admit; but, for the sake of my trade, I deny that they take place in a few hours; for I can scarcely think that a good dark indigo-blue overcoat put on in the morning would become light blue by the evening, even if exposed wet to the sun. And, again, with respect to blacks; it entirely depends upon the mordant used as to what changes will take place. For instance, it is well known that if the mordant be bi-chrome the change will be to a greenish hue, and not brown; but this the author never mentions. Now I notice that this is an addition to the new work, and not contained in the old edition. How a practical dyer could make such a statement is strange indeed. Speaking on oxygen, I find the following (page 31):—

"Dyed fabrics, whether wet or dry, suspended in this gas are not affected—a fact for the dyer to bear in mind when he is identifying this gas with chlorine."

Am I here to understand that oxygen plays no part in dyeing. There is a list of chemical actions, but the actions in dyeing are none of them included in that list, therefore the natural inference must be—oxygen has no action in dyeing. Could anyone get much further from the mark, and is it not calculated to lead anyone obtaining his information from this book only completely astray? The other part—viz., identifying O with Cl—I cannot understand, because I think every dyer who had learnt sufficient chemistry to know that these two elements existed at all would be able to distinguish, or at least have learnt at the same time their different properties.

It is stated (page 65) that alkaline lyes for steeping goods before bleaching ought not to be hotter than blood-heat, because if oil be upon the goods it will not be saponified—a strange statement if we consider the manufacture of soap: and I should imagine that whatever the ulterior action may be, saponification to a certain extent will be sure to take place.

It is stated (page 94) that grease and oil spots are not removed by carbonated alkalies, and it is therefore necessary to causticise them before using. There is no mention made whether for cotton or woollen fibre, and therefore anyone might suppose from this that the use of caustic alkalies was necessary for woollen fibre. Imagine a person gaining the whole of his information from this work, and then trying his hand upon some woollens goods. He would probably remember the experiment if he tried it upon a moderately large scale.

Speaking on alum, and the use of the alkalies in its manufacture, the following is given (page 119):—"AmHO alum is expensive, and possesses no corresponding advantage over the ordinary article." Compare this with "Crookes," page 275, where the following is given:—"The greater part of the alum now met with in commerce is ammonia alum. The ammonia alum is slightly more advantageous in strength." Compare also tome ii., page 205, Schützenberger's "Matières Colorantes":—"La fabrication de l'alum ammoniacal a presque partout remplacé celle de l'alum potassique, pour raisons d'économie." Which statements am I to believe? This manual has been published since either of the others quoted, and by a F.C.S. I am inclined, however, to believe the two quoted, more especially as I find all the alum I get for use is ammonia alum.

I find the following on green colours (page 209):—"Green is well known to be a compound colour produced by yellow and blue, and is *always* produced upon cloth by dyeing it first the one colour and then the other: it is not always the yellow that is dyed first, but sometimes the blue." On turning to the receipts at the end of the book I there find only two out of seven in which the blue and yellow are put on at two separate operations. Now either the above statement is wrong or the receipts are wrong,

which is it? If I have a green to dye this week, and to decide which will be best, to put first the one colour and then the other, or both together, I appeal to this "Manual," how am I to be guided?

Treating on galls (page 213), the analysis of Sir H. Davy is given, and 26 per cent of tannin is there said to be contained in the best galls. There is no mention about this being low—nothing to hinder the reader from taking this as a correct analysis. On the next page a method is given by which 35 to 40 per cent can be extracted; but the query is, How can 40 per cent be obtained from an article in which the best only contains 26 per cent? If the 26 per cent is too low according to better methods of analysis why give it, and say nothing about it if the other is too high, the same? But on turning to page 219 I find it given at 65 per cent.

Under the head Valonia nuts (page 227) I find the Scotch dyers are still behind in silk dyeing; in fact, just as they were twenty-one years ago. I hope that we Yorkshire chaps will be able to get up to them if they stand still a little longer.

On the same page I find divi divi has been tried as a dye in place of sumach, but is not much used. Compare page 63 of Slater's "Manual of Dye Wares":—"Divi divi is one of the most important of the class of astringents—much richer in tannin than sumach." Again, in Crookes's "Handbook of Dyeing," page 500, I find:—"It is one of the most important astringents in the market, and it is very largely used in producing a black upon cotton warps and mixed goods."

Again, on same page, I am informed that the reactions of myrabolans are similar to those of sumach, but of less value. On comparing page 125 of Slater's "Manual of Dye Wares" I read:—"Being much cheaper than gall, and stronger than sumach, they are rapidly superseding both these wares, except in a few special cases." This manual is dated 1870. On comparing Crookes's "Handbook of Dyeing," dated 1874, I read:—"Myrabolans being cheaper than gall, and stronger than sumach, have to a very great extent superseded both." Now what am I to understand by these statements. I seldom get out of my native town, to the newest books I therefore look for information, and in this case I find it in direct contradiction to what was published in 1870 and in 1874, and exactly like what was published in 1853, over twenty years ago. Now, as I have to work hard for my guineas, I protest against such things being palmed off under the guise of "revised and re-written."

Page 229 commences an article on indigo. This extends over about forty pages, and the only new matter that I find in the whole article is a part of page 268, and this is mostly relating to the zinc-vat, which the author says was introduced on the Continent a few years ago. I suppose he is here alluding to the patent of MM. Cohen; if so, however, the difference of the two receipts given is rather singular. In an article in the *Moniteur de la Teinture* upon that method I find Cohen gives the following as fair working proportions, viz.:—Zinc powder, 1·4 parts; quick-lime, 6 parts; and indigo, 1 part. In Napier's "Manual" I find the following:—Iron filings, 30 parts; zinc, 75 parts; slaked lime, 56; and indigo, 100, to set a vat of ordinary size. Just fancy 100 lbs. of indigo for setting a vat of ordinary size. I note also Cohen gives 6 lime to 1 indigo; the "Manual" gives 56 lime to 100 indigo, rather over ½ per cent lime to 1 indigo. There is not one word said of the new method of Schützenberger, and Lalande's process now in use both in England and on the Continent, which surely is a very grave omission in a work that professes to be eminently practical. Would it not have been better to have left out some of the old matter and put in some account of modern proceedings.

Treating upon OH₂ (page 350), I am informed that the amount of hardness of water may be known by the milkiness given with soap. Query, How much milkiness equals, say, 10 degrees of hardness? Seeing that the author professes to teach us chemistry, he might have explained

Clarke's process, water being of so much importance to us.

On page 354 it is stated—Yellow boiled with soap, if fustic be the colouring matter, it is removed. On the same page, a little further down, I find as follows:—"One portion is treated with boiling soap solution. The colour becomes very dark; it is fustic." No explanation as to whether they are on different fibres or not. Which must I believe? they are directly opposite to each other.

Again, on reds by Brazil woods, by which I suppose is meant the red-wood generally, the following is given:—"Reds by Brazil woods are mostly removed by a sour made with vitriol." Further on I find:—"Treated with sulphuric acid they become cherry-red." So vitriol *removes* Brazil-wood reds, and sulphuric acid *makes them cherry-red*. No mention is made if on different fibre, so it must be as read, only which is the right one?

The whole of the receipts, with very few exceptions, are exact reprints from the old edition; and so far as the woollen portion of them goes, very many, indeed most of them, are not worth the paper they are written on for a practical dyer at this day. Receipt No. 221: we are told to take one pound cochineal paste, No. 49, to dye crimson. On looking at No. 49 I find a method of preparing safflower for dyeing cotton; and I would ask how many dyers use sumach for dyeing scarlet; as is given in 223? In 244 we are instructed to boil in soda-ash. Does Napier know the effects of boiling ash on wool? And, again, what dyer would dye a union piece as No. 211 and then 72, and if he did what sort of black would he get? Does the author know the effects of lime-water upon wool?

Taking the book as a whole it is very unsatisfactory indeed. In fact, the whole of the matter, or nearly so, which is of any value to the dyer is contained in the old edition. There is not, so far as I have seen, a single new method for the estimation of the dye wares, leaving us to infer either that there was nothing new in that line, or else that the old methods are still the best.

The preface to the present edition commences as follows:—"Since the time when the Manual was first published many and important changes have taken place both inside and outside the dye-house." Yet throughout the whole work there is not one new engraving; those for chlorimetry, alkalimetry, and all others, are exactly the same as in the old edition, and I am sure there are apparatus much better adapted for that kind of work than those sketched in this work; but they are not even mentioned, much less engraved—a very important omission on the part of an author, the chief merit of whose work consists in his supposed practical knowledge of the wants of the dyer: I find also many repetitions. For instance, on p. 158, I find under the head "Tin" a number of receipts for making spirits; same again at page 197; and then again on pages 345-6. Speaking of iron (pp. 129 to 139) there is the method given of making all the common salts of iron. They are again given on pages 342 and 343. It is the same with copper and alumina salts, and again with the preparations of indigo. Now, why there should be all this repetition I cannot tell; I am sure there is much valuable matter omitted which might have occupied the space thus taken up. Had the book been advertised simply as another edition of the old Manual, without any revision, those who had the old one would have known; but to be advertised as revised and re-written, and then to find it is little more than a reprint, does not exactly suit.—I am, &c.,

J. B. WILKINSON.

SODA TESTS.

To the Editor of the Chemical News.

SIR,—Permit us to avail ourselves of Mr. Hall's courteous mention of our name in the CHEMICAL NEWS (vol. xxxiii., page 8) to subjoin a few remarks on our method of testing soda-ash and reporting the results, for the information

both of producers and consumers of alkali, as we find many erroneous reports and beliefs are in circulation on this subject.

In the first place, we still retain 32 as the commercial equivalent of soda, in accordance with the custom of trade, and also in accordance with our custom since soda-ash was sold by test in London; we report the first place of decimals so far as 0.60; but should the sample exceed this we put it up to the full unit. Thus, with a sample of 50 to 51 per cent alkali which tests 50.10 to 50.60 per cent, we return the actual figures, and the buyer pays on 50 per cent; but when it tests 50.70 to 50.90 per cent we return such sample as 51 per cent, on which the buyer pays.

As the decimals of the unit are not recognised by the trade, the above old customary practice is a sound and good one, so long as this practice is retained, in preventing the advantage being always on the side of the buyer, who nevertheless retains, to a slight extent, the sound side by "the turn of the scale."

The foregoing statements may well evoke the questions, Why retain a standard, 32 for soda, which is well known and admitted to be in error? and, Why not buy and sell ash and caustic soda on a basis of units and tenths? as samples may now be easily and accurately tested within this limit. These questions are, however, trade questions, to be considered and determined by the producers and consumers as may seem good to them, for it is rightly their province to decide upon a change of standard and trade customs; but we may be permitted to add that should the trade think that the time has now come to rectify the standard, and deem it advisable to settle their accounts on the basis of units and tenths of an unit, and formally communicate this decision to us, we, for our part, shall most gladly obey such instructions, and from any given date issue our Alkali Certificates on the basis of 31=soda, and the determination of strength to one-tenth, *i.e.*, from 0.10 to 0.90. Neither buyer nor seller can or will be damnified by this change beyond the one-tenth, the turn of the scale the buyer now gets if a sample tests 0.60, as, all other conditions remaining the same, the buyer will obtain precisely the same amount of soda as before, only where he bought and paid for 100 lbs. soda he will buy and pay for 97 lbs. (96.57), and the seller will sell 97 lbs. soda where formerly he sold and delivered 100 lbs. of soda. But whilst no damage can accrue to either of the interested parties, we venture to maintain that much advantage would be gained by the assimilation of the commercial to the true and real standard for soda, if only by putting an end to all doubts and puzzlings on this subject, and closing the door to fraud, which will always exist whilst two standards are recognised.

A few words more as to the method of testing. Your correspondent, of the East London Soap Works, evidently credits us with not filtering our solution of soda-ash, a practice which we are aware obtains in some quarters, but one which he will take the writers' assurance has never been followed in this laboratory for more than a quarter of a century, nor he believes long previously, in the time of barilla, when soda-ash was almost unknown. A solution of the carefully-prepared sample is made in hot distilled water, filtered, and the filter thoroughly washed; the filtrate is then tested with weighed test acid, the volumetric mode having been disused for obvious reasons long before the time the writer speaks of. And this test having been confirmed by a re-test of a second portion, and we thus being sure of our correctness, the result is calculated, and our certificate of the sample issued, all of which he might have ascertained from us on 'Change or Mincing Lane, as we have never made any secret of our method. Moreover, if he will be so good as to calculate his own and our tests of the three samples of ash he quotes by the invoice standards of 32 and 31, he will find that whatever the producer may have to say he, as the consumer, has no ground of complaint, seeing that his test of 52.50 per cent (we have averaged the three samples), at soda = 32 instead of 31, amounts to 54.20 per cent of alkali against our average

of 53.30 per cent; which last, if we rectify by soda = 31 instead of 32, amounts to but 51.60 per cent of alkali; so that, instead of a loss, we must congratulate him upon a positive seeming gain, in relying on our tests rather than his own as the basis of settlement.—We are, &c.,

E. F. TESCHEMACHER and J. DENHAM SMITH.

1, Highbury Park North, N.,
January 12, 1876.

SODA TESTS.

To the Editor of the Chemical News.

SIR,—I have read with interest the correspondence on soda tests, and think the consumers of soda-ash and caustic soda should stir themselves and get an end put to the present unsatisfactory system of testing. I think the Public Analysts have no business in reporting a sample higher or lower than it really is, but should report exactly what it tests. The system of using a commercial in place of the proper chemical equivalent for soda is a source of differences between chemists which should no longer be tolerated. Such a thing as being able to buy by London, Glasgow, or Newcastle tests, and sell at a profit by Liverpool test, as stated in Mr. Morrison's letter (*CHEMICAL NEWS*, vol. xxxiii., page 8), is a disgrace to the profession. If buyers would take the matter up in earnest, and insist on the test being based on the proper chemical equivalent, we should soon hear the last of high and low tests in soda.—I am, &c.,

Z.

ULTRAMARINE—ITS FORMATION DURING THE INCINERATION OF BREAD.

To the Editor of the Chemical News.

SIR,—Under the heading "Copper in Bread," in the *CHEMICAL NEWS* (vol. xxxiii., page 7), Dr. R. J. Atcherley merely shows that he has misread Dr. Edmunds's note as to the blue colouration which is described as sometimes occurring during the incineration of bread. In fact, Dr. Atcherley somewhat vigorously assails three propositions, neither of which was before your readers. A few weeks since Dr. Edmunds showed me some of the blue masses to which he refers: they were certainly very remarkable, and such as I had not before seen. On viewing them through the side of the tube containing them they were very suggestive of copper, of which, however, I understood that not a trace was present. I had no opportunity for further examination of the blue mass, and I was unable to offer any suggestion as to the probable composition. Should they prove to be ultramarine, the fact would be of interest, but doubtless their precise nature will be decided by some of your correspondents.—I am, &c.,

CHARLES H. PIESSE.

303, Strand, London, January 11, 1876.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Seances de l'Academie des Sciences. No. 25, December 20, 1875.

Formula for the Quantity of Magnetism Taken Away from a Magnet by the Contact of Iron, and for the Supporting Force.—M. J. Jamin.—This paper does not admit of abstraction. The author concludes that the supporting power, in case of a contact sufficiently large to restore the poles to their natural state, is inversely as the adhering surface.

New Researches on the Internal Magnetism of Magnets.—MM. Trève and Durassier.—The authors find that magnetism, far from being confined to the surface, penetrates to the very centre of the steel. They believe themselves authorised in concluding that the fact of the penetration of magnetism into the entire mass of a piece of homogeneous steel, magnetised to saturation, is a general fact; or that the magnetism, at first superficial, penetrates successively into the mass in proportion as the outer layers are dissolved away by an acid.

Researches on Eucalyptus Globulus.—M. F. A. de Hartzen.—Preliminary researches on the essence of *Eucalyptus*.

Action of Mineral Salts on the Crystallisation of Sugar, and Determination of their Coefficient.—M. P. Lagrange.—Contrary to the general opinion, among the salts contained in sugars, the chlorides are the least melassigenous, especially the chloride of sodium. Next follow the sulphates and carbonates, whilst the nitrates of potash and soda exert the most injurious action upon the crystallisation of sugar. If only chlorides and sulphates were present in crude sugars the coefficient 5 would be too high; but seven-tenths of the saline matter consists of nitrates of potash and soda, the respective coefficients of which are 3.5 and 6.5.

Action of Nitric Acid on the Phosphates and the Arseniates of Baryta and Lead.—M. E. Duvillier.—Nitric acid reacting upon the salts named decomposes them, liberating phosphoric and arsenic acids, and forming nitrates of baryta and lead.

Exchanges of Ammonia between Natural Waters and the Atmosphere.—M. Th. Schloesing.—For the same tension of ammonia in the air the amount which dissolves in a natural water, up to the equilibrium of tension, decreases rapidly as the temperature rises. If two vessels of water, the one luke-warm and the other cold, contain each the same proportion of ammonia, the air which rests upon the former is much richer in ammonia than that which covers the latter. It is therefore to be presumed that the atmosphere between the tropics is richer than that of temperate and cold regions. The results furnished by sea-water and distilled water are nearly identical, the tension of the former, for the same percentage of ammonia, being somewhat greater.

Propagation of Heat in Rocks of Schistous Texture. E. Jannettaz.—Not suitable for abstraction.

Aniline-Black, with Reference to the Memoir of M. Coquillion.*—M. A. Rosenstiehl.—In the present state of science, whenever we wish to obtain aniline-black upon any tissue industrially, that is to say, economically and regularly, the simultaneous action of a chlorate, and of a metallic substance, is indispensable. Practice has selected copper for blacks to be developed at about 350° (?), and iron for those which have to be steamed (100°). If industrial conditions are not required, we may obtain aniline-black upon the tissue by the mere use of active oxygen without either chlorate or a metallic compound. In the same manner aniline-black may be produced without the tissue, and without the intervention of a metal, but with the aid of chlorates. The researches of M. Coquillion show that in this case also the same result may be reached without the chlorates. The fact observed by him is an elegant demonstration of the effect of active oxygen upon the salts of aniline.

Action of Ozone upon Animal Matter.—M. A. Boillot.—The author finds that meat may be preserved longer in ozonised air and ozonised oxygen than in ordinary air and oxygen.

TO CORRESPONDENTS.

S. Hall.—Messrs. Teschemacher and Denham Smith's letter renders the publication of yours unnecessary.

* *C. m. t. s. Rendus*, August 30, 1875.

MEETINGS FOR THE WEEK.

- MONDAY, Jan. 24th.—Medical, 8.
— London Institution, 5.
— Royal Geographical, 8.30.
— Society of Arts, 8. Cantor Lectures. "Iron and Steel Manufacture," by W. Mattieu Williams, F.C.S.
- TUESDAY, 25th.—Civil Engineers, 8.
— Royal Institution, 3. "On the Classification of the Vertebrated Animals," by Prof. Garrod.
— Society of Arts, 8. African Section. Opening Meeting.
- WEDNESDAY, Jan. 26th.—Society of Arts, 8. "Iceland, its Scenery and its Rocks," by W. L. Watts (Illustrated by numerous Photographic Transparencies).
- THURSDAY, 27th.—Royal, 8.30.
— Royal Institution, 3. "On the Chemistry of the Non-Metallic Elements," by Prof. Gladstone.
— Philosophical Club, 6.
— London Institution, 7.
- FRIDAY, 28th.—Royal Institution, 9. "Border Territory between Animal and Vegetable Kingdoms," by Professor Huxley.
— Society of Arts, 8. "Industrial Pathology, or the Influence of Certain Injurious Occupations on Health and Life," by Dr. B. W. Richardson, F.R.S.
— Quekett Microscopical Club, 8.
- SATURDAY, 29th.—Royal Institution, 3. "On Excavations in Asia Minor," by R. P. Pullan.
— Physical, 3.

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THE CHEMICAL NEWS.

VOL. XXXII. No. 844.

ON THE SPECTRUM OF GALLIUM.

By M. LECOQ DE BOISBAUDRAN.

I HAVE measured anew the the length of ray-waves of gallium in circumstances of exactitude which the feeble light of the spectrum obtained did not permit me to realise in my first determination. I have found strictly the same number for the principal ray; as for the less brilliant it is a little more refrangible than I estimated it at first. With chloride of gallium, relatively very concentrated, which I have recently submitted to the action of the electric spark, I have observed no other rays than the two following, and if others are found with still more concentrated solutions, they must be very feeble.

Position on the micrometer.— α . 193.72. λ . 417.0. Narrow, strong. Decidedly more brilliant in a spark of mean length than in a very short one.

Position on the micrometer.— β . 208.90. λ . 403.1. Narrow, well marked, but much weaker than α 193.72. Much brighter with a medium than with a short spark. Much more difficult to measure than α , but I do not think that the error of λ much exceeds 0.1. The ray α 417.0 is characteristic of gallium and is a very sensitive reaction.—*Comptes Rendus*.

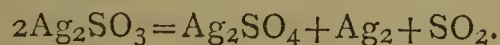
ON THE NATURE AND REACTIONS OF SOME SILVER COMPOUNDS.

By SERGIUS KERN, St. Petersburg.

I. Silver Sulphite (Ag_2SO_3).

THIS salt was obtained in the form of a white precipitate resembling silver chloride, by mixing aqueous solutions of silver nitrate and sulphurous acid. During the preparation of this salt an excess of sulphurous acid must be avoided, because in this case the excess of the acid reduces the silver sulphite. The precipitate obtained is quickly filtered from the solution and dried over sulphuric acid. In order to obtain the silver sulphite in a more stable state I propose to pass through a concentrated solution of silver nitrate in alcohol a current of sulphurous acid; the precipitate of silver sulphite is filtered and dried.

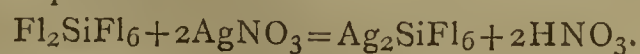
This silver compound is easily decomposed by heat and water, when the latter is for some time in contact with the salt. Alcohol has no action on this salt. At 100° the decomposition of silver sulphite may be expressed by the following formula:—



This reaction gives very pure silver, which may be extracted by the following manner:—The mass obtained from the ignition of Ag_2SO_3 is placed in a test-tube with ammonia gently heated by means of a spirit-lamp. The silver sulphate then dissolves and leaves the metallic silver in the form of a finely divided powder, which, being collected and melted, gives an ingot of very pure metal.

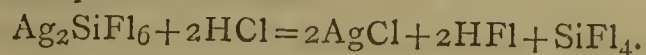
II. Silver Silicofluoride (Ag_2SiF_6).

By mixing aqueous solutions of silver nitrate and hydric silicofluoride this salt is obtained in the form of a greyish white precipitate—



On precipitating the same salt from an alcoholic solution of silver nitrate it was observed that the precipitate of

Ag_2SiF_6 had a darker colour. Hydrochloric acid very easily decomposes this salt—



In the same manner acts sulphuric acid, yielding silver sulphate.

According to Berzelius a strong solution of ammonia decomposes this salt; meanwhile it was observed that a weak solution of ammonia produces an unstable gelatinous precipitate, which hangs a long time in the solution and contains the elements of ammonia.

ON PERBROMIC ACID.

By R. W. EMERSON MACIVOR, M.P.S.L., F.C.S., &c.,
Lecturer on Chemistry, Melbourne, Victoria.

SOME years ago Dr. H. Kaemmerer published a paper in the *Proceedings of the German Chemical Society of Berlin*, in which it is stated that perbromic acid is formed by adding bromine to hydrated perchloric acid. As it appeared, *a priori*, highly improbable that bromine could, under the circumstances specified in the paper referred to, displace chlorine from so stable a combination as perchloric acid, I, at the suggestion of Prof. Dittmar, made the experiments detailed in the present communication.

The perchloric acid employed was prepared by decomposing potassium perchlorate with sulphuric or hydrofluosilicic acid, concentrating by evaporation the solution obtained, and finally distilling it when the oily hydrate, $\text{HOZO}_4 \cdot 2\text{H}_2\text{O}$, was obtained.

30 grms. of this acid were treated, in a glass retort, with 24 grms. pure bromine, which is about one-third more than is required by theory to replace the whole of the chlorine. The retort was heated on a water-bath, and in a short time almost the whole of the free bromine had passed over into the receiver. The remaining traces of dissolved bromine were removed by blowing air through the hot liquid, which then becomes almost perfectly colourless. It was cautiously neutralised with potassium hydrate, when, of course, a dense precipitate of flat white crystals was formed, which was collected, washed with small quantities of cold water, dried, and heated to expel oxygen. The residue, on careful examination, proved to be not bromide, but pure chloride of potassium. The experiment was twice repeated with the same result.

In conclusion, I am, in accordance with what has been stated, compelled to deny the correctness of Dr. Kaemmerer's assertion.

Scientific Chemical Laboratory,
Andersonian University, Glasgow.

ON THE ESTIMATION OF MANGANESE IN IRON AND STEEL.

By SAMUEL PETERS, Laboratory, Bay State Iron Works,
South Boston, United States.

THE following method for the estimation of manganese in iron and steel is not new in principle, but as applied in this form to this purpose it may possibly be of interest to some of the readers of the CHEMICAL NEWS.

Dissolve 0.1 gm. pig-iron or steel in 3 or 4 c.c. nitric acid, about 1.2 sp. gr., and boil gently in a long test-tube (about 8 inches long and $\frac{3}{4}$ inch diameter) for five or ten minutes, or until solution is complete; then add an excess of PbO_2 , say 0.2 or 0.3 gm., and boil again two or three minutes.* Cool the tube and its contents in water. Filter through asbestos, washing out the test-tube and the residue on the filter with distilled water until all the colour has

* It is unnecessary to filter off graphite in pig-iron before boiling with PbO_2 .

been washed through. Transfer to a graduated tube ($\frac{3}{8}$ inch diameter) holding 50 or 60 c.c., graduated in 0.2 c.c., and compare with a standard solution of permanganate, held in a tube for that purpose. The comparison is made in the same manner as that in the Eggertz method when estimating combined carbon in steel, &c. The solution under comparison is then diluted and well mixed with distilled water (by pouring the contents of the graduated tube into a small dish, and then transferring to the tube again), until its colour is exactly of the same intensity as the standard solution. Having attained to this point, the number of c.c. is noted, and the result is obtained by multiplying each c.c. by 0.00001. Each c.c. is equivalent to 0.01 per cent manganese when 0.1 gm. of iron is taken for analysis.

For irons containing 0.10 to 0.35 per cent manganese 0.1 gm. is the proper quantity; but if there be, say, 0.8 to 1.00 per cent, I find it best to take 0.1 gm., and divide the solution (before adding the PbO_2) in four equal parts, and use 0.25 for the estimation, taking another 0.25 for a second estimation. In case of a high percentage, as 1.00 per cent, if 0.1 gm. is taken, I find the results too low, on account of some of the manganese escaping oxidation. This agrees with the observations of others. With an unknown iron one or two trials with 0.1 gm., or half that quantity, will point out the probable amount, and so be a guide for the next trial. If the amount of iron taken does not yield more colour than corresponds to 25 to 35 c.c. of standard hue, it may safely be said that all the manganese is oxidised. I find it well to take this volume as the guide to the quantity of iron to be taken. It is well not to allow the quantity of manganese in the liquor to be tested to exceed 0.4 of a milligram., and certainly not over half a milligram., so far as my experience goes. By taking 0.1 gm. of a speigeisen, containing nearly 12 per cent manganese, and diluting to 50 c.c., and taking 2 c.c., or 0.04, for the estimation of the manganese, very nearly the proper amount of manganese was obtained. This seems to show that if the division of the solution can be accurately made, and the bulk of the coloured liquid can be kept down well, the amount of manganese in speigeisen can be estimated very fairly.

I find that combined carbon in large quantity does not interfere with the accuracy of the method, for a steel containing 2.00 per cent combined carbon, and only 0.80 per cent manganese, was found to give good results by this method.

The standard is made by diluting a permanganate of potash solution of known strength until each c.c. = 0.00001 gm. manganese.

For example, a $\frac{n}{10}$ solution will contain 3.16 gm. permanganate in 1000 c.c., or 0.0011 gm. manganese per c.c.; if this be diluted 110 times it will give the required strength. The standard is contained in a tube of the same bore as the one used for the analysis, or else the standard is put in the latter one, and a solution of permanganate put into a tube of nearly the same bore, and diluted until it exactly corresponds with the standard solution, when it will serve as a standard.

From a hint received from Mr. Pope, of Massachusetts Institute of Technology, I made a standard with permanganate of potash, adding nitric acid, and when it was diluted to the proper hue, added a small quantity of PbO_2 , and when the powder had all settled it was used as a standard, and found to keep well for some time; but it appears, from my experience, that this solution decomposes sooner in some cases than others.

I find that *permanganic acid* of the proper hue keeps better than permanganate of potash of the same hue, and is of course easily made by adding nitric acid to the latter.

The time occupied in obtaining a result by this method is very short (about half an hour), and it appears to me to be a method that will prove of advantage in analysing steel made in Bessemer and Siemens-Martin processes.

December 31, 1875.

ON THE
BLUE COLOUR SOMETIMES PRODUCED IN THE
INCINERATION OF BREAD.

By Major ROSS, late R.A.

DR. EDMUNDS showed me this interesting substance, procured from a muffle in the prosecution of his analytical duties, and asked me to what I thought the blue colour attributable. I said it *might* be due to copper oxide, for "every chemist" who has used a blowpipe knows that, with silica and soda present, copper oxide will afford "at a bright red heat," with a supply of oxygen, a blue glass. The fragments shown me had not, indeed, a vitreous appearance, but an infinitesimal number of very minute blue beads would probably communicate a blue colour to an otherwise white and non-vitreous ash. A few fragments which I took home with me gave the following results pyrologically:—

(1.) A pin's-head fragment of a brilliant blue colour was taken up at the bottom of a hot *boric acid* bead, and the fragment treated with O.P.

(a.) It changed to a dirty green colour, glowed about equal to lime, and was then taken up by the bead with a little effervescence.

(b.) Microscopically, the bead presented the following appearance:—It contained a rounded, semi-transparent, colourless, siliceous-looking mass, fast dissolving in the bead, but evidently first attempting to form a ball; this showed that, although lime or one of the alkaline earths was probably present, there was so much alkali also present that a ball could not be formed; and, indeed, after further O.P., the mass shortly dissolved transparently in the bead.

(c.) The boric acid's green pyrochrome was completely *yellowed*; showing that the alkali was chiefly *soda*, derived probably from the salt used in baking.

(2.) Another blue pin's-head fragment of the ash, heated with pure sodium carbonate on aluminium plate, in a *candle* H.P. (for all *coal-gas* contains sulphur compounds), afforded a *salmon*-coloured ball; which showed that a sulphide was, or sulphides were, present in the fragment.

(3.) I now crushed another blue fragment with a drop of distilled water and a few crystals of pure *boric acid* to a fine paste between agates, and boiled this paste with distilled water in a capsule in order to get rid of the soda, and part of the silica (which latter separates in solution from silicates by this treatment), from the assay.

(a.) The residue, obtained by decanting as much as possible of the solution, and tilting the washed remainder on a white blotting-pad, had now a slightly earthy appearance.

(4.) The residue (3 a.), properly dried and collected, was now attached to the bottom of a boric acid bead, and treated with O.P. as before, when it at once formed a *semi-opaque ball* (apparently) of calcium-phospho-borate,* which was yellow, hot, and green-yellow cold, from the presence of an extremely minute proportion of iron protoxide, which, in larger proportion, would have coloured the ball yellow-green. No sign of the presence of any *other* malleable metallic oxide was given by the boric acid.

(A.) It was thus evident from (1 b. c.) that *sodium*, present as the base of a sulphide (2), and also, perhaps, from the effervescence as sulphate (1 a.) was there, and from (4) that there was no copper (which would have formed black balls), manganese (clear brown balls), or other colouring oxide present, except that of iron, which certainly was so, but in far too trifling proportion to afford such a colour.

(B.) The change of the blue colour to green (1 a.) in O.P. is afforded by *Lapis lazuli* powder, which also yields, like this, a colourless icy mass when directly treated in boric

* A calcium-borate ball is transparent and highly refractive.

acid (1 b.) ("Pyrology," page 266). No sufficient evidence has been here elicited of the presence of *alumina*, though operation (3 a.) indicates that, because the object was to determine the cause of colour alone; but it is known ("Pyrology," pages 67, 118) that a blue colour can be obtained by certain treatment, from silica with nitrogen, as found in the Himalayan black quartz, and from pure sulphur dissolved in phosphoric acid.

(C.) Having therefore found, by the above-mentioned operations, the *absence* of any colourising oxide, except a proportion of that of iron, too minute to have such an effect; and the *presence* of the constituents of *Lapis lazuli* (with the exception of *alumina*, which, however, is probably present) I considered—as indeed the primary operation (1 a. b.) alone would have made me suspect—the blue colour to be due to the formation of *ultramarine*, and communicated my opinion to Dr. Edmunds, who informed me that he had himself, independently, arrived at a similar conclusion.

REPORT

ON THE

DEVELOPMENT OF THE CHEMICAL ARTS DURING THE LAST TEN YEARS.*

By Dr. A. W. HOFMANN.

(Continued from p. 25.)

DR. H. MEIDINGER has constructed a simplified machine to which the way has been paved by the observation that a concentrated solution of salt melts ice, producing, if the concentration be preserved, the same low temperature as does the action of solid salt upon ice.† The machine consists of the following three parts:—A cylindrical vessel (cooler) with double sides quite open at the top; secondly, a conical tin vessel (freezer) of about half the diameter of the former, reaching down nearly to its bottom and furnished above with a firmly-connected covering-plate, which rests upon the top of the cylinder and fits it tightly like a lid; lastly, an annular strainer-like vessel (the salt-holder) which is let down into the space between the cylinder and the freezer at about half the depth of the former. The cylinder is charged about half full of pounded ice, upon which is poured a concentrated solution of salt; the strainer filled with salt is then let down, and lastly, the freezer containing the materials for the ice-cream is forced in and is in complete contact with the freezing mixture over its whole surface. The ice melts in the solution of salt, which, as it becomes diluted dissolves more salt from the strainer, and thus remains nearly saturated and capable of undiminished action upon the ice. The reduction of temperature throughout the apparatus is equable, and a mechanical movement of the vessel is not required. The needful agitation of the freezing ice-cream is performed at intervals of five minutes without any disarrangement of the apparatus. The

machine is constructed by Messrs. Beuttenmüller and Co., of Bretten, in an elegant form fit for the table. Recently it has been applied on a larger scale in perfumery for the separation of fatty oils from spirit.

Freezing mixtures in which a fall of temperature is produced by the solution of salts in liquids have been latterly subjected to examination in various quarters, after different small ice machines for domestic use, adapted to their application have been introduced into trade. Dr. Meidinger has drawn up a table of 16 mixtures‡ according to his own experiments. An abstract of this, comprising the most useful mixtures, is given below.

The author remarks in a note that the number 21 for ice and salt signifies the permanent temperature below 0°, which is observed till the whole mixture is melted. The correct number in relation to the other freezing mixtures would be 81°, *i.e.*, the sum of the latent heat of ice 79°, and the dissolving temperature of salt 2.5°. This temperature would be actually observed if a concentrated solution of salt had no freezing-point, when the entire mass of ice and salt would melt at once.

Mixtures of salts yield a far greater decrease of temperature than the salts singly, as they dissolve together in far less water. One part sal-ammoniac dissolves in 3 parts of water and produces a fall of temperature of 19°. Saltpetre dissolves in 6 parts of water and lowers the temperature 11°. Compare with these the fourth and fifth mixture in the foregoing table. The fifth is also especially to be noticed in comparison with the fourth.

The three last columns of the table show the consumption of materials and the cost (retail and wholesale) for 120 heat-units, which suffices, in case of the salt and ice mixture, for the conversion of 1 kilo. of water into ice. The other mixtures only convert about ½ kilo. with the same consumption.

(To be continued.)

A STUDY OF HYDROCARBONS.

NO. I.—THE CONSTITUTION OF ALIZARI.

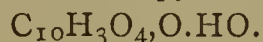
By S. E. PHILLIPS.

(Concluded from p. 27.)

THERE is yet another element of possible variation in these compounds, which may or may not subsist isomerically.

The very numerous mono- and dibasic acids of modern chemistry will very probably resolve themselves into two classes—(1st). The ordinary mono-hydrates, which we have seen in the case of hydrochinon and others, behave generically like alcohol. (2nd). The few terbasic acids which take rank with succinic, tartaric, and others of so-called *diatomic* characters.

Of the former kind we have pyromucic acid—



Of the latter, itaconic acid, $\text{C}_{10}\text{H}_3\text{O}_4, \text{O}.\text{O}.\text{HO}.$

Loss of Heat-units for Quantities to be used for 120° C. Heat-units.

Mixture.	Fall of Temp.	Sp. Heat of Solution.	Sp. Gr. of Solution.	1 kilo. of Mixture.	1 litre of Mixture.	Salts. Kilos.	Water. Kilos.	Cost. \$
1 Salt. 3 Ice.	27°	0.83	1.18	125	100	0.5	1.5	0.34 to 0.12
3 Sulphate of soda crystals. 1 Conc. hydrochloric acid.	37°	0.74	1.31	55	74	2.7	1.8	1.0 to 0.6
2 Nitrate of ammonia. 1 Sal-ammoniac. 3 Water.	30°	0.70	1.20	42	51	3.0	3.0	7.6 to 6.8
3 Sal-ammoniac. 2 Salt-petre. 10 Water	26°	0.76	1.15	40	46	2.1	4.2	2.6 to 2.2
3 Sal-ammoniac. 2 Salt-petre. 4 Sulphate of soda cryst. 9 Water	32°	0.72	1.22	50	61	2.5	2.5	1.8 to 1.6

* "Berichte über die Entwicklung der Chemischen Industrie Während des Letzten Jahrzehends."

† Meidinger, *Bad. Gew.*, 1872, Beil. No. 6. *Dingl. Pol. J.*, cciv., 409.

‡ Meidinger, *Bad. Gewerbz.*, 1858, 98.

§ The cost is given in decimals of a shilling, assuming the shilling to be approximately equal to the German "mark."

MYRISTYL, $(C_{28}H_{29})H$. $(C_{28}H_{27}O_2)O.HO$, Myristic acid. $C_{28}H_{13}H$, Dibenzyl. $(C_{28}H_{13}O_2)O.HO$, Ethylen diphenol.Sulpho-acids, $HO.S_2O_5.C_{28}H_{13}.O.S_2O_5$. $HO.S_2O_5.C_{28}H_{13}O_4.O.S_2O_5$. $C_{28}H_{13}O_4.O.HO$, Carminic acid. $C_{28}H_{11}H$ (?) $C_{28}H_{11}O.HO$, Methyl-benzophenon. $C_{28}H_{11}O_2.O.HO$, Benzyl-benzoic acid. $C_{28}H_{11}O_4.O.HO$, Benzydril-benzoic acid.

Diphenyl-glycollic acid.

 $C_{28}H_{11}O_{10}.O.HO$, Quinic acid. $C_{28}H_9H$, Anthracen. $C_{28}H_9O.HO$, Anthracen aldehyd.Sulpho- and carbo-acids, $HO.CO_2.C_{28}H_9.O.CO_2$. $HO.SO_2.C_{28}H_9.O.SO_2$. $HO.S_2O_5.C_{28}H_9.O.S_2O_5$. $C_{28}H_9O_4.O.HO$, Benzoyl-benzoic acid. $HO.S_2O_5.C_{28}H_9O_4.O.S_2O_5$. $C_{28}H_9O_6.O.HO$, Chrysophanic acid. $C_{28}H_9O_8.O.HO$, Frangulinic acid. $C_{28}H_7H$ (?) $C_{28}H_7O.HO$, Anthracen aldehyd. $C_{28}H_7O_2.O.HO$, Anthraquinon. $C_{28}H_7O_4.O.HO$, Oxy-anthraquinon. $C_{28}H_7O_6.O.HO$, Alizarin. $C_{28}H_7O_8.O.HO$, Purpurin. $C_{28}H_7O_{10}.O.HO$, Rufopin. $HO.SO_2.C_{28}H_7O_2.O.SO_2$. $HO.S_2O_5.C_{28}H_7O_2.O.S_2O_5$. $HO.S_2O_5.C_{28}H_7O_4.O.S_2O_5$ &c. $C_{28}H_{10}O_4$, Diphenic acid.A strong bibasic acid, $C_{28}H_7O_4.O.3HO$ (?) $C_{28}H_{10}O_{24}$, Jervic acid.A tetra-basic acid, $C_{28}H_5O_{18}.O.5HO$ (?)

Or we may compare them isomerically:—

Of the former, piperonylic acid is

 $C_{16}H_5O_6.O.HO = C_{16}H_6O_8$ Of the latter, phthalic acid is $C_{16}H_3O_4.O.3HO = C_{16}H_6O_8$

It is instructive to trace these in their ammoniacal and sulpho-acid derivatives. The former are indistinguishable except in isomeric property; the latter are distinct both in type and individual characters. The pyromucic or itaconic amides are $(C_{10}H_3O_4)H_2N$. The sulpho-acids are mono-basic in one case and tribasic in the other.

There are two very characteristic features of present hypothesis in chemical philosophy. One is the predominant character of the benzol nucleus; the other is the special character of quinon production in the sense we have referred to. And I now submit that both look very small in the light of those wider analogies I have so faintly glanced at. With limited means, and from an amateur point of view, I have done very little in this wide field; but it may be well to indicate the direction and character of research.

In a rough, tabular study of these bodies we have as one of the points of view a generic outline of acid genesis. In this rough book one page is devoted to each group or series, and the headings are based on the ratios of C and H, thus— C_4H_5 , C_4H_3 , C_4H_1 , C_6H_1 , C_8H_1 , $C_{10}H_1$, $C_{14}H_1$, $C_{16}H_1$, $C_{18}H_1$, $C_{20}H_1$, beyond which, as the ratio is more unusual, several are comprised under one page, up to the extreme ratio of $C_{80}H_1$!

A condensed oxy-benzoic acid being the only representative as yet of that ratio it is—

Acid + 7Acid - $14HO = C_{112}H_{33}O_{32}.O.HO$.

At the onset five pages are devoted to the predominant ratio of C_4H_5 , and these are in three columns each.

Meth-alcohol .. $C_2H_3O.HO$
ALCOHOL $C_4H_5O.HO$
Propyl alcohol .. $C_6H_7O.HO$

Formic acid .. $C_2H_1O_2.O.HO$
Acetic acid .. $C_4H_3O_2.O.HO$
Propionic acid.. $C_6H_5O_2.O.HO$

Glyoxalic acid .. $C_4H_1O_4.O.HO$
Pyruvic acid .. $C_6H_3O_4.O.HO$
&c.

Column 1 extends progressively up to melissic alcohol, $C_{60}H_{61}.O.HO$. Column 2 comprises the fatty acids. Column 3 extends through rocellic acid, japinolic acid, and others. The principle herein is that the same formula and character of generic reaction by alcohols give fatty acids: so these similarly give Column 3.

As page 1 may be called the alcohol group, so page 2 may be the glycol group.

GLYCOL $C_4H_5O_2.O.HO$
Propyl glycol.. $C_6H_7O_2.O.HO$

Glycollic acid.. $C_4H_3O_4.O.HO$ (?)
Lactic acid .. $C_6H_5O_4.O.HO$ (?)

(?) .. $C_4H_1O_6.O.HO$
(?) .. $C_8H_3O_6.O.HO$

Page 3 is a glycerine group. Page 4 an erythrite series. Page 6 is the aldehyd group.

ALDEHYD $C_4H_3.O.HO$
Allylic aldehyd.. $C_6H_5.O.HO$
Butylic aldehyd $C_8H_7.O.HO$

(?) .. $C_4H_1O_2.O.HO$
Acrylic acid .. $C_6H_3O_2.O.HO$
Crotonic acid .. $C_8H_5O_2.O.HO$

Tetroleic $C_6H_1O_4.O.HO$
(?) $C_8H_3O_4.O.HO$

After this the pages are a more general survey, irrespective of varied oxy-ratios; for instance, page 11, under the heading $C_{10}H_1$, ranges from phonic or collic acid, $C_{12}H_3O_2.O.HO$, up to cholesterin, with $C_{52}H_{43}O.HO$, with many intermediates. Page 13 similarly comprehends the homologues of $C_{14}H_1$, with chelidonic acid, $C_{14}H_1O_8.O.HO$, meconic acid, $C_{14}H_1O_{10}.O.3HO$, through naphthyl hydride and hydrate up to ethyl naphthyl, $C_{24}H_{11}H$.

Another and more general tabular estimate of hydrocarbons proceeds from a different point of view, and has for initial starting points the higher ratios of H, being variously paged from C_2H_3 , C_4H_5 , C_6H_7 , &c., up to $C_{60}H_{61}$, and under these are comprised a condensed picture of the leading types progressing downwards to the lesser ratios of H. I insert the myristyl page as an illustration. (See Table above):

The pervading idea of these tabular studies is the good old motto—"A place for everything, and everything in its place;" but it is extremely difficult to construct any comprehensive outline of materials so infinitely varied.

Enough has, however, been said to show that benzol in its chemical relations is only one of an extended series of homologues, and in the general play of dedoublement or substitution the same features apply more or less to all.

Acetic acid, $C_4H_3O_2.O.HO$.Tri-methacetic acid, $C_4Me_3O_2.O.HO = C_{10}H_{10}O_4$.

The latter was discovered by M. Boutlerow, a solid substance that distils without decomposition and with an odour recalling that of acetic or valeric acid. Now valeric acid is $C_{10}H_{10}O_4$, and the two are therefore isomeric. Yet farther, just as this is a trimethyl acetic acid, so we may have a triphenyl acetic acid, or the H atoms of benzol may be thus replaced in similar ways.

How or why alizaric acid has been considered di-basic involves a very wide and important question, which I am most anxious should be freely and fairly canvassed, and while deferring that for another paper I may conclude this with the assurance, strong almost to certainty, that the phenols, quinons, and alizarin are all simply monohydrates of the kind and character represented in these tabular connections.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, January 20th, 1876.

Professor ODLING, F.R.S., Vice-President, in the Chair.

THE names of the visitors having been announced, and the minutes of the previous meeting read and confirmed, the following names were read for the first time, Messrs. R. G. W. Typke, F. S. Earp, H. Mitford Faber, B.A., Gerrard Ansdell, J. M. H. Munro, W. J. Williams, N. B. Cooke, D. Walker, G. Harrow, J. Parry, W. Ashwell Shentstone, and C. Williams. Messrs. William Henry Watson, Ernest Holbrooke Gaskell, T. Sebastian Davis, Samuel E. Levy, W. Lamond Howie, Martin F. Roberts, George William Wood, James Hargreaves, Henry Wilson Hake, and Henry Glover, after their names had been read for the third time, were balotted for and duly elected.

Dr. ARMSTRONG exhibited a specimen of pure crystallised glycerin from Messrs. Dunn and Co., of Stratford, a portion of a bulk of 40 pounds. The crystallisation had been induced by exposure to the cold of the early part of January combined with the agitation of a journey by rail. Its sp. gr. at its melting-point 60° F., was found by Dr. van Hamel Roos to be 1.261.

The CHAIRMAN said the Fellows were no doubt much interested in seeing such a rare object as glycerin in the crystalline state. A specimen had once before been exhibited at the Society's meetings some years ago by Dr. Gladstone. In both instances the crystals were formed under similar circumstances, exposure to shaking at a low temperature.

Mr. J. WILLIAMS said he had found hydrocyanic acid to be a very delicate test for the purity of glycerin. If the glycerin were perfectly pure, the mixture might be kept for a twelvemonth without change, whilst a slight trace of impurity in the glycerin caused the mixture to assume a yellow tinge in a short time.

A discussion then took place amongst some of the Fellows as to the possibility of fusing, and then re-crystallising the crystalline glycerin, without exposing it to long-continued and violent agitation.

Mr. E. NEISON then read a "Note on Sebate of Cobalt," in which after referring to the discrepancy between his description of cobalt sebate and that subsequently published by Dr. Otto Witt in the Berlin *Berichte* gave the details of the preparation of this salt from pure cobalt carbonate and pure sebacic acid. Although he had made considerable quantities under various conditions, he always obtained either the vivid purple-blue coloured anhydrous salt as described in his former paper, or a rose-coloured hydrated salt which was readily converted into the blue anhydrous salt by drying. This salt is soluble in about 185 parts of water. The author did not obtain in any case the stable rose-coloured compound insoluble in water described by Dr. Witt.

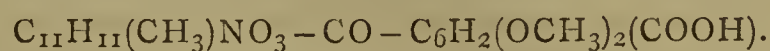
The CHAIRMAN having thanked the author for his examination into the properties of this cobalt salt, Dr. WRIGHT read Part IV. of "Narcotine, Cotarnine, and Hydrocotarnine; On Oxynarcotine, a New Opium Educt, and its Relationship to Narcotine and Narceine," by himself and Mr. G. H. Beckett. The new base oxynarcotine was extracted from the residues of the preparation of narceine by dissolving it with certain precautions in dilute sulphuric acid, re-precipitating with soda, and exhausting the product with successive small quantities of water, which dissolved the narceine and left the sandy crystals of the new base. This was purified by treatment with hot spirit, conversion into the hydrochloride, and re-precipitation by a slight excess of caustic potash. Oxynarcotine crystallises in micaceous sandy crystals whose composition is

represented by the formula $C_{22}H_{23}NO_8$. It forms a hydrochloride, $C_{22}H_{23}NO_8HCl, 2H_2O$.

It having been ascertained that ferric chloride did not oxidise opianic acid to hemipinic acid, the new base was submitted to its action, and the resulting products were found to be hemipinic acid and cotarnine, in accordance with the equation—



Oxynarcotine, therefore, differs from narcotine in containing the carboxyl group instead of the aldehyd group, and may be represented by the formula—



Narceine when submitted to the oxidising action of potassium dichromate and dilute sulphuric acid yielded hemipinic acid together with methylamine; very similar results were obtained with other oxidising agents, from which it would appear probable that the formula for narceine is $C_{13}H_{20}NO_4 - CO - C_6H_2(OCH_3)_2(COOH)$. The action of a dilute solution of potash on narceine seems to give rise to trimethylamine and a kind of weak acid of the formula $C_{23}H_{23}NO_8$. When fused with the hydrate, narceine yields protocatechuic acid.

In reply to a question of the Chairman as to the oxidising action of ferric chloride on narcotine, Dr. WRIGHT explained that it seemed probable that narcotine first split up into opianic acid and hydrocotarnine thus, $C_{22}H_{23}NO_7 + H_2O = C_{10}H_{10}O_5 + C_{12}H_{15}NO_3$, and that by the action of the ferric chloride the latter was oxidised to cotarnine, $C_{12}H_{13}NO_3$.

Mr. DAVID HOWARD said it would be very interesting to ascertain the physiological effect of the new base on animals, as by comparing it with that of narcotine it might throw some light on the question as to where the physiological action lies.

Dr. WRIGHT remarked from the results already obtained it seemed that if two bases were alike, except that one contained more hydrogen than the other, the former was the most active; thus hydrocotarnine was more active than cotarnine.

Dr. ARMSTRONG said the authors had not stated as to whether they had been able to isolate any definite substance from the product obtained on heating oxynarcotine with water. He thought it would give considerable insight into the constitution of narcotine.

Dr. WRIGHT replied that the action of water at 140° to 150° C. gave rise to a brown tarry mass, from which they had been unable to isolate anything except traces of methylamine.

The SECRETARY then read a note "On a Method for Estimating Bismuth Volumetrically," by Mr. M. M. P. MUIR. The author estimates the metal by precipitating a nearly neutral solution of the nitrate by potassium chromate or dichromate in a manner similar to that proposed by Pearson, but he ascertains the critical point by testing the clear solution from time to time with argentic nitrate until red argentic chromate is produced; the chromium solution being previously titrated with a solution of bismuth of known strength. The presence of chlorine, sulphuric acid, calcium, copper, or arsenic interferes seriously with the results obtained by this method.

The CHAIRMAN, in thanking the author, said he thought the directions were somewhat vague as to the strength or degree of concentration of the liquids in which the bismuth was to be determined, a matter of some importance. The quantitative results obtained, however, seemed to be very satisfactory.

Mr. DAVID HOWARD remarked that the great practical difficulty in estimating bismuth was that in most cases it had first to be separated from other metals, as lead, copper, arsenic, and antimony. After it had been separated it was a comparatively simple matter to determine the amount.

The meeting was then adjourned until Thursday

February 3, when papers will be read "On the Formation of the Hydrocarbons of the C_nH_{2n} and C_nH_{2n-2} series, and their Combination with the Haloid Acids and other Compounds," by Dr. H. E. Armstrong; "Note on the Formation of Anthrapurpurin," by Mr. W. H. Perkin, F.R.S.; "On High Melting-Points, with Special Reference to Metallic Salts," by Mr. T. Carnelly; "On Metachromism or Colour Changes," by Mr. W. Ackroyd. The Chairman also announced that on February 17th there would be a lecture by Dr. Frankland, F.R.S., "On some Points in Connection with the Analysis of Water."

NOTICES OF BOOKS.

A Manual of Electro-Metallurgy. By JAMES NAPIER, F.R.S.E., F.C.S., &c. London: C. Griffin and Co.

WE have here before us the fifth edition of a work which has, since its first appearance, been deservedly popular among all who feel an interest in this department of electrical science. Although the wonder with which "galvano-plastic" was greeted at its first appearance has died away, the importance of the art has not lessened. The author, whilst bringing forward the most recent practical improvements, does not overlook theoretical considerations. We quote from his preface the following passage:—"I have, I think, pretty clearly proved that dynamic electricity, at least, is one simple and undecomposable force, by the discovery that when it passes through a fluid in sufficient strength to decompose it the elements of the fluid are not mutually transferred from one electrode to another, which the compound theory would necessitate, and which is asserted by electricians to take place. I have also shown that when the electric force passing through any fluid is too weak to decompose it, there is produced a current of the fluid in one direction only, not in opposite directions, as the two and equally powerful force theory would necessitate. Although upwards of thirty years have elapsed since these facts were made known, subsequent books and lectures on the physical and chemical sciences have continued to uphold the dogma of two electricities in electrolytic action, causing a mutual transfer of elements from pole to pole."

The Textile Colourist: a Journal of Bleaching, Printing, Dyeing, and Finishing Textile Fabrics, and the Manufacture and Application of Colouring Matters. Edited by C. O'NEILL. No. 1, January, 1876. Manchester: Palmer and Howe. London: Simpkin and Marshall.

THE editor of this new journal tells us in his preface that:—"Although some of the existing serials do give a little space to articles connected with textile colouring, this is the first attempt to establish a journal in this country entirely devoted to the subject." This is doubtless technically true. Still it strikes us that Mr. O'Neill somewhat underrated the amount of space devoted to tinctorial questions in our technological literature. However, in so far as this undertaking may aid in the development of the arts of dyeing and calico printing in England, we wish it success. In a paper on "Lime-juice and Argols, Citric and Tartaric Acids," we find the following passage, with which we most cordially agree:—"A large amount of information is acquired in the laboratories of our great manufacturing concerns; most of this might be published without any injury to the individual manufacturer. Especially is this true of analytical methods, and the publication and discussion of these would do much to remove the disgrace to which science is often subjected from the wide discrepancies of commercial analysis."

The Retrospect of Medicine. Edited by W. BRAITHWAITE, M.D., and JAMES BRAITHWAITE, M.D. Vol. lxxii., July—December, 1875. London: Simpkin, Marshall, and Co.

THIS issue, though fully maintaining its value as regards the medical profession, contains very little matter of chemical interest. We extract the following passage on the comparative action of alcohol and beef-extract:—"The experience gained during the Ashantee campaign showed that alcohol, though apparently beneficial when given with food after the day's march was done, was injurious while on the march, the reviving effect passing off after, at the utmost, two and a half miles' march had been accomplished, and being succeeded by languor and exhaustion as great or greater than before. When again resorted to its reviving power was less marked, and its narcotising influence was often traceable in the dulness, unwillingness to march, and loss of cheerfulness of the men. Meat extract, on the contrary, in quantities of not less than half an ounce at a time, was not only powerfully reviving, but sustaining, and so was coffee, though to a considerably less extent."

First Report of the Commissioner of the Imperial Mint, Osaka, Japan, for the Half-year ending 30th of 6th Month of 8th Year of Meiji. Hiogo: Printed at the "Hiogo News" Office.

THIS document bears evidence of the progress of industrial science in the Japanese Empire. A number of the English officials of the Mint have left, their engagements being expired, and have been succeeded by Japanese. Mr. Dillon, however, the assayer and superintendent of the melting department, and Mr. W. Gowland, chemist and metallurgist, remain, and have been appointed "technical advisers" to the Commissioner of the Mint. From the special report of these two gentlemen we gather the following facts:—

The Japanese copper is still found remarkably free from arsenic, and in only one case was antimony found present.

"When a Japanese clay does not already contain quartz or chalcedony in coarse fragments the addition of the latter substance (preferably previously calcined) in sufficient quantity increases enormously the refractory power of the bricks made from it. The large quantities of chalcedony available in Japan will prove of great importance when metallurgical processes are conducted on an extensive scale, and fire-resisting materials are sought after, especially as the natural sands hitherto examined are without exception exceedingly fusible, and quite unfit alone for use where high temperatures are required."

Mention is made of a portion of copper (100 tons) which contained 99.17 per cent of copper, and only very small quantities of iron, lead, and sulphur, but was super-saturated with cupric oxide. Hence the loss on refining amounted to 14 per cent.

CORRESPONDENCE.

ON THE NECESSITY FOR ORGANISATION AMONGST CHEMISTS.

To the Editor of the Chemical News.

SIR,—Owing to unavoidable absence, I did not receive, in time for correction, the proof of the article in the CHEMICAL NEWS (vol. xxxiii., p. 27) "On the Necessity for Organisation amongst Chemists." Permit me, therefore, to take this opportunity of correcting a slight *erratum* on page 28, column 1, line 15:—For "evidence of experts, as such," read "evidence as experts, of such persons."

It was not intended to suggest (as, perhaps, might be

supposed from the text as it stands) that certificates of analysis generally, and the evidence of experts as a class, should be inadmissible in courts of law, but only that the certificates and evidence of such practitioners as those referred to (viz., non-licentiated ones) should not be admitted.—I am, &c.,

CHARLES R. ALDER WRIGHT.

St. Mary's Hospital, Paddington, W.
January 22, 1876.

THE MINERALOGICAL SOCIETY.

To the Editor of the Chemical News.

SIR,—I was glad to see in the CHEMICAL NEWS (vol. xxxiii., page 19) an announcement that it had been arranged to hold the first general meeting of the Mineralogical Society of Great Britain and Ireland, for the election of officers, &c., at the rooms of the Scientific Club, 7, Savile Row, on Tuesday, February 3, and that the chair will be taken at 12 at noon, by Mr. H. C. Sorby, F.R.S.

This announcement is immensely gratifying to me, inasmuch as it was thought by some of my friends that my rough mode of showing, in your columns and elsewhere, the necessity for some such society, would fail in the enlistment of sympathisers.

Happily, it has not so proved, for more than a century of workers are enrolled already; and when I see amongst them such names as Ansted, Church, Crookes, Davies, Geikie, Greg, Haughton, Heddle, Jones, Nicol, and Sorby, I fancy that I see the beginning of the end of "Doubtful Minerals" and "Dual Mineral Nomenclature."—I am, &c.,

T. A. R.

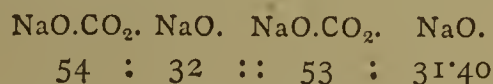
Liverpool, January 24, 1876.

SODA TESTS.

To the Editor of the Chemical News.

SIR,—Mr. John Pattinson's note in the CHEMICAL NEWS (vol. xxxiii., p. 17) reminded us—but too late to correct ours of the 12th inst.—that $31 = \text{soda} = 31.40$ by the commercial test, not 32. We therefore ask your permission to correct our figures, and to read $98\frac{3}{4}$ lbs. (98.72) instead of "97 lbs. (96.57);" 53.20 for " 54.20 per cent of alkali;" and 52.60 for " 51.60 per cent," which last correction involves the cancelling of our concluding sentence.

For the satisfaction of your readers, who may not at a glance discern why $31 \text{ NaO} = 31.40$, the commercial test, which rests on carbonate of soda, a reliable salt, we subjoin the proportion—



which will doubtless make it plain. May we also pray that "writers" be read "writer's," and "invoice" "inverse."—We are, &c.,

E. F. TESCHEMACHER and J. DENHAM SMITH.

1, Highbury Park North, N.,
January 22, 1876.

SODA TESTS.

To the Editor of the Chemical News.

SIR,—Whilst I fully concur in what your correspondent, "Z," has stated to be a disgrace to the profession, I must emphatically contradict Messrs. Teschemacher and Smith that it is a trade question whether the standard, that is the equivalent, for soda is to be taken as 32 or 31. It is a well known fact that manufacturers and merchants do not like to relinquish or alter their customs, unless the reason be so obvious that it needs no discussion. But in this case the necessity for an alteration, though irrefutable among

chemists, does not appear to be as readily acknowledged by the trade, and the question arises—Shall chemists allow laws to be imposed upon them by those who, to a great extent, have no judgment as to the right or wrong of their certificates? or shall these simply be given on the basis of scientific truth? The answer, indeed, cannot be doubtful to all who appreciate the latter, and it is to be regretted that a firm of the reputation of Messrs. Teschemacher and Smith should have given vent to these sentiments. If such publications become known, is it to be wondered at that the respect for chemistry as a profession does not stand higher in this country? The formation of a guild among chemists, as proposed by Dr. Wright, to whom I give my hearty approval, would certainly help to have such controversies speedily settled; and it is to be hoped that the existence of a society of those professional chemists to whom scientific truth is the first incitement in the exercise of their profession, may protect chemists generally from the necessity of forming a league for the defence against trade customs.—I am, &c.,

G. R.

SODA TESTS.

To the Editor of the Chemical News.

SIR,—I can confirm Messrs. Teschemacher and Smith in saying that "many erroneous reports and beliefs are in circulation on this subject," and at the same time free myself from the appearance of having merely carelessly assumed that they did not filter. In conversation on the subject of the recent letters in the CHEMICAL NEWS, a large consumer of ash, of very many years' experience, whom it did not occur to me to doubt, said that whatever the Liverpool people might do Teschemacher used 31. I therefore—perhaps hastily—assumed it might be so, and then it followed, as the only possible way of making my results on that basis agree with theirs, that they must have taken in the residues, and my examples (the only duplicate samples I had left) showed on that assumption, and on that basis, a moderately near agreement.

I am surprised at the results they obtain from my figures, if I understand their meaning correctly. It is, average of my samples 52.50 at $31 = 54.20$ at 32 equivalent, whilst the average of their certificates is only 53.30 , being a gain of 0.9 to us as consumers. The 54.20 appears to have been obtained thus— $31 : 32 :: 52.50 : 54.20$. Surely Messrs. Teschemacher have here made the error pointed out by Mr. Pattinson. The real difference is only 0.77 on 58.49 , as will be seen further on. This correction applied to my figures makes them stand thus:—

	Na ₂ O (Na=23)	Add to	Total.	Teschemacher's
	in Solution.	make Na=24.		Certificate.
I. ..	53.82	0.71	54.53	54.40
II. ..	53.57	0.71	54.28	54.20
III. ..	50.18	0.66	50.84	51.20
Average ..	52.52	0.69	53.21	53.27

Using the same calculation to the records of my analyses for last year, and comparing them with the certificates, I have much pleasure in saying that I perfectly accept Messrs. Teschemacher's statement that they do filter, and will add at the same time that the estimations appear to have been very carefully made.

As regards decimals, Mr. Pattinson says (vol. xxxiii., p. 17), "I have always understood that in the alkali trade no fractions, not even 0.9 , are charged or paid for," and treats it as a kind of set-off against the loss to the buyer by the use of the 32 equivalent.

Messrs. Teschemacher's plan of reporting up to 0.6 , which is not charged on the invoice, and then going up to the next whole number, seems to the uninitiated to do the same thing, but only gives the buyer "the turn of the scale," and the seller all the appearance of generosity, but no compensation to the buyer for the use of the 32 equivalent. Mr. Pattinson's suggestion of reporting all the

decimals as they are found, and letting buyers and sellers settle it themselves, seems much better.

As to the 32 equivalent, Messrs. Teschemacher say it would be desirable, for the sake of preventing both confusion and fraud, if the 31 could be adopted. Trade customs are, without doubt, very difficult to alter. But allow me to suggest a plan, which seems to me easy to carry out, that is, for the analysts to make out their certificates substantially as follows:—

Soda (Na=24)	59.26
Soda (Na=23)	58.49

(I have filled in the theoretical percentages of pure carbonate to show the difference in the use of the two equivalents.) Contracts could then be made on whichever basis buyers and sellers might determine, and perhaps before many years the 32 equivalent would disappear.

Thanking you much for the space you have devoted to this subject, and trusting good results will follow,—I am, &c.,

SAMUEL HALL.

East London Soap Works, Bow, E.,
January 24, 1876.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Seances de l'Academie des Sciences. No. 25, December 20, 1875.

Crystalline Boride of Manganese, and on the Part Played by Manganese in the Metallurgy of Iron.—MM. Troost and P. Hautefeuille.—The effects of manganese in the metallurgy of iron are due to the formation of compounds which are produced with a liberation of heat greater than that arising from the corresponding compounds of iron, and to the easy scorification of these compounds.

Oxyfluorides of Niobium and Tantalum.—M. A. Joly.—Not suitable for abstraction.

Determination of Alkaline Metals in the Silicates, and in Matters not Attacked by Acids, by means of Hydrate of Baryta.—M. A. Terreil.—Reserved for insertion in full.

New Method of Producing Trichloroacetic Acid.—M. A. Clermont.—When the reaction of permanganate of potash upon the hydrate of chloral is at an end, the brown oxide of manganese is separated by filtration over asbestos. Concentrated phosphoric acid is added in excess, and the mixture is distilled. When the thermometer reaches 195°, all that passes over is pure trichloroacetic acid, which, if slowly cooled, crystallises at 44.8°.

No. 1, January 3, 1876.

Internal Constitution of Magnets.—M. J. Jamin.—The author concludes that a bundle of steel plates will always be stronger than a single bar of steel of the same dimensions, and the proportion will increase indefinitely with the number of the plates. This view justifies the use of slender laminæ in the construction of magnets.

New Thermic Researches on the Formation of Organic Compounds: Acetylen.—M. Berthelot.—“The elementary formation of a first fundamental compound, produced—with absorption of heat, and under the influence of an energy foreign to their direct reaction—a compound which subsequently evolves heat whilst forming in a direct manner other compounds, of which it is, properly speaking, the radical; this formation, I say, does not merely characterise the union of carbon and hydrogen: the same pheno-

mena are observed also in the synthesis of many other binary compounds formed in accordance with the law of multiple proportions. Such is, eminently, that of the oxides of nitrogen, all formed with the evolution of heat, if we set out from the binoxide of nitrogen—a fundamental compound whose electro-synthesis, on the contrary, absorbs heat.”

Manner in which Calorific Vibrations may Expand Bodies, and on the Coefficient of Expansion.—M. de St.-Venant.—This paper consists, to a great extent, of mathematical formulæ, and requires the accompanying diagram.

Sixteenth Memoir on the Electro-Conductivity of Imperfect Conductors.—M. Th. du Moncel.—Not suitable for abstraction.

New Crystalline Hydrate of Hydrochloric Acid.—MM. I. Pierre and E. Puchot.—If concentrated commercial hydrochloric acid is submitted to a very low temperature, no portion is separated out in a crystalline form, even if kept for a very long time at from -25° to -30° , but if a continuous current of nearly dry hydrochloric acid gas is passed through it the result is different. When the temperature of the liquid has fallen to -21° or -22° , and is kept there for a few minutes after supersaturation, it rises spontaneously to -18° , although the freezing mixture remained at -25° . From this moment an abundant crystallisation ensues, and the temperature remains without sensible variable variation at -18° . The crystals on exposure to the air are decomposed, giving off dense white fumes. In water they dissolve rapidly. They contain four equivalents of water to one of dry hydrochloric acid. The authors recommend snow and hydrochloric acid as a frigorific mixture, with which they have obtained the temperature of -35° .

New Fundamental Law of Electro-Dynamics.—M. R. Clausius.—A purely mathematical paper.

Phenomena of Induction.—M. Mouton.—Not suitable for abstraction.

Part Played by Acids in Dyeing with Alizarin and its Congeners.—M. A. Rosenstiehl.—The author has shown in a former paper that the best results are obtained in dyeing with alizarin and purpurin by adding to the baths equal equivalents of these substances and of lime in the state of soluble bicarbonate. In continuing his researches he has discovered further facts relating to the chemical function of these tinctorial bodies. If a dye-bath is made up of water containing bicarbonate of lime, as the temperature rises there is produced a chemical action between this salt and the colouring matter, the result of which is the formation of an insoluble lime-lake, which takes no part in the process of dyeing. Comparative trials have shown that in the best conditions the loss is one-fifth of the colouring matter. In the former paper it was shown that carbonic acid rapidly decomposes the alizarin lime-lake; that it acts more slowly upon that of purpurin; but that it, by its presence, very much retards the formation of the latter lake. It results from this observation that the loss may be avoided by passing into the bath a continuous current of carbonic acid. Experiment confirmed this: in presence of carbonic acid the formation of lime lakes is prevented, the bath can be totally exhausted, and the colours obtained are notably more intense. After having proved the good effect of carbonic acid on the small scale, he repeated his experiments on quantities from 100 to 200 times larger, so as to approach the conditions of industrial operations. He dyed pieces of 25 metres in 50 litres of water. The result of these experiments was very different from that obtained on the small scale. No useful effect resulted from the employment of carbonic acid, except the quantity of carbonate of lime was decidedly too large. Hence the author concludes that in operating on the large scale the same losses are not experienced as on the small, although in the two cases the same colouring matters are used in the

same proportions and in identical conditions of temperature. The cause of this remarkable discrepancy lies in the mass of carbonic acid naturally dissolved in the water, and proving an obstacle to the formation of calcareous lakes. It is soon expelled from the small volume of water used in laboratory experiments, whilst a longer time is required for its escape from a large volume of water. Hence the dyeing may be completed, and the bath be exhausted before all the gas has escaped into the air. This explains a fact well known to practical men, the cause of which was unknown: it is impossible to execute several successive dyeings in the same bath, even if it is reset with chalk and tinctorial matter. The carbonic acid has partly escaped during the first operation, and what remains is not sufficient for a new operation. Hence this gas, as naturally dissolved in the water, plays an important part. The author then made a series of experiments with several salts of lime, and found that acetate of lime and acetic acid may advantageously replace the carbonate and carbonic acid. During the operation the mordanted tissue laid hold at once of the lime and of the colouring matter. The acetic acid is set at liberty, and evaporates in the water or accumulates in the bath without at all injuring the saturation of the mordants, which is thus easily effected in an acid medium. He has tried the action of this salt on the various colouring matters of madder, on the extracts, and on artificial alizarin, and found that it precipitates none of them, if we do not overstep the proportion of two equivalents of acetate to one of colouring matter, and take care to acidify the bath from the beginning. The colours come out of this bath perfectly saturated; the bath is perfectly exhausted, and is, after dyeing, more limpid than if carbonic acid had been used. The same beneficial results were observed on the large scale. The advantages of this method are evident: it is no longer necessary to exhaust the bath at once; and we may dye in presence of an excess of colouring matter, working at a lower temperature and in less time.

Phosphates of Sesquioxide of Iron and of Alumina.—M. Millot.—The author describes the following salts:— $2\text{PO}_5, \text{Fe}_2\text{O}_3, 8\text{HO}$; $3\text{PO}_5, 2\text{Fe}_2\text{O}_3, 8\text{HO}$; $\text{PO}_5, \text{Fe}_2\text{O}_3, 4\text{HO}$; $2\text{PO}_5, 3\text{Fe}_2\text{O}_3, 8\text{HO}$; $\text{PO}_5, 2\text{Fe}_2\text{O}_3, 4\text{HO}$; $2\text{PO}_5, \text{Al}_2\text{O}_3, 8\text{HO}$; $3\text{PO}_5, 2\text{Al}_2\text{O}_3, 16\text{HO}$; $\text{PO}_5, \text{Al}_2\text{O}_3, 2\text{HO}$; $2\text{PO}_5, 3\text{Al}_2\text{O}_3, 8\text{HO}$. All these phosphates of iron and alumina if dried at 100° are hygrometric, and re-take moisture from the air. These salts are all insoluble in acetic acid, soluble in ammoniacal citrate of ammonia, oxalate of ammonia, alkaline carbonates, and ammonia. The phosphates of alumina are much more soluble in these reagents than the corresponding salts of iron.

A Secondary Hexylic Alcohol.—M. W. Oechsner de Coninck.—The compound obtained has the composition $\text{C}_6\text{H}_{14}\text{O}$. It is limpid, very mobile, refracts light strongly, possesses an agreeable ethereal odour, and a burning taste.

Assimilability of Fossil Phosphates, and on the Danger of the Exclusive Use of Nitrogenised Manures.—M. A. Roussille.—The use of sulphate of ammonia on soils poor in phosphoric acid in some cases actually diminishes the crop.

Preparation of Gaseous Hydrobromic Acid.—M. A. Bertrand.—The author uses 100 parts bromide of calcium, 50 of water, and 100 of sulphuric acid. The hydrobromic acid is not decomposed, as in case of the alkaline bromides. Or he treats 100 parts of bromide of potassium with 100 parts of syrupy phosphoric acid, mixed with 300 parts of water.

Les Mondes, Revue Hebdomadaire des Sciences,
No. 17, December 23, 1875.

The purple of *Murex trunculus*, one of the molluscs yielding the purple of the Ancients, contains two tinctorial principles, of which one is identical with indigotin. Chemical and spectroscopic analysis shows in *Elysia viridis*,

and in other mollusca, chlorophyll identical with the green colour of plants.

Experiments with the Electric Light.—A few weeks ago a new so-called pyro-electric apparatus was exhibited on the roof of the works of Messrs. Siemens and Halske, of Berlin. The light produced was strong enough to enable ordinary writing to be read at the distance of a mile. A mirror was placed before the apparatus so as to reflect the luminous rays upon the heavens. A luminous train, like the tail of a comet, was thus thrown upon the clouds, in which signals made before the mirror could be distinctly traced. The War Department intends to purchase several of these apparatus for military and naval purposes.

M. Orsat has devised an apparatus for executing gas analyses with rapidity, and with all the exactitude requisite for industrial purposes.

Analysis of the Touchstone.—

Silica	84.40
Alumina	5.25
Oxide of iron	1.15
Lime	0.43
Magnesia	0.13
Potash	0.69
Soda	1.70
Lithia	spectroscopic traces
Phosphoric traces	0.05
Sulphur	0.60
Water	0.70
Organic matter.	{		Nitrogen	0.19
			Hydrogen	0.09
			Carbon	4.37
Loss	0.25
							100.00

No. 18, Dec. 30, 1875, and No. 1, Jan. 6, 1876.

These issues contain no chemical matter.

M. Reimann's Farber Zeitung, No 1, 1876.

A union of German woollen manufacturers is in the course of formation to oppose the importation of foreign, and especially of English, woollens by inducing the Government to raise the scale of duties. Judging from the fate of the iron import duties these gentlemen need not expect much success in their laudable undertaking.

The well known extractor of dye-woods—Dubosc, of Havre—is about to introduce into the market a new extract containing gallic (?) tannic acid prepared from the Brazilian Quebracho wood (*Aspidosperma*). The new extract is to form a substitute for catechu, sumach, &c.

Justus Liebig's Annalen der Chemie,
Band 179, Heft 3.

Nature and Origin of Meteorites.—Dr. Mohr.

Investigations on Isomerism in the Benzoic Series: on the Dichlorobenzoic Acids.—F. Beilstein.—In order to ascertain the influence of pre-existing groups or elements upon a recently-introduced atom of chlorine the author examined the behaviour of para- and orthochlorobenzoic acid in this respect. Parachlorobenzoic acid was converted into ordinary dichlorobenzoic acid, whilst a new acid was obtained from the ortho-acid.

Communications from the Laboratory of Kasan.—These comprise a paper on the serial succession of the apposition, and separation of the elements of hydriodic acid in organic compounds, by Alexander Saytzeff; on amylen bromide and amyl-glycol from diethyl carbinol, by G. Wagner and A. Saytzeff; transformation of diethyl carbinol into methyl-propyl-carbinol, by G. Wagner and A. Saytzeff; on butylen bromide and butyl-glycol from

normal butylic alcohol, by N. Grabowsky and A. Saytzeff; attempt at the production of a secondary alcohol with the ethyl and allyl radicals, by J. Kanonnikoff and A. Saytzeff; on diallyl carbinol, by Michael Saytzeff; on the isomerism of the amylenes obtained from the amylic alcohol of fermentation, by F. Flavitzky.

Relative Constitution of Eugenic Oil.—Dr. Max Wassermann.—A lengthy paper, not adapted for abstraction.

MISCELLANEOUS.

Chemistry Extraordinary.—Our attention has been called to a lengthy review of Mr. Proctor's "Our Place among Infinities" which appears in a recent issue of the *Hour*. In it we find, passing over minor prodigies, the following truly marvellous passage:—"Now, we should like very much to know whether any chemist has ever been able to produce a gas which glows of itself or becomes red-hot. Long before such a stage is reached all known gases *explode*, and then nothing remains but a small particle of dust and a vacuum around it. Science, in fact, teaches us that to obtain a gas we must first possess a substance, and therefore to form an atmosphere we must first obtain a solid earth." Will the writer oblige men of science by exploding nitrogen, or hydrogen, or oxygen by the application of heat far below redness, or indeed at any temperature whatever? Or will he kindly explode any pure gas so that nothing may remain but "a small particle of dust and a vacuum around it?" We should much like to know what kind of man can have gravely and deliberately penned such intolerable nonsense. Did any daily paper commit itself in a similar manner upon questions of history, law, politics, or theology it would be well-nigh laughed out of existence. But physical science is, it appears, so unimportant a matter that there is no necessity to employ competent men for its discussion. That Mr. Proctor's work should be reviewed in an article which teems with errors and gives evidence of ignorance that would be discreditable in a school boy is a disgrace to the age and the nation.

PATENTS.

ABRIDGMENTS OF PROVISIONAL AND COMPLETE SPECIFICATIONS.

Improvements in the recovery of alkali from the liquid in which esparto, wood, straw, or other material has been boiled. D. A. Fyfe and W. H. Bowers, both of Manchester, Lancaster. December 1, 1874.—No. 4112. The inventors inject streams of heated air or hot gases from furnaces into the liquid to be evaporated. They also burn out the carbon in the residuum in chambers, and employ the hot gases issuing therefrom as aforesaid.

Improvements in the mode of and apparatus or machinery for the treatment of fibre and manufacture of pulp, and in the recovery of chemicals used. C. H. Roeckner, Newcastle-on-Tyne, Northumberland. December 1, 1874.—No. 4117. This consists—1. Of a propeller for giving the pulp an upward direction by itself or in combination with an uptake. 2. Placing fibre boilers in diagonal position and rotating same. 3. Removing injurious material by causing same to boil over, or by skimming. 4. Obtaining suction in strainers by oscillating fan; and diagonal blades or a worm are used for propelling knots or other impurities. 5. The chemicals are recovered by using tanks with compartments below one another, liquor flowing from one to the other; when sufficiently evaporated it is drawn off to calciner.

Improvements in the means of and apparatus for clarifying cane-juice, the same being also applicable to the production of bisulphite of lime or other combination of sulphurous gas, or carbonic acid gas, or other gas with an alkali. B. Hunt, Serle Street, Lincoln's Inn, Middlesex. (A communication from J. Girdwood, Kingston, Jamaica.) December 3, 1874.—No. 4153. To carry this invention into effect the air in the juice- or liquor-box is first exhausted by a Gifford's injector, which causes the sulphurous gas produced in a furnace of ordinary construction, and washed, as has been hitherto accomplished, by a falling

shower of water, and then stored in a receiver, to pass therefrom into the box; the cane-juice is then allowed to fall into a trough from a gutter from the mill. The trough causes the juice to spread into a thin stream, so that it falls through the cover of the box into two trays (perforated with diamond holes), equally across its whole width, falling through the holes of the trays in a shower which, owing to the shape of the holes, takes a "fish-tail" shape, widening as the fall, thereby subdividing the particles of liquor more fully than round holes could, and absorbing a larger quantity of sulphurous gas. The cane-juice fallen to the bottom of the box is, by the usual acting of the injector, drawn up by its lower pipe, its upper pipe at the same time drawing from the box a current of the sulphurous gas. At the junction of these two pipes the sulphurous gas is brought again into direct contact with the cane-juice, and passes through the injector along with it, under a considerable pressure, into the pipe leading into the clarifiers; the cane-juice is thus elevated into the clarifiers, and made to absorb more of the sulphurous gas. The entire apparatus can also be used for the production of bisulphite of lime or other combination of sulphurous gas, or carbonic acid gas, or other gas, with an alkali, by using water in which lime or any other alkali has been dissolved in place of the cane-juice.

Improvements in the mode of cleaning cotton-waste, and in utilising the fluids employed therein. A. Ford, Woburn Square, Middlesex. December 3, 1874.—No. 4150. This invention relates to improvements in the mode of cleaning cotton-waste, and in utilising the fluids employed therein, of the character described in the Specification of Letters Patent granted to the said Alfred Ford, dated January 22, 1874, No. 269, and the novelty of the present improvements consists in employing oleaginous substances for the purpose of liquefying the oil contained in dirty cotton-waste, and thereby enabling the same to be effectually pressed out.

Improved means or method of clarifying impure water or sewage, and preparing the precipitate or sludge for re-use. R. Goodall, Armley, near Leeds, York. December 3, 1874.—No. 4158. I take common ashes, or gas-lime, or waste lime, and add thereto sulphuric acid or brown oil of vitriol; I then add water in sufficient quantity to make the mass flow along pipes. When the impure waters contain soapy or unctuous matter, I use slacked lime first, or the following mortar, say. 6 parts of slacked lime and 1 part animal carbon ground together with water, and agitate. I then add a portion of acid mixture as above, and agitate the whole together. To prepare the sludge for re-use I lift it on to strainers, and allow it to remain for a time to thicken, and then mix it about the proportion of one-third sludge to two-thirds newly slacked lime, and grind the mixture, which may then be used to clarify.

Improvements in the manufacture of carbonates of soda. L. Mond, Northwich, Chester. December 4, 1874.—No. 4175. This invention refers to the production of carbonates of soda, by the process known as direct, by ammonia; and consists in a purification of the brine, and in securing the presence of sulphur during the production of the said carbonate. Also in obtaining a more dense monocarbonate.

Improvements in the manufacture of artificial manures. W. H. R. Wise, Duke Street, Adelphi, Westminster. December 4, 1874.—No. 4176. This invention relates to the manufacture of artificial manures from various animal, mineral, and vegetable matters, and consists in the mode of treating such matters, and the proportions and conditions in which they are mixed. The various matters are treated as follows:—Manure is made from blood by mixing it with sulphuric acid and soot or ashes, or both, and then drying the mixture. Manure is made from the other animal matters by treating them in a similar manner after preparing them as follows:—The softer kinds of animal and fish offal are reduced to a pulpy state by mechanical means. The fat and gelatine are separated from bones, and the harder kinds of animal matter by boiling, and the residuum is then boiled under steam pressure. Instead of making the matters above enumerated into separate manures they may be combined to form one manure. In this case the prepared bones are mixed with blood to which sulphuric acid has been added; a quantity of offal is then added, and the mixture is stirred, and sulphuric acid and soot or ashes are added, and the whole is dried.

Improvements in the treatment of hydrocarbons in combination with other substances, and in the manufacture of a hard material therefrom. G. H. Smith, New York, and H. C. Paterson, George Square, Glasgow. December 5, 1874.—No. 4194. This invention relates to the manufacture from the substances hereinafter mentioned of a new material that is suitable for building and other like purposes. A material suitable for blocks and bricks is made from two mixtures. The first contains coal-tar, or other analogous hydrocarbon, mixed with small broken stones or shingle, a portion of which should be pulverised or mixed with sand, so that the interstices between the stones of larger size may be properly filled up. The second mixture is composed of clay and pitch; sand or chalk may be substituted for the clay, and analogous hydrocarbons for the pitch. The first mixture is mixed in a mixing apparatus at a heat which is gradually increased until the product is adhesive to the touch. The second mixture is formed by grinding the powder thus obtained, and is added to the first mixture while its particles are adhesive to the touch. The mixture of the two compounds is confined in a close vessel, and heated so as to diffuse the vapours uniformly throughout the ingredients. In manufacturing a building block, the material having been tested, is removed while hot to moulds, and pressed and shaped as required. A valuable material may be made by an analogous process to that described above, sawdust or vegetable fibres being substituted for shingle, &c. In some cases the sawdust or fibre is mixed directly with the second compound. In this invention the quantity of hydrocarbon is very small compared with the other ingredients, and it is made to diffuse itself over the particles of the clay.

NOTES AND QUERIES.

** Our Notes and Queries column was opened for the purpose of giving and obtaining information likely to be of use to our readers generally. We cannot undertake to let this column be the means of transmitting merely private information, or such trade notices as should legitimately come in the advertising columns.

Cleaning Engravings.—Will you be good enough to tell me, through the medium of the CHEMICAL NEWS, how I can best remove stains from engravings? I have tried a solution of chloride of lime, and succeeded in removing the stains, but could not get rid of the lime. Though I washed the print for six hours there remained, upon drying, a crystalline deposit on the face of the print. If I use a second bath of hyposulphite of soda as an anti-chlor will it not be as difficult to remove it from the fibre of the paper as it is to remove the lime? and would not the crystallisation of the hypo in the body of the paper be just as destructive as that of the lime would be?—PERPLEXED.

MEETINGS FOR THE WEEK.

SATURDAY, Jan 29th.—Physical, 3. "Photography of Fluorescent Substances," by Dr. J. H. Gladstone, F.R.S.

MONDAY, Jan. 31st.—Medical, 8.
London Institution, 5.

TUESDAY, Feb. 1st.—Civil Engineers, 8.
Zoological, 8.30.
Royal Institution, 3. "On the Classification of the Vertebrated Animals," by Prof. Garrod.

WEDNESDAY, 2nd.—Society of Arts, 8.
Geological, 8. "Evidence of a Carnivorous Reptile (Cynodrakon major, Ow.) about the size of a Lion, with Remarks thereon," by Prof. Owen, C.B., F.R.S., F.G.S. "On the Occurrence of the Genus Astrocrinites (Austin) in the Scotch Carboniferous Limestone Series, with the Description of a New Species (A. ? Bennici), and Remarks on the Genus," by R. Etheridge, jun., F.G.S. "On the Genus Merycochærus (Fam. Oreodontidæ), with Descriptions of Two New Species," by G. T. Bettany, B.A., B.Sc.
Microscopical, 8. (Anniversary).
Pharmaceutical, 8.

THURSDAY, 3rd.—Royal, 8.30.
Royal Institution, 3. "On the Chemistry of the Non-Metallic Elements," by Prof. Gladstone.
Royal Society Club, 8.30.
Chemical, 8. "On the Formation of the Hydrocarbons of the C_nH_{2n} and C_nH_{2n-2} Series and their Combination with the Haloid Acids and other Compounds," by H. E. Armstrong. "Note on the Formation of Anthrapurpurin," by W. H. Perkin. "On High Melting-Points. with special reference to Metallic Salts," by T. Carnelley. "On Metachromism, or Colour Changes," by W. Ackroyd.

FRIDAY, 4th.—Royal Institution, 9. "Applications of Electricity to Protect Life on Railways," by Mr. Preece.
Mineralogical (7, Savile Row), 12. "On the Scottish Rhombohedral Carbonates," by Prof. M. Forster Heddle, M.D., F.G.S.E.
Geologist's Association. (Anniversary.)

SATURDAY, 5th.—Royal Institution, 3. "On Excavations in Asia Minor," by R. P. Pullan.

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TO LECTURERS, SCIENTIFIC TEACHERS, AND OTHERS.

MR. J. C. STEVENS has received Instructions from CHARLES HEISCH, Esq., F.C.S., to offer for Sale by Auction (without reserve), at his Great Rooms, 38, King Street, Covent Garden, on Friday, February 4th, at half-past 12 precisely, the Contents of his late Chemical Laboratory in the Middlesex Hospital Medical College, consisting of Chemical and Physical Apparatus, Microscopes, Electrical Apparatus, Chemical Sets, Glass Bottles, &c., and a quantity of Photographic Apparatus, &c.

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THE CHEMICAL NEWS.

VOL. XXXII. No. 845.

THE DETERMINATION OF MANGANESE IN SPEIGELEISEN.

By WILLIAM GALBRAITH.

As is well known to those who frequently have occasion to determine the amount of manganese in speigeleisen, the usual methods are tedious and require great care on the part of the analyst. If sodium acetate is used to separate the iron, the oxide of manganese retains a considerable quantity of soda, which is extremely difficult to get rid of; and, on the other hand, if ammonium acetate is used, the precipitation of the manganese is very slow or it requires a large excess of bromine.

Although the above methods have been in use for a long time no attempt seems to have been made to get a more expeditious one, or at all events none have been successful until Mr. Parry showed that a definite oxide of manganese could be got which enabled him to determine the manganese very accurately and expeditiously. His method is simply to dissolve a weighed quantity of the speigeleisen in nitric acid (sp. gr. 1.20) in a small pear-shaped flask, evaporate to dryness, and heat pretty strongly over a Bunsen burner or spirit-lamp for about ten minutes. He then treats the contents of the flask exactly as a manganese ore, heating with sodium oxalate and hydrochloric acid, and measuring the resulting carbonic acid.

The apparatus he uses (which was devised for the purpose) is, he states, a modification of Schiebler's, but as a matter of fact has many advantages over that apparatus, excellent as it is. One very decided advantage is that it admits of heating the solution, and altogether it would be very valuable in a laboratory, where the accurate estimation of carbonates or the measurement of gases are frequently required.

As everyone, however, has not got the apparatus, I thought it would be advisable to show that the manganese could be determined some other way.

If it is admitted that Mn_2O_3 can be easily formed, a number of methods immediately suggest themselves as being likely to give the amount of manganese, prominent among which is the well-known method of treating with hydrochloric acid, and passing the resulting chlorine through a solution of iodide of potassium, the liberated iodine being titrated with sodium hyposulphite. ("Fresenius," fifth edition, p. 135). Accordingly I tried that method, but although I took every care, and returned to it again and again I completely failed to obtain accurate or even constant results. This seems strange (I may say that Mr. Parry had previously tried and failed also), and at first I attributed my failure to the fact that I was not getting Mn_2O_3 , but was afterwards convinced that it is much easier to get that oxide than at first sight it appears. I can scarcely avoid coming to the conclusion that there is something wrong with this method of determining free chlorine.

The next method I tried proved in every way successful. I proceed exactly as Mr. Parry does (and find no difficulty in getting Mn_2O_3). 1 grm. of the speigeleisen is dissolved in nitric acid (sp. gr. 1.20) in a small round-bottomed flask, and evaporated to dryness. When dry the flame, which may be either a spirit-lamp or a Bunsen burner, is turned so that the bottom of the flask is cherry-red, for ten minutes. It is then allowed to cool very gradually.

At this point instead of forming carbonic acid, I simply put into the flask a weighed quantity of ammonio-ferrous sulphate or ferrous sulphate of a known strength, and

then heat with rather dilute hydrochloric acid. The contents of the flask very soon dissolves, but it is well to keep shaking the solution while it is being heated to prevent loss of chlorine. It only remains now to determine the iron left unoxidised in order to arrive at the quantity of manganese, which can be done of course with potassium bichromate solution. If it is feared that the ferrous solution may get oxidised by exposure to the air, a small piece of marble put into the flask, which can also be fitted with a cork and tube, will readily prevent that.

In four successive experiments I obtained the following results:—

No.	Fe Oxidised.	Equal to Manganese p. c.	By Parry's Method.
1	0.2018	19.82	20.16
2	0.2103	20.65	20.85
3	0.2396	23.53	23.75
4	0.2435	23.88	—

No. 2 gave by the acetate of ammonium method 20.55 per cent, which was done with great care. No. 4 is a repetition of No. 3.

It is evident of course that there is nothing original or new in the above method, but it contrasts very favourably with the usual methods of separating the iron with sodium or ammonium acetate, and precipitating the manganese from the filtrate with bromine. It is not at all troublesome, does not take long, and has the advantage that the only chemicals and apparatus required are those which are necessary for the assay of iron ores.

Ebbw Vale, Mon.

ON SOME NEWLY OBSERVED PROPERTIES POSSESSED BY CERTAIN SALTS OF FULMINIC ACID.*

By EDMUND W. DAVY, M.A., M.D.

Professor of Forensic Medicine, Royal College of Surgeons, Ireland

THE salts of fulminic acid, or the fulminates, have not received the attention which the interest arising from their extraordinary properties would lead us to expect. This is, no doubt, due in a great measure to their being such highly explosive and consequently dangerous compounds, which have already occasioned several serious and even fatal accidents to individuals whilst making them objects of research: they are, therefore, considering the amount of personal danger attendant on a study of their properties, not very inviting subjects of inquiry, and necessitate the exercise of much caution on the part of those engaged in their investigation.

The compound which is known to chemists under the name of fulminic acid, and which is expressed by the empirical formula $H_2C_2N_2O_2$, though it has never yet been isolated or obtained in the free state, is capable, as is well known, of forming a number of simple and compound salts, which are endowed with more or less explosive properties. Of those salts, by far the most important is the fulminate of mercury, which constitutes, as is well known, the active constituent of the percussion caps, and of the detonating matters which are used to fire the charges in our guns and pieces of ordnance; and for those purposes it is now manufactured in large quantities, and forms a very important instrument of modern warfare, since by its employment the use of flint and steel, matches, and other rude means of firing small and large guns have been quite abandoned, at least among all civilised nations.

Whilst making some experiments on the fulminate of mercury, I observed that when that salt and the ferrocyanide of potassium, both in aqueous solution, are gently heated together, the mixture at first acquires a faint reddish yellow tint, which quickly passes into a port-wine or deep purple colour, without the separation apparently, at

* A Paper read before the Royal Irish Academy.

least at first, of any gas or solid matter. The development of this colouration, under the circumstances stated, being considered very singular, and hitherto unnoticed (as far as I have been able to ascertain), led me to study the matter more closely, to determine the nature of this coloured compound, and of the changes taking place in its formation.

On prosecuting this inquiry, I further ascertained that when the purple compound was fully developed, if the heat was continued for some time, or more quickly if the temperature was raised to and maintained at the boiling-point, the purple colour gradually disappeared, the liquid acquiring a light yellow tint, whilst more and more of a reddish brown solid matter (which was ascertained to be the peroxide of iron) was produced; these changes being accompanied by the evolution of more or less of ammonia, and by the mixture, which was at first quite neutral, acquiring a strong alkaline reaction. The solution being filtered, and concentrated by evaporation, was found to yield small prismatic crystals of a colourless or very light yellow salt, which appears to be a double cyanide of potassium and mercury.

Considering that the principal feature of interest in the reaction of the fulminate on the ferrocyanide was the formation of the purple compound, my attention was chiefly directed to its investigation. But I soon ascertained that this compound was a substance of a very unstable character, and that it presented great difficulties in the way of its separation from the matters with which it was associated, as procured in the reaction referred to; and not being able to obtain it in a pure or suitable state to submit it to actual analysis, I was for a considerable time unable to obtain any clue as to its real nature, further than it was some organic compound of iron, in which cyanogen, or at least its elements, were constituents.

At last it occurred to me that the colouration observed might be in some measure connected with the formation of the fulminate of iron; and on making some of that salt, and comparing its reactions with those of the compound referred to, many points of agreement between them were at once perceptible. I may observe that the fulminate of iron is readily obtained by the action of metallic iron on the fulminate of mercury suspended in water. Thus if about equal bulks of the fulminate and of fine iron filings are placed in a small stoppered bottle, which is then filled with distilled water, and being closed is occasionally agitated, the liquid in a short time acquires a yellowish tint, which gradually deepens in colour, whilst the filings become tarnished, and more or less of mercury, in the form of minute globules, make their appearance. After a few hours the decomposition of the fulminate of mercury will be more or less complete, and on filtering the mixture a dull yellow liquid is obtained, which holds the fulminate of iron in solution. This fulminate, as so obtained, was described by my late father amongst several other compounds of fulminic acid, which he was, I believe, the first to discover, during his elaborate researches on that acid. This salt was observed by him to produce, when treated with diluted acids, a fine red or purple colour, which disappeared after some time, evolving hydrocyanic acid amongst other products. He also found that a somewhat similar colour, rapidly changing to a bluish black, with a precipitate of that colour, was developed on heating this fulminate.

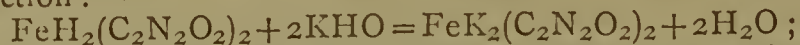
But this development of colour only occurs in the case of the freshly prepared fulminate of iron, for the salt, being one of very great instability, commences almost immediately after its formation to undergo spontaneous changes, which are attended by the separation of a dark brown substance, even when the solution is kept excluded from the air in a well stoppered bottle: and after such changes have taken place it ceases to develop, either by the action of acids or by heat, the colouration just stated. My father also observed that alkalis produced in freshly prepared fulminate of iron a dull green precipitate, quickly changing to a brown colour, which is obviously due to the

separation of iron as an oxide from the fulminate of iron. And I have myself observed that the light yellow liquid which remains after the action of the alkalis and the separation of the oxide at once develops a fine port-wine colour when it is treated with diluted acids, and that the colouration so produced is much more stable than that developed by directly treating the fulminate of iron similarly; and that this red- or purple-coloured compound resembles in all its characters that produced in the new reaction of the ferrocyanide on the fulminate of mercury already referred to.

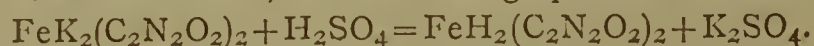
As to the singular development of colour when the fulminate of iron is treated with dilute acids, I am not aware that any explanation has as yet been given; and the one that I would now suggest accounts for its production, not only in the case of the fulminate of iron, but also in the new reactions which I have myself recently observed, and explains some of the properties of this curious purple-coloured compound. To make the explanation I would offer intelligible, I should first observe that fulminic acid is generally regarded as a bibasic acid, which is capable of forming two classes of salts, viz., the neutral and the acid salts. In the first, the two atoms of hydrogen in the hydrated acid ($\text{H}_2\text{C}_2\text{N}_2\text{O}_2$) are replaced either by two atoms of a monad metal, as in the case of the fulminate of silver ($\text{Ag}_2\text{C}_2\text{N}_2\text{O}_2$), or by one atom of a dyad metal, as in the fulminate of mercury ($\text{HgC}_2\text{N}_2\text{O}_2$). In the second class we have either one atom of hydrogen still retained, whilst the other is replaced by a monad metal, as in the case of the acid fulminate of silver ($\text{AgHC}_2\text{N}_2\text{O}_2$), or two atoms of hydrogen are retained (the molecule of fulminic acid being doubled) where a dyad metal occurs, as in the acid fulminate of mercury ($\text{HgH}_2(\text{C}_2\text{N}_2\text{O}_2)_2$). Now as iron in most of its combinations plays the part of a dyad, we should express its neutral fulminate thus, $\text{FeC}_2\text{N}_2\text{O}_2$; and when this salt is treated with a diluted acid there is formed, as I conceive, an acid fulminate of iron (a hitherto undescribed salt) by the following reaction, where, for example, sulphuric acid has been employed,—



and that it is this acid fulminate which possesses the red or purple colour, whilst it is at the same time much more stable or less prone to decompose than the neutral salt. If this acid fulminate is treated with an alkali its purple colour disappears, owing, as I conceive, to the formation of a neutral double fulminate of iron and the metal of the alkali, which is a colourless salt in dilute solution; thus in the case of potash being added to the acid fulminate of iron, there would be a double neutral fulminate of iron and potassium formed, according to the following reaction:—



and this colourless solution being treated with a diluted acid again develops the purple colour by the re-formation of the acid fulminate, as the following equation indicates:—



Or, again, if to some freshly prepared fulminate of iron a dilute solution of caustic potash be carefully added, the mixture will continue (as I have observed) neutral so long as the alkali produces a further precipitate of the oxide of iron; and when it ceases to do so, if the mixture be then filtered, a light yellow solution will be obtained, which holds dissolved, as I conceive, a double neutral fulminate of iron and potassium resulting from the displacement of one-half the iron in the neutral salt, as is shown in the following equation:—



and this double fulminate develops, as before observed, the purple colouration when treated with a diluted acid, and again becomes colourless, or very nearly so, on adding an excess of alkali, especially after the application of heat, and the colour can be again restored by acidifying the mixture, and these changes may be produced many times in succession.

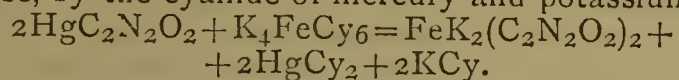
For the production of the acid fulminate of iron the

double neutral salt is much preferable to that of the simple neutral fulminate of that metal, as in the latter case there will be produced, as before shown, a proto-salt of iron, which reacts on the acid fulminate, occasioning its more or less rapid decomposition.

I may further observe that on heating the double fulminate just described there will be developed the red or purple colouration, unless there is present too great an excess of alkali. This remark, however, does not apply to the case of ammonia, the excess of which being expelled by heat does not interfere with its production.

This development of the acid fulminate by heat is not so easily accounted for as where it has been due to the action of acids; it may, however, depend on the circumstance that the simple neutral and double fulminate of iron, are both easily resolvable by heat under certain conditions into the acid fulminate.

I shall now point out how the explanation I have given of the production of the purple colouration in the case of the fulminate of iron may likewise serve to account for the similar development of colour, which I have myself observed, in the reaction of the ferrocyanide of potassium on fulminate of mercury. My experiments would seem to show, that when those compounds react on each other, there is at first formed, amongst other products, the double fulminate of iron and potassium, which, like that salt prepared directly, as already described, from the fulminate of iron, passes into the purple acid fulminate of that metal, on being heated or treated with diluted acids; the following formulæ and equation explaining the production of the double salt, accompanied, as it is in this case, by the cyanide of mercury and potassium:—



Amongst other facts which might be mentioned in support of the foregoing statement is the following one that I have observed, that when the ferrocyanide of potassium and the fulminate of mercury, along with water, react on each other at the ordinary temperature, the mixture after a short time acquires a yellowish tint, which gradually passes into that of a reddish shade; and if a portion of the mixture in this early stage of reaction be treated with a drop or two of any dilute acid, or is heated, the deep purple colouration which results when the double fulminate is similarly acted on will at once be developed. I shall now briefly notice some of the more characteristic properties of the acid fulminate of iron as obtained by the action of the ferrocyanide of potassium on the fulminate of mercury, most of my experiments having been made on that salt as so procured. When that compound is dissolved in water it appears to possess, at the ordinary temperature, considerable stability, for it has been exposed to the air and even light in an open vessel for several weeks without its appearing to undergo any change of colour; but when the solution is allowed, even spontaneously, to evaporate to dryness, the dark purple residue very soon passes to a brown colour, from the decomposition of this salt, and the separation of its iron in the form of peroxide; and this proneness to decompose in the dry condition may account for the residue not exploding on the application of a strong heat, the salt having quietly decomposed before reaching the temperature necessary to explode it or other fulminates; and I may further observe, that even in aqueous solution it soon decomposes if the temperature is raised to the boiling-point, its decomposition being attended with the separation of peroxide of iron and ammonia.

It does not appear to be soluble in ether, chloroform, bisulphide of carbon, or in benzol, though it is readily dissolved by alcohol.

It is quickly decomposed by strong acids, with the evolution of hydrocyanic acid and the development of Prussian blue, and even in their diluted condition the same occurs, but more slowly.

The caustic alkalies, at the ordinary temperature, slowly decolourise its solution; with the assistance, however, of heat that effect is quickly produced.

It appears to possess but little, if any, disposition to assume a crystalline form, for as yet all my attempts to obtain it separately in such a condition have been unsuccessful.

Several experiments were made as to the effects of different metallic salts on this compound, but no very characteristic results were observable, except in the case of the nitrate of silver, which produced a dull bluish precipitate, leaving the liquid colourless if sufficient of the silver salt be added. This precipitate, however, is one of great instability, for it very soon loses its blue colour (even when lying at the bottom of the stratum of liquid from which it has been precipitated), and becomes of a white or yellowish white appearance.

If, however, while it still retains its blue colour, it is treated with diluted hydrochloric acid, or with an alkaline chloride, the solution regains its original purple colour, whilst the chloride of silver precipitates; but if the addition of the acid or chloride be delayed till after the precipitate has become white, then both fail to reproduce the purple colouration, owing to the previous decomposition of the silver compound.

This red or purple combination appearing to be but little affected by many of the metallic salts seems to strengthen the view I have taken as to its nature, for had it been a peculiar cyanogen compound, such as we have in the case of the ferro-, ferri-, and nitro-ferricyanogen, as well as in other compound salt radicals of that substance, we should have expected that it would have produced very characteristic effects with different metallic salts.

I may further observe that the same compound is formed when the ferricyanide of potassium (or as it is better known under the name of red prussiate of potash), instead of the ferrocyanide of potassium (the yellow prussiate), in aqueous solution is heated along with the fulminate of mercury, and that it, as well as the ferrocyanide, even without the application of heat, give rise to, but more slowly, the formation of the red or purple combination, the ferricyanide acting, however, in this respect more readily than the ferrocyanide.

Lastly, I may add that I found that a similar purple compound was produced when the fulminate of silver was substituted for the mercurial salt in the reactions referred to, and that it is probable that some at least of the other fulminates would give rise to like effects.

I regret that the results which I have brought before the Academy are not, in some respects, of a more definite character; but all who have experimented on the fulminates have experienced the great difficulties of such enquiries, arising from their instability and complexity of constitution; but I hope before long to be able to investigate more fully the subjects of this communication, as well as other matters bearing on them. I trust, however, that the results of the observations which I have already made may not be considered as devoid of interest, as any facts which may extend our knowledge of fulminic acid (a compound regarding the true nature of which chemists are not yet agreed) must possess more or less interest in a scientific point of view; and it is well known that many facts and observations, which at first have been regarded as mere matters of interest to men of science, have afterwards proved of much practical utility.

Verification of Instruments.—The Kew Committee of the Royal Society give notice that in order to afford to the public greater facilities for the verification of instruments at Kew than have hitherto existed, they are prepared to undertake the transport of instruments, &c., from London to Kew and back, free of charge. With this object they have made arrangements for Mr. R. Strachan, of the Meteorological Office, 116, Victoria Street, to receive at that office any instruments intended for verification. As soon as the instruments have been verified and are returned to London, notice will be sent from Kew to the parties concerned.

REPORT ON THE DEVELOPMENT OF THE CHEMICAL ARTS DURING THE LAST TEN YEARS.*

By Dr. A. W. HOFMANN.

(Continued from p. 37.)

THE ice and salt mixture, which is added for comparison, will be seen to be much more efficient and much cheaper than all mixtures, if it is intended to use the materials only once. The second mixture, Glauber's salt and hydrochloric acid, cannot be re-used, as likewise the last mixture in the table. These two are still relatively cheap. The mixture, which, by evaporating the solution of the saline residue, can easily be restored to its original state (nitrate of ammonia and sal-ammoniac), requires such a heavy outlay that it would be out of the question if used only once. It was used in an apparatus by S. Charles, which first became known at the Paris Exhibition of 1867. This consisted of a small wooden cask with a perforated lid. The inner vessel containing the articles to be frozen was made of tin, and was fitted with a screw-thread so that when caused to revolve it effectively mixed the salts and the water. Another modification, by Toselli and Co., of Paris, is known as the *glacière italienne roulante*.† It consists of a tall tin capsule in which a conical tin tube is suspended. A good cover connects both vessels, and the internal tube in which are put the materials for the ice-cream is also stoppered separately. The freezing mixture occupies the annular space around the tube. When charged the whole apparatus is wrapped up in cloth and rolled up and down upon a table. In efficiency both forms of apparatus are nearly equal, but the first mentioned is more convenient. Neither has come extensively into use, at least not in places where ice is to be procured. It is necessary to work upon large quantities in obtaining trifling results. 4 kilos. of mixture yield scarcely 1 kilo. of ice-cream of less firmness than that prepared with ice and salt, or a little more than $\frac{1}{2}$ kilo. in summer. Where cold spring-water is not to be had, or where no cold cellar exists the result is altogether doubtful without repeating the operation twice over, the first process being the preparation of cold water, which is too tedious.

The evaporation of the saline solution in order to recover the salt is a task such as does not otherwise occur in the kitchen, and requires some care in its performance. The total of the process is not adapted for domestic arrangements even though the expense of restoring the salts be insignificant.

We have now to examine in how far the solution of salts is available for the production of ice on a commercial scale. This question can be arithmetically answered by means of the figures given in the table. To prepare 1 kilo. ice from water at the mean temperature of 12° C., not much less than 120 heat-units will be consumed if we take losses into consideration. This amount is, indeed, yielded by the mixture of nitrate of ammonia and sal-ammoniac, but little more than the half of this falls below 0° C., since the substances used in the most favourable case will have this initial temperature. The cold still contained in the spent mixture, when no longer applicable for freezing, may, indeed, be transferred to the water used in a fresh mixture, and thus the total cold of solution below 0° may be conceivably utilised, losses being neglected. We require, therefore, 3 kilos. of water for 1 kilo. of ice, and in the regeneration of the materials these 3 kilos. of water must be evaporated by artificial heat. The effect of 1 kilo. of coal burnt under the evaporating pan amounts

to little more than 6 kilos. of steam. Consequently 1 kilo. of coal yields little more than 2 kilos. of ice, overlooking, too, the mechanical power required. This result is very unfavourable since other ice-machines produce a better effect—the ammonia apparatus four to five times greater. Hence no arrangement for the manufacture of ice on the large scale has been constructed on this principle, although it would have a great advantage in simplicity of structure, requiring merely open vessels. Nor can we expect that the circumstances will ever appear more favourable, except salts are discovered which during their solution produce a fall of temperature several times greater than that of the known mixtures. But this is improbable, since all known salts have been examined in this respect. If common salt were so costly a body that its recovery were desirable, not more salt than would serve for 4 kilos. of ice could be recovered by the consumption of 1 kilo. of coal in evaporating the spent mixture.

We may finally mention that in 1869 Rüdorff* examined the fall of temperature to be obtained by the solution of single salts. His table of results contains 20 salts, among which we call attention to two not yet mentioned, as they produce the lowest temperature of any single salt; the sulphocyanides of ammonium and of potassium. 105 parts of the former dissolved in 100 parts of water produce a fall of 31.2°; 130 parts of the latter in 100 parts water lower the temperature 34.5°.

(To be continued.)

BLOWPIPE IMPLEMENTS.

By P. CASAMAJOR.

THE title of this communication obliges me to explain that I have no intention of adding to the formidable list of appliances which dealers include under the name of *complete set* of instruments for blowpipe analysis.

I propose to describe a new form of pocket blowpipe and an easily constructed apparatus for holding charcoal and other supports, and to call attention to supports made of common things that are found everywhere whose use I have had to adopt from time to time as emergencies arose.

Pocket Blowpipe.—In his incomparable book, "On the Use of the Blowpipe in Chemical Analysis and Mineralogical Determinations," Berzelius says that "As it is very desirable, especially for those who study mineralogy, that the blowpipe should take up little room and be convenient to carry without depriving it of its essential qualities, several chemists have endeavoured to attain the last limit of simplicity in the construction of this instrument. The most remarkable blowpipes in this respect are those of Tennant and of Wollaston."

Not being fully satisfied with these, nor with other forms which have come under my notice, I have endeavoured to attain "the last limit of simplicity" in the instrument represented in Figs. 1 and 2. The former represents the blowpipe packed for carrying in the pocket with the jet put away inside of the main tube, while Fig. 2 represents the same instrument with the jet in its place when ready for use. The main tube is a cylinder with an elliptical section, which is preferable to a circular section as the curved jet takes up considerable room in one direction, which would entail the necessity of having a circular tube of about half diameter. This would be clumsy to carry and inconvenient to blow into. A tube, whose section is an ellipse with a major axis of half an inch, may be made quite flat, affording a small package for the pocket and a convenient mouthpiece.

In Fig. 2, representing the blowpipe as mounted for work, I have sketched with dotted lines another blowpipe

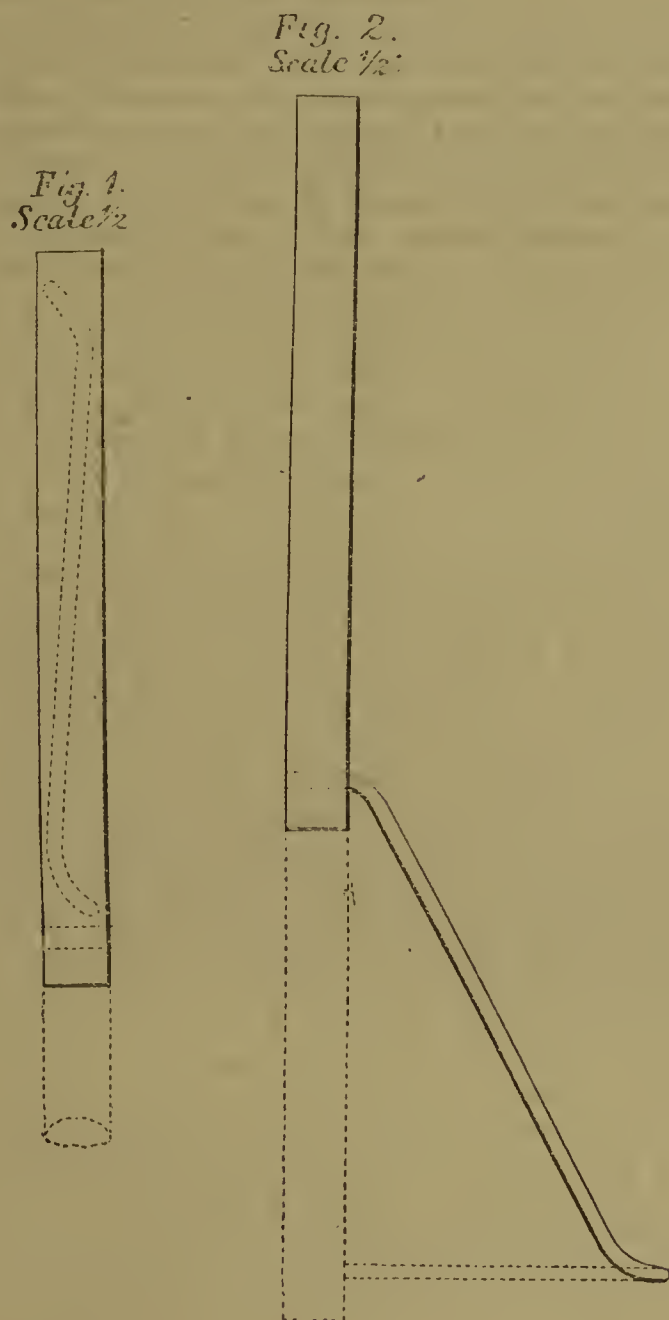
* "Berichte über die Entwicklung der Chemischen Industrie Während des Letzten Jahrzehends."

† *Bad. Gew.*, 1868, 106. *Wagn. Jahresber.*, 1867, 538, and 1868, 605.

* Rüdorff, *Ber. Chem. Ges.*, ii., 68. *Dingl. Pol. J.*, xciv., 57. *Wagn. Jahresber.*, 1869, 508.

with a jet of the usual shape, that the portability of the new blowpipe may be appreciated, while it may be seen that in every essential quality it does not differ from the one with the full length of stem.

For the length of blowpipes Berzelius gives the distance of 8 inches from the mouthpiece to the tip of the jet, adding, however, that for every operator this distance must be such as to afford the clearest view of the substance under examination. A blowpipe similar to the one represented in Figs. 1 and 2, which I have in my possession, and whose length from the mouthpiece to the tip of the jet is 8 inches, has a main tube whose length is only $4\frac{1}{2}$ inches. The relation of $4\frac{1}{2}$ to 8 is a trifle less than that of Dr. Wollaston's blowpipe as given by Berzelius in the two conditions of being packed for the pocket and ready for use. This advantage is obtained by using a curved jet, which also enables the flame to take a more convenient direction than with straight but oblique jet of Dr. Wollaston's blowpipe.



If the stem of the blowpipe is made in two pieces, while the jet is made with a double curve, the length when put up for the pocket can be reduced to $3\frac{1}{4}$ inches, which gives the still smaller relation of $3\frac{1}{4}$ to 8, which I believe has never before been attained. I have one of these dimensions whose stem is formed of two conical portions with circular sections. When packed for carrying, one cone fits within the other, and the curved jet is placed inside of the inner cone. As the stem when developed is longer than when made of only one piece, the jet is shorter and less curved than that represented in Figs. 1 and 2, and it finds ample room in the inner cone. By making the two portions of the stem cylindrical, and screwing them together, a more clumsy and expensive instrument is obtained, but also one whose portions are held more firmly together.

To avoid projections on the surface of the blowpipe,

the socket in which the jet is inserted is formed by a tube soldered at both ends in the interior of the blowpipe. It is indicated by dotted lines in Figs. 1 and 2. On the side of this tube a hole is pierced to allow the air to pass out into the jet.

When the jet is put up in the interior of the stem, it may be wrapped up in a piece of paper to keep it from dropping out. When needed it may be picked out or shaken out.

Mouthpieces.—Extra mouthpieces of horn, wood, or ivory should be avoided, as they invariably fit loosely. Berzelius advises that blowpipes be made either of silver or tin-plate without extra mouthpiece; those made of brass are disagreeable to the mouth and leave a bad smell on the hands. Although silver is a better conductor of heat than any other substance, Berzelius tells us that he was never inconvenienced by the heating of his silver blowpipe even after very prolonged blowing.

Plating brass with either silver or nickel can only be considered as a palliative, as the nobler metal soon wears off. German silver is a very good material for blowpipes, as it is almost free from taste and smell when clean, and is a bad conductor of heat.

For blowpipes made of baser metals, mouthpieces may be made of silver and soldered on. I can propose another material which fits tightly on the stem, which is pleasant to the mouth, and which may be renewed in a few seconds at a trifling expense. This consists of a piece of rubber tubing slipped over the mouth of the blowpipe.

Charcoal Holder.—Charcoal, although one of the most necessary agents in blowpipe analysis, is also one of the most troublesome on account of its bulk, which is disproportionate to that of every other substance employed in determinations of this kind. This defect is aggravated by the circumstance that only a small portion serves as

Fig. 3.
Scale $\frac{1}{2}$

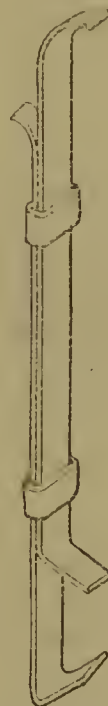


Fig. 4.
actual size.



support, while the greater part is used as a handle to keep the useful portion at a safe distance from the hand of the operator. A consideration of this and a desire of economising a troublesome agent induced me to cut up my charcoal into blocks of various sizes, about as long as they were wide, which I tried to hold before the blowpipe flame by means of tongs and forceps. Finding the use of these inconvenient, I was led to adopt the charcoal holder represented in Figs. 3 and 4, which differs from tongs and forceps in holding the charcoal from behind so that no portion of the holder comes between the charcoal and the flame, and in being able to hold the charcoal firmly without any pressure from the hand of the operator.

This holder is made of metallic strip somewhat less than $\frac{1}{2}$ inch wide, and of sufficient thickness not to bend when in use. German silver is the best material, for

reasons already pointed out. Two strips are cut about inches long, and bent in such a way that each piece has one of the jaws for holding the charcoal, and a projection for varying the distance between the jaws. After giving to each piece its proper shape they are placed in position and strapped together with two pieces of the same metal. These should not be put on too tightly, as a certain freedom should be allowed to regulate the opening between the jaws.

Clay Pipes for Support.—Fragments of a clay pipe bowl and even pieces of pipe stems held before the blow-pipe flame form excellent supports for exhibiting the colours which metallic oxides afford with borax and salt of phosphorus. These fluxes, on being heated, coat the surface of the white clay with a glaze, on which may be seen the colours due to the oxidising and the reducing flames. The flux should first be melted so as to coat the white clay with a transparent colourless glaze. If now we add a substance containing copper and expose the glaze to the oxidising flame, a bright green enamel will be obtained. If now a portion of the edge of the enamel, or some other small part of the glazed surface, is touched with the reducing flame the dark red colour of cuprous oxide will be exhibited by the side of the colour due to the oxidising flame. In the same manner, with manganese, the oxidising flame will give a violet enamel, which may be decolourised in spots by the reduction flame.

The fragments of baked clay may even, to a certain degree, replace charcoal as supports to effect reductions to regulus and to exhibit some of the rings presented by volatile metals. These reactions are best shown by mixing the substance under examination with cream of tartar or one of its substitutes, such as a mixture of carbonate of soda and flour.

After being carbonised by the application of a reduction flame the mass may be triturated with water in an agate mortar. The metallic grains obtained with cream of tartar are even larger and more distinct than those obtained with carbonate of soda on charcoal. The rings exhibited by volatile metals are best seen if they are coloured, as those exhibited by lead and bismuth. White rings are not plainly seen on white clay, but may be exhibited on pieces of red or black terra cotta.

When using pieces of white clay before the blowpipe it is interesting to notice that an oxidising flame leaves the clay white, while a reduction of flame gives immediately a grey tinge which deepens as more carbon is deposited.

I have called attention to these supports as the material is always at hand and forms a great resource to a chemist who may not have either charcoal or platinum wire at hand. The pieces of clay pipe may be held in the charcoal holder already described, or with tongs or forceps, or, at the last resort, by pieces of wire conveniently twisted.

Other Supports.—When chalk is heated before the blowpipe nearly the whole of its carbonic acid is driven off, but the lime that remains keeps the shape of the chalk, and may, on account of its infusibility, be used as support to effect reductions of metallic substances and to hold cupels of bone-ash. These are made by boring cavities in the chalk, which are afterwards filled with bone-ash slightly moistened. The surface of the cupel is formed by pressing on it with the agate pestle. Berzelius recommends that cupels be made in this manner in cavities made in charcoal. I have substituted chalk for charcoal, as the latter burns away too rapidly from under the cupel. If no bone-ash is at hand a slight depression may be made on the surface of the chalk itself, and on this scorifications and cupellations may be carried on, as lime is very porous as well as infusible. As the surface of the lime cracks into little fissures it is better to heat it first and press it down with the agate pestle. Even after this has been done care should be taken to prevent loss of material by keeping a constant watch on the globule of workable lead as it dwindles down.

Another material to form supports for cupels is found in fragments of black-lead crucibles, in which cavities are

easily bored. The charcoal holder before described may serve for these supports of graphite or of chalk.

Brooklyn, January 7, 1876.

PROCEEDINGS OF SOCIETIES.

PHYSICAL SOCIETY.

January 29th, 1876.

Professor GLADSTONE, F.R.S., President, in the Chair.

THE following candidates were elected Members of the Society:—Sir John Conroy, Bart., M.A., and H. T. Burls.

The SECRETARY then read a communication from Mr. J. A. FLEMING, "*On the Polarisation of Electrodes in Water Free from Air.*" The experiments described were undertaken in order to meet objections which had been raised by Prof. Rowland to a previous paper by the author, in which he endeavoured to show that when an electrolyte flows in a very strong magnetic field the electromotive force generated by its motion effects the electrolysis of the liquid—a fact which he holds to be proved by the subsequent polarisation of the electrodes. Prof. Rowland considered that the effect observed was due to the presence of dissolved air, and, conversely, that in air-free water, at any rate with the same electromotive force, similar effects would not be observed. These doubts raise the two questions—(1) In air-free water can platinum electrodes be polarised by a very small electromotive force to the same degree and with the same facility as in aerated water; and (2) is this very feeble polarisation really a decomposition of the electrolyte? To test the first point experiments were made with a voltameter containing dilute sulphuric acid, which had been previously boiled, the voltameter being connected with a Sprengel pump. The platinum plates were acted on by a very small external electromotive force for one minute, and the effect of the polarisation current due to this action noticed on an extremely delicate galvanometer, the effect of the direct current employed being also noted. After a series of observations had been made, using different amounts of electromotive force, the dilute acid was removed, and, after being thoroughly aerated, replaced in the voltameter. On repeating the experiments with this one change in the conditions, the results obtained were almost identical, from which fact the author concludes that the first question may be answered in the affirmative. With regard to the second, Mr. Fleming believes that the assertion that polarisation is decomposition of the electrolyte has never been called in question, and in proof of it describes an experiment, showing that when acidulated water flows rapidly past slightly polarised plates the current which they give is very much diminished, while by causing the water to flow slowly but slight change is produced. This seems to indicate that there is something on the plates which can be *wiped off* mechanically, and it can only be a product of electrolysis.

Prof. FOSTER, while admitting the accuracy of Mr. Fleming's experiments, doubted whether he was justified in definitely ascribing polarisation to chemical action. He thought that, even though the effect be proved not to be due to dissolved air, we must look for some cause other than chemical action; for it has long been acknowledged that the decomposition of water requires an electromotive force considerably in excess of that employed in these experiments.

Prof. GLADSTONE then made a brief communication on "*The Photography of Fluorescent Substances.*" He exhibited several photographs taken of white paper, on which devices had been previously drawn with solutions of sulphate of quinine, œsculine, &c., and one was taken in the room. He remarked that the leaves of trees come out

dark in a negative as they contain the fluorescent substance chlorophyll, and suggested that the irregularities of colour observed in photographs of oil paintings are probably due to the intermixture of fluorescent substances in the paints used.

Mr. MELDOLA referred to Prof. Vogel's experiments on the effect produced on the resulting photograph by the addition of a fluorescent substance to the collodion, thereby increasing the sensitiveness of the plate to particular rays.

Mr. S. P. THOMSON, B.A., B.Sc., then gave a summary of the recent experiments made in America by Mr. T. E. Edison, Dr. Beard, Prof. Houston, and others upon the new phase of electric manifestation—the so-called etheric force. This force is characterised by a faint spark, the only evidence in fact yet known of its existence. It may be obtained from the iron core of any electro-magnet, or from a metallic bar slipped into the coil in place of the core, but only when the battery circuit is being interrupted, as may be done by introducing into the circuit an automatic contact-breaker. The sparks so produced are apparently without polarity; devoid of chemical or physiological effect; affect neither electroscopes nor galvanometers: and are stated to be retro-active, being exhibited when one end of a wire through which they are passing is brought round to touch the wire. A detailed description was then given of experiments on this force conducted in the physical laboratory at South Kensington, some of which were confirmatory of the published researches of the discoverers, while others were at variance with them. Great pains had been taken to avoid leakage, and to distinguish the effects from those of ordinary induced currents. The batteries and coils employed were insulated from the earth as well as from the other portions of the apparatus. A bar of zinc placed above the poles of a powerful electro-magnet, or within its coils, was found to give better results than one of cadmium, which is recommended by the discoverers. The sparks, which resembled those of dynamic electricity, were of inappreciable length, and far too faint to ignite gun-cotton, or illuminate a delicate Geissler's tube. It was also found that when a bar of zinc was placed within the coil of an electro-magnet in the place of its core, and joined by a wire to the gas-fittings of the building, faint but distinct sparks could be drawn from any portion of this wire by a second wire proceeding from another part of the gas-pipes. Another peculiar effect was observed when the wire attached to one end of the zinc bar, and armed at its extremity with a thin iron wire, was rubbed lightly against the other end of the zinc bar, sparks being thus obtained, apparently passing from one pole of the zinc bar, through the wire, to the other.

Dr. STONE believed he had detected a distinct galvanic taste on applying to the tongue the wire through which the "force" was passing.

Prof. FOSTER suggested the use of an electro-dynamo-meter to ascertain the electromotive force of the current exhibiting these sparks, as its indications would be independent of direction of current.

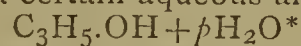
GLASGOW PHILOSOPHICAL SOCIETY:
(CHEMICAL SECTION).

Ordinary Meeting, January 17th, 1876.

Professor JOHN FERGUSON, M.A., President, in the Chair.

PROFESSOR DITTMAR, F.R.S.E., submitted three communications from the chemical laboratory of Anderson's University. The first was by Messrs. Dittmar and D. R. Stewart, and was entitled "*Notes on Aqueous Alcohols.*" In the course of an investigation on the vapour-tension and other physical properties of allyl alcohol and on the propyl alcohol, which they started some time ago, the authors were led to study somewhat minutely the comportment on distillation of aqueous allyl alcohol, and the results they arrived at induced them to extend their experi-

ments to aqueous ethyl and methyl alcohol. The paper laid before the section embraced the results of this collateral inquiry. It dealt first with allyl alcohol, and gave an account of the means taken for the purification of the crude products. 500 grms. of glycerin, 125 grms. of crystallised oxalic acid, 1 grm. of sal-ammoniac, and 1 grm. of common salt were mixed and subjected to slow distillation. The fraction coming over between 195° and 260° was taken as containing the allyl alcohol, and was rendered alkaline with caustic soda and boiled, small additional quantities of the alkali being introduced from time to time so as to maintain an alkaline reaction; and this operation was continued until the mixture remained alkaline after several hours' boiling. The acids originally present as such, or as ethers, having thus been rendered non-volatile, the mixture was distilled, and from the distillate the allyl alcohol separated out by fractional distillation, and treatment of the proper fractions with dry carbonate of potash. The crude alcohol thus obtained is contaminated mainly with water and acroleine. Acid sulphite of soda was used to eliminate the latter, the distillations being continued until the specific gravity of the alcohol remained constant at 0.8576 at 15° (water of 15.5° = 1). The authors persevered with the resulting product, and in the course of their attempts to pass from a nearly anhydrous to really absolute allyl alcohol, they were struck by the obstinacy with which this alcohol retained the last remnants of water, and more still by the observation that the boiling-point rose as the dehydration progressed. The latter observation led to the institution of a series of distillation experiments with synthetically prepared aqueous alcohols of known strengths. (The results of these distillation experiments, and others involved in the paper, were recorded in a series of very elaborate tables, which will be published in the *Proceedings of the Philosophical Society of Glasgow*.) From the numbers given in the first table, it appeared to the authors that there must be a certain aqueous alcohol,—



(where p is an unknown number situated between 1 and $\frac{3}{2}$), which, under ordinary barometric pressure, boils constant at a certain temperature not far removed from 88°; and that all aqueous allyl alcohols behave as if they were mixtures of this pseudo hydrate and water or allyl alcohol. Proceeding to methyl alcohol, the authors eventually obtained a product of the specific gravity of 0.7981 at 15.4° (water of 15.5° = 1), and they felt themselves justified in assuming it to be anhydrous. The numbers given in the table referring to the second series of experiments showed that if there be an aqueous methyl alcohol which boils at a lower temperature than the alcohol itself, its percentage must be very close to 100. The boiling-point of pure methyl alcohol, according to the observations of the authors, is 65.1°, i.e., 0.9° less than the generally adopted value of 66°. Dupré's alcohol must have been largely contaminated with aceton. From the numbers given by the authors in the table referring to their experiments with ethyl alcohol, it appeared that the boiling-point of all aqueous alcohols containing less than 28 per cent of water lies within the narrow range of from 77.4° and 78.0°, and that the minimum corresponds to about 5 per cent of water. This is quite in agreement with an old observation of Sömmerring's,† according to which, when an almost absolute alcohol is distilled, the distillate is weaker, and the residue always stronger, than the original mixture.

The second paper read by Prof. DITTMAR was "*On Perbromic Acid*," by Mr. R. W. E. MACIVOR, a full abstract of which has already appeared in the CHEMICAL NEWS.

The concluding paper was by Prof. DITTMAR himself, its title being "*On a New Method of Assaying Chrome-Iron Ore.*"

* $C_3H_5O \cdot H_2O$ corresponds to 23.7 per cent of water;
 $C_3H_5O \cdot \frac{3}{2}H_2O$ " " 31.8 " "

† This important observation is entirely ignored both by the English and by the second and third editions of the German "*Dictionary of Chemistry.*"

CORRESPONDENCE.

RAPID FILTRATION.

To the Editor of the Chemical News.

SIR,—The problem of effectual and rapid filtration has frequently, and with useful effects, been discussed in your columns. One of the latest suggestions, which attracted attention, was that of Carmichael, who proposes to use, instead of the ordinary conical funnel, one with a bottom almost flat, to which is adapted a piece of perforated platinum foil. On this is placed a *flat* disc of filter-paper, just sufficiently large to cover the bottom of the funnel. The advantages claimed are—that the filtration is conducted more rapidly, that the filter-paper is much smaller, and that the risk of tearing the filter is obviated.

In order to test the relative merits I connected a funnel of each description with a filter-pump which maintained a vacuum of about 5 inches of mercury. The filters used were of Swedish paper, the ordinary one being 5 inches in diameter, that for Carmichael's funnel $1\frac{1}{4}$ inches.

On testing with water only, Carmichael's funnel was the more rapid in its action, 6 ounces of water running through it in a minute, while 2 minutes 20 seconds were required by the other. When, however, precipitates were introduced the advantage in every case was not so marked. When filtering crystalline precipitates, such as ammoniacal magnesic phosphate, &c., the new arrangement was found the more effective, as the filtrate passed through the paper readily. But on the introduction of gelatinous substances, such as ferric hydrate or phosphate, after a very short time the filtration ceased almost entirely. On examination it was found that the precipitate was collected on the surface of the paper in a compact layer, which was almost impervious to the filtrate. The filtration was not helped by a more perfect vacuum, for with one of 12 or 15 inches of mercury the operation was not hastened. In these instances the ordinary filter was found more effectual. This same action, however, seems in other cases to be beneficial, as the precipitate is thus obtained in a closer and denser mass, and is drained almost dry in a few minutes. It was also observed that, in experimenting on finely divided precipitates, such as plumbic chromate, the filtrate passed through clear in a shorter time than usual, the layer of precipitate on the surface of the paper itself probably assisting the more complete filtration. The quantity of washing water necessary is considerably less with Carmichael's filter, as the whole quantity used acts directly on the precipitate itself. This not only effects a saving of time, but in the case of slightly soluble compounds a source of loss is much lessened.

It may, then, be inferred from the preceding observations—

- (1.) That crystalline and granular precipitates filter more rapidly, and can be washed to greater advantage, by using a Carmichael funnel.
- (2.) That bulky, and especially gelatinous, precipitates are filtered more expeditiously through an ordinary funnel, with a platinum cone.

—I am, &c.,

WILLIAM JAGO.

Royal College of Chemistry,
South Kensington, S.W.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Seances de l'Academie des Sciences. No. 2, January 10, 1876.

Spectrum of Gallium.—M. Lecoq de Boisbaudran.—Inserted in full.

Union of the Carbides of Hydrogen with the Hydracids and the Halogens.—M. Berthelot.—The author describes his methods of experimenting, and states the amount of heat evolved.

Causes of Failure in the Detection of Minute Quantities of Iodine.—M. A. Chatin.—The following precautions are necessary in the detection of iodine in a common potable water:—It is needful to precipitate the soluble salts of lime and magnesia with an excess of pure carbonate of potassa. The iodine being thus fixed will be found in the residue after evaporation, which is slightly ignited to destroy organic matter. The liquid must be separated by decantation from the earthy carbonates, which will be deposited during the first quarter of the evaporation. Towards the end of this process the heat must be diminished to avoid any loss of the soluble residue by spiriting. This last point is important, as the iodide is among the last drops evaporated. The excess of carbonate remaining after the precipitation of the calcio-magnesian salts should be the larger the more organic matter is present. We may ascertain that this excess has been sufficient, either by the residue appearing colourless after calcination, or by the circumstance that although coloured it forms a paste if treated with alcohol at 90 per cent. If the alkaline carbonate has been insufficient the residue will be divided in the alcohol, like a powder; the iodine then escapes in great part, or entirely during calcination. The alkaline residue left on the evaporation of the water is repeatedly (generally three times) treated with alcohol, and the solutions are mixed together in a capsule capable of holding at least four times the quantity. Before proceeding to evaporate, which must be done at a low temperature, the alcohol is mixed with about half its volume of pure distilled water. The water should have been distilled after an admixture with carbonate of potassa. Frequent agitation is useful. Slight calcination is again needed to destroy a certain quantity of organic matter which has escaped the former ignition, and the presence of which would mask the characters of traces of iodine. The residue at the bottom of the capsule should be colourless and scarcely perceptible. If it is very appreciable in quantity too much alkaline salt is present, and it must be re-dissolved in alcohol. The last condition is that this residue must be dissolved in a minimum of water, two drops, or even a single drop, which must be led over the bottom of the capsule with a glass rod, so as to dissolve all the iodide present. With the end of the stirrer this liquid is divided into three or four portions, one of which—which will give the most distinct reaction—being left in the bottom of the capsule, the others being placed on fragments of porcelain. One of these little drops is mixed with chloride of palladium; the others, having first received a trace of recent starch-paste, are carefully touched, the one with nitric acid, the other with commercial sulphuric acid; chlorine water only gives the blue colouration if the quantities are more considerable. A common cause of failure is the use of chlorine water, and of two dilute solutions. Earths, ores, metals, sulphur, &c., are first finely divided, and then boiled in a solution of carbonate of potassa, which is then treated as above. It is well to make blank experiments along with researches of this kind.

Influence of Temper upon Magnetisation.—M. J. M. Gauguier.—The ingots of steel which have received the hardest temper take the strongest magnetisation when powerful sources are employed.

Sanitary Use of Ozone.—M. Carvalho having presented to the Academy a model of a machine for ozonising the air of rooms in unhealthy climates, M. P. Thenard pointed out the dangers which may result from the random and unprofessional use of an agent by no means fully understood.

Researches on Aldehyd.—M. Berthelot.—A thermochemical study, the heat evolved being measured by the transformation of aldehyd into acetic acid.

Moniteur Scientifique, du Dr. Quesneville,
January, 1876.

History of the Manufacture of Turkey-Red.—Theodore Chateau.—This paper, so far, has a merely historical interest.

Chemical Products at the Vienna Exhibition.—M. E. Kopp.—A catalogue of the exhibitors of colouring matters with the articles shown.

Recent Progress in Industrial Chemistry.—This paper consists of extracts from Dr. Hofmann's "Report," now appearing in the CHEMICAL NEWS.

Account of Memoirs Published in the Berlin "Berichte."—M. Adolph Baeyer.

Notices of Laboratory Apparatus.—M. A. Gueront.—These notices require the accompanying figures.

Violet Ultramarine.—M. Robert Lussy.—Already noticed.

The remainder of this issue is taken up with papers from the Berlin *Berichte*, *Comptes Rendus*, and *Journal of the Chemical Society*.

Les Mondes, Revue Hebdomadaire des Sciences,
No. 2, January 13, 1876.

This issue contains no chemical matter.

No. 3, January 20, 1876.

Apparatus for Distilling Sulphuric Acid.—MM. Faure and Kessler.—This apparatus is composed of a wide shallow platinum cistern, covered with a cap of lead with double walls, between which circulates water, so as to keep the cap at a moderate temperature. The edges of the cistern are connected with the leaden cap by a hydraulic joint, and the two metals do not come in contact. On the one hand the cistern receives through a leaden tube acid at 60° from a preparatory concentrator; and on the other, it delivers acid at full strength through a pipe of platinum. Thirty-nine of these arrangements are already in operation in twenty-six sulphuric acid works.

MISCELLANEOUS.

Average Composition and Quality of the Metropolitan Waters in the Year 1875.—The following are the returns of the Society of Medical Officers of Health:—

Names of Water Companies.	Total Solid Matter per Gall.	Oxygen required by Organic Matter, &c.	Nitrogen.		Hardness	
			As Nitrates, &c.	As Ammonia.	Before Boiling.	After Boiling.
	Grs.	Grs.	Grs.	Grs.	Deg.	Deg.
<i>Thames Water Cos.</i>						
Grand Junction ..	19.78	0.084	0.146	0.002	14.9	3.8
West Middlesex ..	19.32	0.044	0.146	0.000	14.5	3.6
Southwark and Vauxhall	19.89	0.082	0.151	0.002	14.8	3.8
Chelsea	19.59	0.077	0.142	0.001	14.8	3.8
Lambeth	19.78	0.085	0.150	0.001	14.8	3.8
<i>Other Companies.</i>						
Kent	28.03	0.007	0.306	0.000	20.9	5.9
New River	19.06	0.030	0.147	0.000	14.5	3.4
East London	20.70	0.046	0.172	0.001	15.1	3.9

Note. The amount of oxygen required to oxidise the organic matter, nitrates, &c., is determined by a standard solution of permanganate of potash acting for three hours, and in the case of the metropolitan waters the quantity of organic matter is about eight times the amount of oxygen required by it.

The water was found to be clear and nearly colourless in all cases but the following, when it was slightly turbid—viz., in the case of all the Thames Companies except the West Middlesex. These results are the averages of the

analyses of the samples of water taken monthly from the mains of the several companies during the year, and they show that the total proportions of solid matter dissolved in the water have been as follows—namely, 19.67 grains per gallon of the Thames supply, 19.06 grains of the New River supply, 20.7 grains of the East London supply, which is chiefly from the Lea, and 28.03 grains per gallon of the Kent Company's water, which is obtained from deep wells in the chalk. The solid matter consists, in all cases, of a very large proportion (about two-thirds of the whole) of carbonate of lime, with a little carbonate of magnesia; and the other third is composed, in nearly equal parts, of sulphate of lime, common salt, and nitrate of magnesia. These substances, in the proportions in which they exist in the metropolitan supply, are of prime importance in a dietetical and sanitary point of view; for experience has shown that waters of a moderate degree of hardness, from the presence of calcareous salts, are more wholesome than those which are deficient of such substances. It is satisfactory, also, to know that the water supplied to the metropolis is remarkably free from organic pollution—the nitrogen, for example, which is present as actual or saline ammonia, does not exceed the 0.002 part of a grain per gallon of water; and in several cases, as in Kent, the New River, and the West Middlesex Companies' water, it is entirely absent. So also with regard to organic or albumenoid nitrogen, the proportion is exceedingly small—averaging, in the case of the Kent Company's water, the 0.003 of a grain per gallon; in the West Middlesex and New River Companies' Water, the 0.004 of a grain; in the East London water, the 0.006 of a grain; and in the rest of the companies', the 0.007 of a grain. Nitrogen, as nitrates, ranges from 0.147 of a grain per gallon of the water derived from the Thames, to 0.306 of a grain in the deep-well waters from the chalk. All these facts, together with the small proportion of alkaline chloride in the water, indicate a remarkable freedom from sewage or other organic pollution; and they fully sustain the high opinion entertained of the wholesome quality of the water supplied to London, as expressed in the reports of the scientific commission of 1850, the select committee of the House of Commons, 1867, and the Royal Commission on water supply in 1869, all of whom were specially appointed to investigate the quality of the water supplied to this metropolis. There is, moreover, a remarkable uniformity and constancy of the quality of the water supplied to London. Taking the last five years as an illustration of this, it will be found that the total amount of solid matter in the Thames supply has ranged from a yearly average of 17.98 grains per gallon to 19.67; in the New River water the range has been from 18.52 grains per gallon to 19.22; in the East London from 18.85 grains to 20.74; and in the Kent Company from 27.21 grains per gallon to 28.17. The same is the case with the individual constituents of the water, the most important of which show no sign of increase, but rather of decrease, in their proportions from year to year. If, therefore, in 1867 the select committee of the House of Commons were, as they reported, "satisfied that both the quantity and quality of the water supplied from the Thames is so far satisfactory that there is no ground for disturbing the arrangements made under the Act of 1852, and that any attempt to do so would end in entailing a waste of capital, and an unnecessary charge upon the owners and occupiers of property in the metropolis," there is manifestly still greater reason to be satisfied with the quality of the water at the present time, when with more refined processes of chemical analysis it is found that the water is yearly improving. This will be more evident when the turbidity observed in the water supplied by some of the Thames companies is effectually removed by improved methods of storage and filtration. The quantity of water daily supplied to the metropolis has ranged from 31.9 gallons per head of the population to 36.9 gallons, the average for the whole year being 33.7 gallons per head per diem. Month by month the proportions have been as follows:—

1875.	Gallons per Diem.	Houses Supplied.	Gallons per Head, Daily.
January ..	108,399,776	515,292	31.9
February ..	109,527,033	515,346	32.2
March ..	109,352,343	516,514	32.2
April ..	113,045,907	517,200	32.8
May ..	121,543,736	517,489	35.3
June ..	127,381,916	518,127	36.9
July ..	122,319,088	519,063	35.5
August ..	125,186,742	519,569	36.3
September ..	117,744,887	521,068	34.2
October ..	117,744,907	521,068	34.2
November ..	109,998,771	521,336	31.9
December ..	111,420,340	521,566	32.3
Average ..	116,138,787	518,606	33.7

About half of the water is derived from the Thames, about one-third from the New River and the Lea, and the rest from deep wells in the chalk. The largest proportion of it, amounting to about 80 per cent., is used for domestic purposes, and, considering the requirements of a household, a daily supply of 26.8 gallons per head of the population, or about 180 gallons per house, is abundantly sufficient for all domestic and sanitary purposes.

H. LETHEBY, M.B.

Brown Crystals of Calcite.—Dr. Phipson writes us:—"In the white quartz which accompanies the talcose schist on the Clyde, I have found in the neighbourhood of Helensburgh some very curious dark liver-coloured calcite imbedded in the quartz. The crystals are not perfect and isolated, but present the usual cleavage. On dissolving in very dilute acid this calcite leaves about 5 per cent of a brown residue consisting chiefly of oxide of manganese. Although this quantity of foreign matter is intimately mixed up with the carbonate of lime, it does not (except by colour) affect its usual brilliant crystalline appearance. In my opinion this oxide of manganese (which contains some peroxide of iron) has been derived from carbonate of manganese that has crystallised originally with the carbonate of lime. How has this carbonate of manganese been transformed into oxide whilst the carbonate of lime is not affected—unless by superheated steam or water under pressure?"

PATENTS.

ABRIDGMENTS OF PROVISIONAL AND COMPLETE SPECIFICATIONS.

An improved process for the distillation of alcoholic spirits, by which alcohol, free from fusel oil, is produced by a single distillation, also including an improved distilling apparatus adapted therefor, and an improved method of reducing the proof of spirits in order to produce whiskey. R. C. Brooks and A. J. Van Winkle, San Francisco. December 8, 1874.—No. 4209. This invention relates to an improved process for producing by a single distillation alcoholic spirits which are free from ether and fusel oil, and also in an improved apparatus or still for accomplishing said distillation, which is termed by the inventors a "low temperature pulsating still." This improved process consists in a modification of the ordinary process of fractional distillation which is used by chemists in the analysis of substances. This process is followed as far as it serves to eliminate the ether and alcohol separately from the wort and still, but at this point the ordinary process of fractional distillation is diverged from, by allowing the water and fusel oil to remain in the wort and be withdrawn from the still without being volatilised. Ether is one of the volatile extracts of wort, or the fermented solution from which spirits are obtained; vapourises at the low temperature of 173° F. Alcohol vapourises at 185° F., and water at 212° F., while fusel oil is not converted into a vapour at a temperature less than about 269° F. These substances are the volatilisable portions which are obtained from wort by heating it in a still, and as the alcohol is the portion which it is desirable to obtain free from the remaining volatile portions advantage is taken of the difference or space of temperature which exists between their vapourising points, to first volatilise and eliminate the ether both from the wort and still, and thus uncover the alcohol which will volatilise at the next highest temperature. The alcohol is then converted into vapour by applying a heat which will volatilise it and send it into the worm of the still and into the condenser without being high enough to vapourise either the water or fusel oil in the wort; and the result is that alcohol is obtained free from water and fusel oil, excepting only as much of the water as the alcohol vapours carry off in solution, which is usually just about sufficient to reduce the condensed alcoholic vapours to proof spirits. The alcohol portion being thus obtained independent of the ether and fusel oil, the wort is withdrawn from the

still as spent wort and replaced with a fresh charge. The process may be carried out using any ordinary still, but for convenience the inventors have devised what they term a "low temperature pulsating still." The arrangement of this improved still is such that the temperature of each part is automatically regulated and prevented from exceeding the limit required to vapourise, condense, and separate each of the products successively in the order in which they are converted into vapour; and it is called a pulsating still because, as each fresh charge of wort is introduced into the heating-chambers of the still, the temperature of the condensing and separating apparatus falls back or recedes by the automatic operation, so that the distillation of each separate charge forms a separate complete operation, beginning at a fixed low temperature, and increasing to the maximum temperature until the distillation is complete.

Improvements in the method of and apparatus for smelting or extracting iron, copper, or other metals from their ores. R. Stone, Liverpool, Lancaster. December 14, 1874.—No. 4298. This invention has for its object, first, the utilisation of the waste and inflammable gases given off during combustion in the furnace or cupola used for the smelting of metals. The waste gases are brought down through suitable pipes or flues from the mouth of the furnace or cupola (where they are emitted), and returned to the bottom of the furnace either separately by a distinct exhaust and blowing apparatus for that purpose, or in combination and connection with the ordinary blast-pipe or pipes. Secondly. The introduction of steam separately into the furnace, or in combination with the return waste gases, or in combination with gases generated by heat from any of the known salts that give off oxygen. Thirdly. The introduction of a spray, jet, or jets of hydrocarbon oil or spirit into the furnace, either in combination with a jet or jets of steam, or cold or heated atmospheric air, or separately. Fourthly. The utilisation of the said waste heat and gases arising from the furnace or cupola for heating a chamber or pipes through which pass the blast-pipes of the furnace or cupola. Fifthly. Apparatus and arrangement of same for carrying the before-mentioned method into operation.

An improved method of treating curried or dressed leather cuttings and waste so as to obtain valuable products. M. Murphy, Liverpool, Lancaster. December 17, 1874.—No. 4355. This consists—First. In treating the leather waste or scraps in a liquid or gaseous bath of hydrocarbon or other suitable oil or solvent to remove the animal or vegetable oils or fats. Second. In distilling and condensing the product left by the treatment described under the first part so as to separate the solvent from the oils or fats.

NOTES AND QUERIES.

Commercial Analysis of Phosphates.—Will some correspondents be kind enough to explain the method of determining the percentage of phosphate of lime in insoluble precipitated phosphate calcined?—**AGRICULTURIST.**

MEETINGS FOR THE WEEK.

MONDAY, Feb. 7th.—Medical, 8.

— London Institution, 5.
— Royal Institution, 2. General Monthly Meeting.
— Society of Arts, 8. Cantor Lectures. "Iron and Steel Manufacture," by W. Mattieu Williams, F.C.S.

TUESDAY, 8th.—Civil Engineers, 8.

— Photographic, 8. (Anniversary.)
— Royal Institution, 3. "On the Classification of the Vertebrated Animals," by Prof. Garrod.

WEDNESDAY, 9th.—Society of Arts, 8. "The Cultivation of Hardy Fruits, with a View to Improvement of Quality and Ensuring Constant and Abundant Production," by Shirley Hibberd.

THURSDAY, 10th.—Royal, 8.30.

— Royal Institution, 3. "On the Chemistry of the Non-Metallic Elements," by Prof. Gladstone.
— Mathematical, 8.
— London Institution, 7.

FRIDAY, 11th.—Royal Institution, 9. "Mechanical Action of Light," by William Crookes, F.R.S.

— Astronomical, 3. (Anniversary.)
— Quekett Microscopical Club, 8.
— Society of Arts, 8. Chemical Section. "Sole-Leather Tanning, with some Remarks on the Import of Hides and Cattle," by Sparke Evans.

SATURDAY, 12th.—Royal Institution, 3. "On the Vegetable Kingdom," by W. Thiselton Dyer.

— Physical, 3.

TO CORRESPONDENTS.

T. Birdsall.—We do not know. You will probably obtain the information by advertising.

J. S. Rigby.—We believe the process is patented in England. A reference to the Patent Office Library will give the desired information.

W. B.—You can obtain it from any large operative chemist.

THE CHEMICAL NEWS.

VOL. XXXII. No. 846.

CRYSTALLISED GLYCERIN.

By DR. PAUL F. VAN HAMEL ROOS.

ACCORDING to an observation of Wöhler in 1867, the glycerin seems to get in the solid state by exposing it continually to movement at a low temperature.

As I have had the opportunity of experimenting on crystallised glycerin I beg to call attention to the following points:—

It seems that the glycerin only by being in the highest state of chemical purity can be solidified.

When this chemically pure and perfectly anhydrous crystallised glycerin is melted and afterwards exposed to a temperature of 30° F., the smallest crystal of crystallised glycerin transforms all the liquid again in a solid body.

If the temperature is 24° F. the transformation is *spontaneous*, i.e., without putting any crystal into the melted glycerin it gets solid by *stirring* the liquor vigorously. The sp. gr. of this glycerin is at its melting-point 60° F. 1.261.

When this glycerin is exposed to the action of hydrocyanic acid no alteration in the colour can be observed after several weeks.

If it is mixed with yeast and exposed to a temperature of 70° F. to 80° F. no fermentation results (after two or three weeks of contact). Prof. Redtenbacher describes such a fermentation, with propionic acid as one of the results. No acid has been observed in the mixture of glycerin and yeast. If a glycerin contains one-tenth per cent of water or more, the solidification is impossible.

The crystals of glycerin seem to have a prismatic form, but it is very difficult to undertake crystallographic measurements, the melting-point being so low and the crystals very small. I am trying to get bigger crystallisations, and I hope to be able to mention soon some other experiments about this rare material.

Stirling Chemical Works, Stratford, E.,
February 2, 1876.

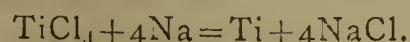
NOTE ON THE PREPARATION OF METALLIC TITANIUM.

By SERGIUS KERN, St. Petersburg.

IN many manuals of chemistry it is proposed to prepare this metal by the ignition of metallic potassium or sodium with the double fluoride of titanium and potassium (TiK₂Fl₆). The titanium obtained by this process in the form of a grey powder decomposes water very easily at 100°; but experiments proved that the titanium obtained by this method always contains an excess of unoxidised potassium or sodium, and the presence of these metals explains well why the titanium decomposes water at such a low temperature.

By the following method analogous to the production of metallic silicon titanium is very easily prepared:—

Through a tube with a bulb in the middle of it in which sodium is melted, vapours of titanium tetrachloride are passed. Then by the following reaction titanium is obtained:—



The mixture of titanium and sodium chloride is washed by means of cold water; the remaining precipitate of titanium is washed with ethyl-ether and dried over sulphuric acid. Titanium carefully prepared by this process has no action on water at 100° and only decomposes it at about 500°.

REPORT

ON THE

DEVELOPMENT OF THE CHEMICAL ARTS DURING THE LAST TEN YEARS.*

By Dr. A. W. HOFMANN.

(Continued from p. 50.)

II. Cold from Spontaneous Evaporation.

LIQUIDS capable of forming vapours require, as is well known, for their transformation into the gaseous or aëriiform state, considerable quantities of heat, which are necessary to maintain them in that condition. The heat of evaporation is not indicated by the thermometer, and is therefore often spoken of as combined heat in contradistinction from the so-called free heat which acts upon the thermometer and determines temperature. The combined heat of different liquids varies greatly; that of water, e.g., at a temperature of evaporation = 34°, amounting to 58.3 heat-units, whilst that of an equal weight of ether evaporating at the same temperature is only 90.

In the process of evaporation liquids are compelled to draw their supply of heat for evaporation in the first place from their own store of free heat. In consequence the temperature sinks. As, however, heat is conveyed from without to every substance whose temperature is lower than that of its surroundings, and as this influx is the more rapid, the greater the difference of temperature the cooling process is not without its limits. A state of equilibrium is attained as soon as at a certain reduction of temperature the loss of free heat caused by continued evaporation is compensated by the access of heat from without.

The depth of the lowest temperature of an evaporating liquid is more or less dependent on external circumstances. This point is, however, in all cases reached the more readily because as the temperature of evaporation falls the tension of the vapour, and at the same time its density and its quantity, decrease. The volume, e.g., of 1 cubic metre, which, at 34°, can be filled with 37.25 grms. of saturated watery vapour, admits, at 0°, only 4.76 grms., and at -10° only 2.29 grms. Hence it is perfectly plain that at -10°, circumstances being otherwise unaltered, evaporation proceeds much more slowly, and consequently the accession of heat from without must have a greater effect than at 34°.

The case is similar with other liquids, but so, in general terms, that those evaporate most rapidly which, at a given temperature of evaporation, possess the greatest maximum tension, or, what amounts to the same thing, those whose boiling-point lies lowest. Thus, if ether evaporates spontaneously, the volume of 1 cubic metre contains at 34°, 3750 grms.; at 0°, 1515 grms.; and even at -10°, 654 grms. of vapour; whilst at this temperature water yields only 2.29 grms. The much lower latent heat of the vapour of ether is, as we see, amply compensated by the far greater weight of the mass that evaporates under equal conditions. Thus the strong cooling power of evaporating ether is easily intelligible.

Still more striking in this respect are liquid sulphurous acid and liquid ammonia, whose boiling-points are respectively -10° and -33°.

The intensity of the cooling of an evaporating liquid is greatly augmented by cutting off, as far as possible, the accession of heat from without. This is effected, of course, by the use of coverings which conduct badly. On the other hand an attempt is made to remove influences which interfere with the speed of evaporation. An essential point is removal of the external atmospheric pressure, since the air opposes a mechanical hindrance, not, indeed, to the formation of the vapour rising from any liquid, but to its rapid dispersal. Hence a given

* "Berichte über die Entwicklung der Chemischen Industrie Während des Letzten Jahrzehends."

space, for whose perfect repletion with saturated vapour several minutes would not suffice, is almost instantaneously saturated if the air be withdrawn.

For the removal of the air a good air-pump is in most cases employed. The air-pump alone as a promoter of evaporation would, however, in general, prove insufficient, since its action is not powerful enough to remove the vapours with the same speed as they are produced in a space free from air. But the evaporation is completely interrupted as soon as the given space is filled with vapour of the same temperature at which the evaporation goes on. This purpose of a speedy removal of the vapours arising from an evaporating liquid is satisfactorily effected by their absorption; thus the vapour of water is removed by means of concentrated sulphuric acid.

(To be continued.)

ON A READY MEANS OF DETECTING ARSENICAL COMPOUNDS.*

By EDMUND W. DAVY, M.A., M.D.

Professor of Forensic Medicine, Royal College of Surgeons, Ireland

THE extensive employment of certain compounds of the metal arsenic for the criminal destruction of human life has rendered their detection under different circumstances a matter of great importance to society, and to attain this end they have long been objects of much interest to the chemist and toxicologist. Fortunately for mankind, the metal itself, as well as its combinations, have been found to be endowed with very characteristic chemical properties, and on these are based several excellent tests, by which, in the hands of the chemist or in those skilled in the detection of poisons, very minute quantities of arsenic or of its compounds can be identified with more or less facility; and the fear of such detection has acted as a great preventative against their criminal employment as poisons; for, before such means of recognising their presence were discovered, secret poisoning by arsenious acid—which is popularly known as “arsenic”—was carried on to a fearful extent, a greater number, perhaps, of individuals having been already deprived of life by that substance than by all the other known poisons put together. But now, owing to our possessing the means by which even very minute quantities of arsenical compounds can be detected with almost unerring certainty, and there having been of late years certain legal restrictions placed on the sale of arsenic, cases of homicidal poisoning by that substance have become comparatively rare. Still, as such cases or those from accident do from time to time occur, and as different arsenical compounds are used for a number of industrial purposes, some of which are highly objectionable, endangering as they do the health, and even lives, of many individuals, it is very desirable that we should be able readily to detect those virulent substances, not only where they may occur by design or accident in different articles of food or drink, or in the bodies of those who have died from their effects, but likewise where they may exist in various manufactured products, the use of which might be attended with very serious consequences. The test which I would now propose, being one of such simplicity and ease of execution that it might be performed by almost any one, will, I should hope, be found useful for the objects stated, especially to those who are not very conversant with the details of chemical manipulation. As it is a modification of Mr. Marsh's test, it is necessary for me briefly to refer to that method before describing the one I would now suggest. That gentleman's test, as is well known, is founded upon the circumstance that nascent hydrogen in presence of certain compounds of arsenic will give rise to the formation of arseniuretted hydrogen,—a gas which,

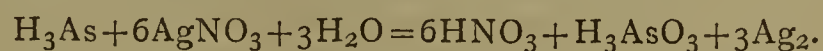
being possessed of very characteristic properties, may be easily recognised, and thus very minute quantities of arsenic under different circumstances can be readily detected. This method, as proposed by its discoverer, consists in generating, in a suitable apparatus, hydrogen by the action of dilute sulphuric acid on metallic zinc, and then adding in the state of solution the arsenical compound, when arseniuretted hydrogen will be quickly generated, and a fine jet of the gas being ignited, and a cold surface placed down on the top of the flame, very characteristic spots or stains of metallic arsenic will be produced; or the gas being passed through a heated tube, it will be decomposed, and a metallic sublimate formed at a short distance beyond the heated portion. I need not refer to the apparatus recommended by Mr. Marsh for carrying out his test, as it is now so well known, nor to the modifications of it which have been subsequently proposed; and I must acknowledge that this beautiful means of detecting arsenic, owing to its great delicacy and very conclusive results in the hands of the experienced chemist, leaves but little to be desired. It, however, labours under this serious disadvantage, that the acid and the zinc which are employed in the process may one or other of them, or even both, contain more or less of arsenic as an impurity, and consequently the indications of that substance which are thus obtained may be due not to its existing in the suspected matter or object under investigation, but to its occurring as an impurity in the materials employed in this process for its detection; and I may add that it is difficult to get in commerce the zinc and sulphuric acid required perfectly free from arsenic.

To obviate more or less this source of fallacy several modifications of the original process of Marsh have been suggested. Thus Fleitmann, some years ago, proposed the use of a strong solution of caustic potash, assisted by heat, instead of the acid, to act on the zinc as a means of generating the hydrogen gas, and in this way one source of arsenical contamination was avoided. It was found, however, to be too slow a means of generating hydrogen to detect arsenic in the usual way by Marsh's method. Prof. Bloxam has suggested the employment of a galvanic battery for the generation of the same gas, and in this way obviates the use of zinc, and thus excludes another possible source of fallacy; but, owing to the trouble and expense attendant on the use of a galvanic battery, which for this purpose must be of some power, and the arrangement being of rather a complicated character, and still requiring sulphuric acid, it has, I believe, been but little employed. I should also add that the metal aluminium, and more recently magnesium, have been proposed as substitutes for zinc in Marsh's process or in Fleitmann's modification of it, as being less likely to be contaminated with arsenic than that metal. The modification which I would now suggest, and which, as far as I can ascertain, has not hitherto been proposed, is the employment of an amalgam of sodium and mercury as a means of generating the hydrogen required for the test; and by the use of this substance I do away with, altogether, the necessity of any acid, and I employ two metals which are not liable to arsenical contamination. As to sodium, I am not aware that arsenic has ever been pointed out as one of its impurities; and as to its presence in mercury, that is, I believe, a circumstance of very rare occurrence; but, should it exist in that metal as an impurity, it can be readily removed from it by digesting the mercury in diluted nitric acid, and afterwards well washing it with water. The amalgam which I have found to answer very well for the detection of arsenic consists of 1 part by weight of sodium to 8 or 10 parts of mercury, and is easily made by heating moderately in a test-tube over a lamp the mercury, and then adding gradually in small pieces the sodium, taking care to keep away the face, if unprotected from the mouth of the tube, lest some of that metal in an ignited state might be spirted out during the addition of the first portions. Those metals readily combine under these circumstances, forming an alloy that is liquid whilst hot, but

* A Paper read before the Royal Irish Academy.

becomes hard and brittle when cold. The contents of the tube, while still hot and liquid, are quickly poured out on a clean plate, and, when cool, broken up in small lumps, which are then immediately placed in a well-corked or stoppered bottle.

The way I employ this amalgam is simply to place the suspected solution, or solid matter along with a little water, in the bottom of a test-glass; then add a small bit of the amalgam, about the size of a grain of wheat; and lastly, place without delay, on the top of the glass, a piece of white filtering-paper or the cover of a white porcelain crucible moistened with a drop of a dilute solution of nitrate of silver, slightly acidulated with nitric acid, when—if arsenic is present—a dull black or deep brown stain on the paper, or a dark silvery one on the porcelain, will be quickly developed in the part moistened, owing to the silver of the salt being reduced to the metallic condition by the agency of the arseniuretted hydrogen thus evolved, which, coming in contact with the nitrate of silver, gives rise to the following reaction:—

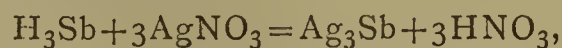


The silver solution, which I have found to answer very well for this purpose, was made by dissolving 20 grains of the nitrate in an ounce of distilled water, and then adding 2 drops of strong nitric acid, to render the solution slightly acid. I may further add that I generally place a small disc of bibulous paper between the mixture in the glass and the paper or cover moistened with the silver solution, to intercept any particles of the liquid which might otherwise be projected against them, producing there minute black spots, and thus interfering with the results of this test.

I have found that exceedingly minute quantities of arsenic can be readily detected by this very simple process: thus the 1-1000th part of a grain of arsenious acid, dissolved in 1 c.c. of distilled water, gives a very decided effect in a few moments; but much smaller quantities are detectable by it: thus the 1-100,000th or even the 1-1,000,000th part of a grain of arsenious acid, dissolved in the same quantity of water (1 c.c.), will afford, by the blackening of the silver salt, after a little time, an indication of the presence of arsenic. I have also ascertained that this method of detecting arsenic is not only directly applicable to where it exists as arsenious acid, but likewise to several other compounds of arsenic, whether they are soluble or insoluble in water: thus, for example, the two sulphides of arsenic (orpiment and realgar), the alkaline arseniates, and even metallic arsenic itself if reduced to powder, will readily show their arsenical nature by this test; and we may in a few moments detect by it the occurrence of arsenic in different green, yellow, and orange pigments, which are still much employed in the manufacture of wall-papers, in painting, and in the colouring of certain textile and other articles used in dress or for ornamentation. Thus, for example, if a little of the colouring matter of any arsenical pigment be scraped off from a wall-paper, or a small piece of the paper itself be taken and placed in a test-glass with a little water, and having been stirred or shaken to detach the colour, a piece of the amalgam be added, it will—by the blackening of the silver salt employed as before described—soon indicate the presence of arsenic. In the same way it can be easily demonstrated that the colouring matter in certain green tarletans, calicoes, and other articles used for dress or for ornament, are arsenical. I may further state that the presence of organic matter seems to interfere but little with this test, for I have found that very minute quantities of arsenious acid, when mixed with considerable amounts of milk, tea, coffee, ale, porter, soup, or stirabout, could—with almost the same facility—be detected by this method, as where they were only simply dissolved in water; thus showing that the cases to which it is applicable are very extended.

But I should here observe that, as in the case of Marsh's original method, there is one other metal which

under certain circumstances, will produce with the sodium amalgam results closely resembling those occasioned by arsenic: the metal I refer to is antimony, which is capable of uniting with nascent hydrogen to form a gas (antimoniuretted hydrogen), which, coming in contact with nitrate of silver, produces a black antimonide of that metal, by the following reaction:—



and the blackening of the silver salt from the formation of that compound might be easily mistaken for the effect produced by the arsenical gas.

But owing to the fact, first pointed out by Fleitmann, that antimoniuretted hydrogen is not evolved (except, perhaps, as a mere trace) from strongly alkaline solutions, though the conditions may exist there for its formation, and as the action of the sodium amalgam is to render the mixture quickly alkaline, there will be only a very minute quantity of the antimony that may be present so evolved; and, by previously rendering the mixture strongly alkaline, we may almost altogether prevent the evolution of that gas. If, however, we make the mixture containing the antimony in solution first strongly acid, and then add the amalgam, or even acidify after its addition, the antimoniuretted hydrogen will be evolved in abundance, producing a deep black stain on the paper moistened with the nitrate of silver; and, for the purpose of this acidification, I have found that tartaric acid answers very well. As the presence of alkalis in solution do not interfere with the evolution of the arsenical gas, this is itself a means of distinguishing the two metals, arsenic and antimony.

But it may be occasionally necessary to determine whether the effects observed on the paper moistened with nitrate of silver are due to arsenic or to antimony. There are different methods by which we may determine this question; but the one I have found the simplest, and on the whole the most satisfactory, is to digest the paper stain in sulphide of ammonium, when the arsenic or antimony present will be converted into a sulphide, and dissolved by the excess of the alkaline salt, leaving the silver sulphide undissolved, and adhering principally to the paper; the alkaline solution, on being evaporated to dryness, will, in the case of arsenic, leave a bright yellow residue almost insoluble in hydrochloric acid, whereas in the case of antimony an orange one will remain, which readily dissolves in that acid, at least on the application of heat.

Before concluding I wish to observe that, according to some experiments recently made by Dr. Russell, it appears that hydrogen alone is capable of reducing solutions of nitrate of silver to the metallic state; but this action, even from his observations, is an exceedingly slow one, and takes place to a very minute extent in dilute solutions. On the other hand, M. H. Pellet maintains that hydrogen carefully freed from acid and arsenic, by passing it through solutions of soda and of nitrate of silver, has no action on that salt at the ordinary temperature. But he states that nitrate of silver which has been fused possesses an alkaline reaction in solution, and that a slight precipitate is produced in such by pure hydrogen: if, however, he observes, a drop or two of nitric acid be added, then nothing is precipitated. Be this as it may, as regards the reducing action of pure hydrogen, I found, in an experiment I made, that hydrogen which had been passed through solutions of caustic soda, and of nitrate of silver, and was afterwards brought in contact with a porcelain crucible cover, moistened with the dilute and acidulated solution of nitrate of silver already noticed, produced only the faintest possible effect, even after several hours' exposure to a stream of this gas, and this very slight action might possibly be due to the hydrogen not being perfectly freed from its impurities. Consequently, it is very doubtful that any reduction of the silver salt from the hydrogen alone will occur under the circumstances of the proposed test. Finally, I must observe that where paper moistened with the silver solution is used to detect

arsenic or antimony, we must bear in mind that nitrate of silver will alone, after some time, blacken the paper, especially if it is exposed to the light; but this gradual change which is so produced is very unlike the rapid effect that takes place where either arseniuretted or antimoniuiretted hydrogen acts on paper moistened with that silver salt.

ACTION OF AMMONIA UPON ROSANILINE.

By M. E. JACQUEMIN.

MM. PERSOZ, De Luynes, and Salvétat* have shown that magenta, since named rosaniline, is capable of playing the part of a feeble acid; that it combines with ammonia to form a compound, colourless, but alterable even by an excess of the solvent, and incapable of dyeing without the intervention of an acid which displaces it and restores to it the power of combining with the textile fibre.

In 1861, resuming the study of this question after the publication of my memoir on the aniline-reds,† I remarked that the alteration of magenta is not immediate; that it is only produced gradually; and that a certain number of days are required for it to become complete. I have shown every year since that time in the Course of Organic Chemistry at the Higher School of Pharmacy, at Strasbourg, that it is possible to render manifest the presence of the colour up to its entire transformation. It is simply requisite to steep wool, previous moistened, in the colourless ammoniacal solution, which is heated moderately, but not to the boiling-point. The curious phenomenon is then produced of a tissue which is dyed a bright red in a colourless liquid. According to Dr. Hoffman aniline-red is a compound of a colourless base and of an acid. But this acid in the commercial substance, having entered into combination with ammonia, it is not possible to admit that the wool induces the decomposition of the ammoniacal salt and the reconstitution of the red in order to combine with the latter. We are led to consider the combination of the rosaniline and the ammonia as a molecular compound which is dissociated by heat, the wool having the property of uniting with the colourless base, which abandons the ammonia, and of filling as regards it the function of an acid in producing a red compound.

Having continued the study of this question, I shall have, in my next communication, the honour of pointing out the products of the decomposing action exercised by ammonia on the different aniline colours, and of defining more precisely its first function. It will then be seen that it is not even possible to admit always a molecular combination, and that, in the case of aniline-blue for instance, it is not necessary to go beyond the phenomenon of simple solution, since a skein of cotton steeped in the liquid is dyed blue by the simple process of the evaporation of the solvent in the air.—*Comptes Rendus*.

Mineralogical Society.—At the meeting held on Thursday, the 3rd inst., the following Officers were elected, viz.:—

President—H. C. Sorby, F.R.S.

Vice-Presidents—Prof. Haughton, F.R.S.; Prof. Heddle, P.R.S.E.

Council—Prof. Ansted, F.R.S.; Prof. Bonney, M.A.; Prof. Church, M.A.; W. Crookes, F.R.S.; F. Drew, F.G.S.; Major Duncan, D.C.L.; A. Geikie, F.R.S.; Capt. Marshall Hall, F.G.S.; Prof. T. Rupert Jones, F.R.S.; Prof. Nicol, F.R.S.E.; Prof. Rudler, F.G.S.; R. H. Scott, F.R.S.

Treasurer—R. P. Greg, F.G.S.

Foreign Secretary—C. Le Neve Foster, D.Sc.

Secretary—J. H. Collins, F.G.S.

* "Rapport d'expertise sur le rouge d'aniline," 1860; Pelouze et Frémy, *Traité de Chimie*, 3rd edition, t. iv., p. 710.

† February, 1861, "Procès Renard et Franck contre Depouilly freres et Ch. Lauth."

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, February 3rd, 1876.

Professor ABEL, F.R.S., President, in the Chair.

THE minutes of the previous meeting having been read and confirmed, the presents of books made to the Society were announced, and the donors thanked; after which Messrs. A. Smetham, B. Shirley Dyer, W. E. Halse, and E. H. Girling were formerly admitted Fellows of the Society. The names read for the first time were those of Messrs. A. B. Prescott, M.D., N. Bradley, and S. W. Porter. Messrs. William Galbraith, David E. Brown, A. H. Scott White, B.A., George Wilson, M.A., William Foulkes Lowe, Stephen William Nockolds, George Haycraft, Frederick James Lloyd, Harry Allen, Frederick Isenbart Scard, Harold Bailey Dixon, and William Alexander Smith were elected Fellows after their names had been read for the third time.

The PRESIDENT said he had to announce that he had obtained permission to invite the Fellows of the Society to visit the Royal Arsenal at Woolwich, and hoped that on that occasion he should be able not only to show them a trial of the 80-ton gun and the working of the largest forge-hammers, but also to exhibit some experiments on detonation which could not be conveniently performed in the lecture-room, and other kindred subjects which might prove of interest to the Fellows.

Mr. W. ACKROYD then read a communication on "*Metachromism, or Colour Change*." In this elaborate paper the author, after giving a brief account of the notices scattered throughout various scientific papers on the subject of metachromism, as he terms the changes of colour which various substances undergo when heated, passed on to the classification of metachromes, which he arranges in two groups,—namely, those of the zinc oxide class, colourless bodies which acquire a yellow colour on being heated; and those of the borate of copper class, which change from one colour or combination of colours of the spectrum to the contiguous colours: the red iodide of mercury, for instance, becomes darker and darker, as it is heated, up to about 140° C., when it is converted into the yellow modification. At higher temperatures the yellow becomes gradually darker, until at 220° C. it is deep orange. From a study of the two classes the following metachromatic scale was arrived at:—

White, colourless, violet, indigo, blue (metallic appearance), green, yellow, orange, red, brown, black.

The colours of the more refrangible end may be replaced by a metallic appearance. Metachromism has an important bearing on allotropy. A body expanding through the influence of heat being really a continuous series of allotropes. In support of this the relation of colour and density was discussed. It was shown that metachromism is due to the storage of potential energy, the author holding that molecular vibrations or kinetic energy have nothing to do with this phenomenon of selective absorption. Contracting metachromes changing from less to more refrangible colours, where would this change cease providing a low enough temperature could be had? Presumably at the absolute zero of temperature and at this point all metachromes would be white or metallic looking, judging from their behaviour at attainable temperatures. Following expanding metachromes from the absolute zero of colour the change in each would vary with the coefficient of expansion, giving us at the normal temperature all that variety of hue which we see in the inorganic world. Including certain cases of decomposition (given in a table), colour change may denote—

- (1). If to more refrangible— α contraction, or β decomposition.
- (2). If to less refrangible— α expansion, or β combination.

The observations relate to anhydrous and for the most part binary compounds.

The paper concludes with some remarks on the simultaneous change of colour and density observed on heating certain minerals, such as zircon.

The PRESIDENT said they were much indebted to Mr. Ackroyd for his interesting paper, which raised several points for discussion.

Mr. W. N. HARTLEY said for the past two years he had made many experiments on the changes which solutions of certain salts undergo when heated, but in most cases the change was due to a variation in the hydration of the salt, the series of colours produced, however, being somewhat in the order given by the author. For instance, the brown or pink solution of cobalt, when heated, darkens at first, the effect being probably due to expansion, but variation in the hydration then begins to come in, and the brown hexahydrate is reduced to the green dihydrate. Again, the yellowish green colour of a solution of copper chloride turns to brown when heated to 100°C ., which may perhaps be due to metachromism. The solid copper bromide behaved in an entirely different manner; the golden yellow tetrahydrate loses water at a comparatively low temperature, changing to the brown monohydrate. The yellowish green solution of the salt changes to brown when heated like the chloride. In those solutions, however, in which no change of hydration takes place, the solution darkens, which is in accordance with Mr. Ackroyd's observations. A peculiar phenomenon is observed when dichroic minerals such as epidote are heated; the dichroism entirely disappearing under these circumstances.

Mr. FRISWELL understood the author to say that if the colour changed to one at the more refrangible end of the spectrum, decomposition usually took place. He would like to ask whether he had examined any of the platino-cyanides: the red hydrated magnesium salt, as was well known, when heated, lost water, and changed first to orange, then to yellow, and finally to white, whilst the barium compound under similar circumstances also lost water, but changed from brilliant yellow to red-brown. Both these compounds undergo decomposition, and yet a change took place in opposite directions in the two cases.

Mr. JOHN A. R. NEWLANDS said that metachromism pure and simple included such changes as that of a white substance which when heated became yellow and on cooling regained its original colour. Other changes of a more permanent kind, as that of the scarlet mercuric iodide into the yellow modification of the same substance, having a distinct crystalline form, could hardly be included under metachromism, but might be rather considered as due to some chemical change. If the yellow mercuric iodide be regarded as the simple molecule, the scarlet modification might possibly be a combination of mercuric iodide with itself. At any rate it would be well to keep in view the possibility of the chemical combination of two or more molecules of the same substance producing effects akin to those originating in metachromism.

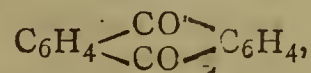
Mr. ACKROYD replied, that in the case of mercuric iodide he had determined the specific gravity before and after the change, but the results were not concordant: the change from red to yellow would indicate contraction however. He had not examined the platino-cyanides.

Mr. HARTLEY said he might mention he had once observed that a magnificent specimen of the magnesium platino-cyanide, sealed up in a specimen glass which had been placed for a short time near a gas flame, lost its beautiful green iridescence, and became of a yellowish colour: after remaining some time in a cool place, it reabsorbed the water which had been expelled, and regained its original appearance.

Mr. W. H. PERKIN, F.R.S., then read a paper "On the Formation of Anthrapurpurin." Although it was known

that potassium anthraquinon-monosulphate, when fused with caustic potash, yielded alizarin, it was generally believed that in the artificial preparation of alizarin the latter was produced from the disulphonic acid. This the author finds, however, is not the case: the alizarin is produced from the monosulphonic acid, whilst the disulphonic acid yields anthrapurpurin, a substance in many of its properties akin to alizarin. When the disulphonic acid is heated with potassic hydrate, it first yields oxyanthraquinon-monosulphonic acid, $\text{C}_{14}\text{H}_6\text{O}_7(\text{OH})(\text{SO}_3\text{H})$, and this, by the further action of the fused alkali, is changed to anthrapurpurin, $\text{C}_{14}\text{H}_6\text{O}_2(\text{OH})_3$. The action of an aqueous solution of potash at a high temperature on this acid gives rise to an intermediate body, $\text{C}_{14}\text{H}_6\text{O}_2(\text{OH})_2$, isomeric with alizarin, and apparently identical with the substance recently observed by Schunck and Roemer. It is converted into anthrapurpurin by the further action of the alkali. In alizarin the hydroxyls are both in the same benzene group, but in anthraquinon-disulphonic acid it would appear that one HSO_3 exists in each benzene group, which would account for the non-formation of alizarin from this acid when it is heated with an alkali, and would, moreover, tend to show that in anthrapurpurin two hydroxyls are in one benzene group and one in the other.

Dr. ARMSTRONG remarked there could be no doubt that anthraquinon had the formula—



and since alizarin yielded phthalic acid on oxidation it was highly probable that the two OH groups were both in one benzene group, a view which was confirmed by the synthesis of alizarin from phthalic acid and pyrocatechin. The discovery of an isomeride of alizarin, to which the author had alluded, was of great interest: there were now five bodies known having the same composition as alizarin.

Mr. PERKIN said the isomeride alluded to did not yield phthalic acid on oxidation, from which it was probable that both the OH groups were not in the same benzene group. Anthraflavon, also, did not yield phthalic acid.

The PRESIDENT having thanked the author in the name of the Society, a communication from Mr. C. O. SULLIVAN "On Maltose" was read, in which the author conclusively proves that maltose obtained by the action of malt extract on starch is not merely a mixture of dextrose and dextrin, but a distinct compound. Comparative experiments were made by treating both a mixture of dextrose and dextrin with alcohol, and also maltose. In the former case the dextrose was removed, leaving a residue of dextrin which had scarcely any action on Fehling's test. With maltose, however, the case was different, the portion which dissolved having exactly the same reducing action as the undissolved portion. Fermentation experiments made with maltose and the above mixture led to analogous results. He concludes with observing that maltose is a distinct compound, isomeric with cane-sugar, and having a specific rotary power rather more than twice as great; moreover, 100 parts of maltose are capable of reducing as much cupric oxide as 65 parts of dextrose.

The PRESIDENT thanked the author, in the name of the Society, for his interesting communication; after which a paper by Mr. T. FLETCHER was read, on "A Simple Form of Gas Regulator," in which the author states he has had a regulator in use for the last fifteen years very similar to that recently described by Mr. Page, except that he passes the gas in the reverse way, and considers it is practically better to have a pin-hole in the cork or centre tube, to prevent the gas being extinguished, instead of the double service. He also recommends an iron chamber of large size, containing 2 or 3 lbs. of mercury.

The last paper, by Mr. T. CARNELLY, B.Sc., "On High Melting-Points, with Special Reference to those of Metallic Salts," was taken as read, owing to the lateness of the hour. The principle upon which the experiments described

in this paper were conducted consists in heating a small platinum crucible containing the salt by means of a Bunsen, or other suitable means, and the instant the salt is seen to melt the whole is dropped into cold water. From the observed rise of temperature the melting-point of the salt can be calculated. Tables are appended to the paper giving the fusing-points of a large number of substances as observed by this method.

The meeting was finally adjourned until Thursday, February 17th, when Prof. Frankland, F.R.S., will deliver a lecture "On some Points in connection with the Analysis of Waters."

NOTICES OF BOOKS.

On the First Principles of Chemistry. By EDMUND J. MILLS, D.Sc., F.R.S., Young Professor of Technical Chemistry in the Andersonian College, Glasgow. (Reprinted from the *Philosophical Magazine* for January, 1876.)

DR. E. J. MILLS—but lately elected Young Professor of Technical Chemistry in the Andersonian College, Glasgow—has published an essay on the first principles of chemistry. Issuing, as this does, from a Chair of Chemistry at one of our Universities so justly famed for its advancement of natural science, we of course look for something the perusal of which shall reward us.

In treating of a matter which more than borders on the region of metaphysics, it is only natural that a writer should indulge in preliminary considerations of a so-called philosophical character before attacking the actual subject which he intends to discuss. Dr. Mills forms no exception to this rule, but rather avails himself of the privilege to its fullest extent. Let us, however, first listen, and afterwards judge.

Our author, then, commencing with some highly laudatory observations upon Ferrier and his philosophy, after quoting several most amusing passages from the eccentric author of "Les Institutes," and after subsequently pointing out to us what he considers to be the analogy between the final triumph of human reason and the lesson inculcated in the parable of the Prodigal Son, proceeds to the consideration of the difficulties of investigating first principles in language replete with metaphor, the quotation of which in its entirety space alone precludes.

But one of the passages in this paragraph demands special attention. Speaking of the ideal reformer who would "disinter the pure jewels of science" lying deep under a metaphorical roadway, Dr. Mills says:—

"He must have an infinite capability of doubting. For his is no search for truth—of which all men chatter, but which none are admitted to possess; his is a reasoned labour, and the issue, of which he is careless, must in the main be wise."

Calm contemplation of these sentences is unavailing. They may be "chewed," but are not to be "digested." Absence of conspicuity of expression in the handling of such themes as the present one is no rare thing; but Kant, at his worst, leaves us a loophole of meaning, and even Schopenhauer does not desert us in the dark. Macaulay said somewhere that there are arguments, the mere statement of which constitutes their refutation; but Dr. Mills in here avoiding that lucidity so characteristic of such stylists as Prof. Huxley or Dr. Tyndall, may be said to have afforded us arguments of a most irrefutable character.

Is the search for first principles no search for truth? Are men to be careless of the issue of their research? It is as difficult to reply to these questions as it is to the one Dr. Mills asks his readers immediately after the passage above cited. To quote—

"Such being the case, can we wonder that no clear statement of the first principles of chemistry has ever yet

been made, and that even the very definition of the science is either unknown or obscure?"

Dr. Mills becomes more intelligible further on where he advocates reform—a reform which means nothing more nor less than the disestablishment of an ancient institution, the atomic theory. In asking "Where then are we to commence?" Dr. Mills says:—

"The first principle of all science is motion. Every event of which we are conscious proves on analysis to be motion of some kind, and matter is not distinguishable from motion except as more or less determinate motion."

Matter and motion are very inseparable ideas in many, and in fact most minds, but it is surely going rather far to speak of one as "not distinguishable" from the other.

"We have then to select those derived forms of motion," Dr. Mills goes on to say, "which, lying nearest to it, are yet within the province of chemistry. One of these must necessarily be action, because the chief business of chemistry is a kind of work." And, moreover, a hard kind of badly-remunerated work. Further on we read:—

"Chemical substances are valued, not for what they are conceived as *being* but as *doing*, and the first question we ask about a body is—What is its function?"

Very true. But Dr. Mills, it would seem, fails to distinguish nicely between "property" and "function," for the first questions he asks about a body are—Has it an alcoholic function? or a saline function? or a ketonic function? or a zincous function? or a chlorous function? questions which he answers through the agency of what he is pleased to term "elementic discriminants." Later on in his essay Dr. Mills contents himself with remarking that "constitution" has been, and always is, considered of small importance in comparison with "action." The two standing to one another, as he somewhat synonymously expresses himself, "in the relation of speculation to experiment."

"Indigo, sugar, salt, and alcohol," says Dr. Mills in pursuing this idea, "were extremely valuable before anything whatever was asserted of their constitution—valuable then as now for the employments to which they could be put." "The producer," he continues, "prudently regarding their constitution with a wise indifference." Some would have called the producer *imprudent* and his indifference *unwise*, but this is of course merely a matter of opinion.

After reaching this climax, Dr. Mills descends from his flight of diction, and becomes once more interesting and intelligible. The following two passages give us the text, as it were, of our sermon:—"The most important character of chemical action is continuity, which, as has already been stated, is an immediate derivative of the idea of motion." And somewhat later—"For it is as experimentally certain that the generated substances take part in a reaction as it is that the generators themselves share therein. When, therefore, a body is enclosed in some vessel, and set aside, it must not be regarded as inactive; it may quiver, but will not expire."

By which Dr. Mills would say that chemical action never ceases. He would assert, and practically does (in the last quoted passage) assert that when, for instance, a mixture of oxygen and hydrogen, containing these gases in the theoretical proportion necessary for the formation of water, is exploded, some further chemical action continues, and water acts upon water. But of what nature is this continued action, and upon what observations does Dr. Mills base his assumption?

So much for continuity of chemical action, to which we return immediately. Let us hear what Dr. Mills says as regards chemical mass:—

"According to the evidence we possess (and some of it is of the highest order of accuracy), no matter what may be the masses of the substances acting, the entire mass of each takes part in the process."

The expression "takes part" is as vague as many of Dr. Mills's pet expressions. There can be no doubt as to

the influence which mass exerts upon chemical action, but whether that influence is entirely physical or entirely chemical, or partly both, are questions which still await reply. Now Dr. Mills asserts that if, for instance, an ounce of zinc be immersed in water, and a quantity of sulphuric acid added, the whole of this reagent acts whether it weigh an ounce, or a pound, or a hundred-weight, and that all the excess of acid which is not required to make zinc sulphate should not as "undeniably sharing in the process" be omitted from the equation symbolising that process. It must be admitted that practically (and we use this term advisedly) an excess of acid appears necessary for the complete solution of *all* the zinc present, but this excess can never be admitted as theoretically necessary, for were this so we should indeed have to abandon our atomic theory. The most plausible argument that atones for this apparent deficiency in theory is that the excess of acid replaces one of the factors necessary for the theoretical realisation of the process, and that the replaced is that most convenient of all factors—*time*. Looked at from this point of view, such an equation as $\text{Zn} + \text{H}_2\text{SO}_4 = \text{ZnSO}_4 + \text{H}_2$ ceases, we think, to deserve being spoken of as expressing but a "poor and single point" of the process.

Dr. Mills next proceeds to discuss the well known researches of Harcourt and Esson, and those of Guldberg and Waage, on the influence of mass, time, &c., on chemical action. The former chemists assert to have shown that in chemical action the amount of change is at each moment proportional to the total amount of changing substance, but they do not, at the same time, dispute the truth of Dalton's "definite proportions," and certainly do not disprove them. Guldberg and Waage, arriving at the same results, argue further that the researches of Berthelot, Scheerer, and more particularly those of Debus, in the other direction, give results capable of representation on their principles. But Bunsen's experiments on the explosion of mixtures of certain gases would seem to point to conclusions of a very opposite tendency, and results might, in fact, be thus stated:—In that mutual decomposition of bodies called chemical action all quantities present do not act, but only (and generally a small) proportion of the whole, and that in the case of bodies in solution the proportion between the acting masses and the total mass would mostly appear to be always continuous; but that in the case of gases (Bunsen) this proportion, though always admitting of representation by simple ratios, is not continuous but discontinuous.

Now Dr. Mills argues that Bunsen's experiments go to prove continuity in place of that discontinuity to which they apparently point; but the only argument he brings against them is that one has no right to deduce discontinuity where it has been expressly introduced. It might, however, with equal justice, be said that we have no right to deduce continuity where the unavoidable conditions of experiment would specially favour it; so that in saying that those positions in chemical action which Dalton termed "definite proportions" are fitly conceived as *maxima* and *minima* on curved lines Dr. Mills is giving us a very fascinating idea, which, however, requires the further corroboration of facts ere it can be fully accepted as the explanation of facts.

And now Dr. Mills deserts chemistry to launch us in the midst of metaphysics. He raises, but does not solve, the question whether matter, considered as directed concrete isolable motion, may not be transferred into energy that is diffuse, or non-isolable motion, or *vice versa*. As a small result of which consideration we are told that water cannot consist of oxygen and hydrogen, and that no one can assert that argentic chloride has the same composition now as it had (in the good old times) one hundred years ago! But this is not all. As a further and far more important result of such cogitations Dr. Mills would abolish the vulgar chemical equation, ignore the theory of "constitution," and dispense with the Berzelian nomenclature; and having thus left us, as it were, with-

out a chemical leg to stand upon, he comforts us with the assurance that chemistry existed before Dalton.

Dr. Mills's suggestion of replacing the old chemical symbols by mathematical ones as expressing continuity is very worthy of attention, but, as many of Dr. Mills's suggestions are, is far in advance of facts. The same remark applies to Dr. Mills's idea with regard to a dynamic equivalent, or Bergmannic as he christens it. "It is sufficiently obvious," he says, "that the real equivalent of a body is that which performs the unit of work, however that unit may come to be defined."

For example, if *i* stand for baric iodide, and *c* for baric chloride, we have a solution of the equation—

$$i = fc.$$

This indicates a field of research, and is certainly the best thing in the essay.

As regards Dr. Mills's "energy" explanation of isomerism, and also as regards his researches on "valencies," we must refrain from more than mentioning these, and refer the reader to the tabular summary of the principles advocated by Dr. Mills at the end of his essay.

And now having followed Dr. Mills somewhat closely in his arguments, one may well ask with him:—

"What is the practical result of this discussion?"

Dr. Mills challenges the reader to choose between the "atomic" theory and the so-called "dynamic theory," and some readers might give him this answer:—

What is theory after all but the logical interpretation of accepted facts which, becoming the basis of new generalisations, induces the discovery of new facts. But facts must precede theory. And the atomic theory is, we assert, in its widest development a logically admissible interpretation of those facts which are already known to us, but Dr. Mills would have us put this aside and advocate a theory for which facts, as he himself admits, have as yet specially to be created—a theory which is therefore, as Dr. Mills states it, no theory at all in the stricter sense of the word but a system of metaphysics. Such a system as approaches nearest to those of Schelling and Hegel, but which in its main principle is far older than the science of chemistry itself.

To adopt somewhat the phraseology of Dr. Mills the reader might continue to say that rather than desert the strict path of inductive inference to wander in the fascinating but delusive mazes of metaphysics, he (the reader) would prefer to retain the "atomic" theory because accumulating facts warrant its retention rather than its rejection, and because, as Dr. Mills himself says, in a mood which none can too much admire, "It is juster and wiser to accept facts than to attempt to transcend them."

H. W. H.

CORRESPONDENCE.

SPRENGEL'S WATER-VACUUM PUMP.

To the Editor of the Chemical News.

SIR,—Circumstances have, till now, prevented my calling attention to an error committed by one of your contributors in your final issue for 1875. Mr. W. Thomsom, F.C.S., speaks (CHEMICAL NEWS, vol. xxxii., p. 310) of "Bunsen's vacuum pump."

Now those people who persist in speaking of the "water pump" as Bunsen's may think they have sufficient warrant for doing so, although (as I shall presently show) they have not a particle. But this Fellow of the Chemical Society, without referring to the fluid used for obtaining the vacuum, or, indeed, without mentioning Bunsen's appliances for using the vacuum, speaks of the method of obtaining a vacuum discovered by Sprengel as though it were due to Bunsen. Doubtless he sins in

company with a great many others, but the persistent ignoring of the facts thus displayed ought, in common fairness, to be put an end to.

Bunsen has said (*Phil. Mag.*, [4], vol. xlv., p. 153):—"The interesting discovery that by means of columns of liquids flowing downwards a more perfect vacuum can be produced than was possible by the air-pumps hitherto in use, belongs solely and only to Dr. Sprengel." He, in his "Researches on the Vacuum" (*Journ. Chem. Soc.* 1865) brings prominently forward that water is, from a practical point of view, the only liquid which could come into consideration as a substitute for mercury, used in the instrument described by him. Bunsen adds, further on,—"If, in the face of these facts, which are open to all, anyone attributes to me . . . a share in his [Dr. Sprengel's] discovery, I can regret this only all the more keenly, as in my treatise on the new method of filtration I could not possibly have expressed myself with regard to Dr. Sprengel's claims more loyally and precisely than I have done."

Bunsen's fame does not require to be supported by injustice towards a fellow-worker to whom we are all so much indebted. Let me advise Mr. W. Thomson and others to read carefully Sprengel's paper, and, in future, when the "water-pump" or "vacuum-pump," which is used with Bunsen's filter, is spoken of, let it, instead of being linked with the name of anyone who may happen to apply it to a particular purpose, be associated with the name of the true discoverer, Sprengel,—I am, &c.,

F.C.S.

NOTES ON AQUEOUS ALCOHOLS.

To the Editor of the Chemical News.

SIR,—In reference to a remark which appears in the report of Messrs. Dittmar and Stewart's "Notes on Aqueous Alcohols," allow me to state that Dupré's alcohol was not largely contaminated by acetone. The methylic alcohol employed had been most carefully prepared, and 20 grms. of it, when oxidised with sulphuric acid and potassium dichromate, yielded only traces of acetic acid.

A. DUPRÉ.

Laboratory, Westminster Hospital,
London, February 7, 1876.

ON THE NECESSITY FOR ORGANISATION AMONGST CHEMISTS.

To the Editor of the Chemical News.

SIR,—I read with deep interest the article by C. R. Alder Wright, D.Sc., in the *CHEMICAL NEWS*, vol. xxxiii., p. 27, and hope to find that the proposal it contains to obtain a charter to constitute a Guild of Analysts and Assayers will meet with an immediate and hearty response by all who follow those professions. It is a fact—however much the necessity is to be deplored—that some such means of both excluding and extruding incompetent persons from the practice of commercial analysis cannot be much longer delayed if conscientious and experienced men are to win a living and save themselves from the aggravation of having their carefully executed assays placed side by side with the results of the rough and ready, not to say slipshod, work of every novice. Dr. Wright very ably advocates the necessity for organisation amongst professional chemists, and his paper deserves their serious reflection. I trust other qualified gentlemen will kindly come forward and assist him to put the proposed association into operation.—I am, &c.

MAITRE ESSAYER.

Sheffield.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Seances de l'Academie des Sciences. No. 3, January 17, 1876.

Action of Fuming Sulphuric Acid upon the Carbides of Hydrogen.—M. Berthelot.—The author remarks that the action of sulphuric acid upon organic compounds gives rise to five principal phenomena; that is to say, combination of the acid with the organic matter; dehydration, or consecutively hydration of the organic matter; its polymeric condensation; and, finally, its profound destruction, with liberation of sulphurous acid. Without stopping to consider the last result, which is generally complicated with the four others, he examines the evolution of heat corresponding to the former.

Transformations of Cane-Sugar in Crude Sugars and in the Sugar-Cane.—M. A. Müntz.—The sugar possessed of reducing properties existing in crude sugars and in the cane ordinarily consists of an inactive glucose, with which are often associated variable proportions of normal glucose and of levulose.

Optical Inactivity of the Reducing Sugar Found in Commercial Products.—MM. Aimé Girard and Laborde.—The authors consider it established that there exists in commercial products a reducing sugar having no sensible influence upon polarised light, and consequently incapable of influencing the results furnished by the polarimeter.

Spectrum of Nitrogen, and on those of the Alkaline Metals in Geissler's Tubes.—M. G. Salet.—The author, with reference to the researches of Schuster, published in 1872, proposes to demonstrate that a grooved or fluted spectrum can be produced with nitrogen heated in contact with sodium; that the disappearance of the spectrum of nitrogen is due to the disappearance of the nitrogen itself, which is totally absorbed by sodium under the influence of the electric effluve; and that the spectrum described by Schuster should probably be ascribed to the vapours of the alkaline metal.

Certain New Derivatives of Anethol.—M. F. Landolph.—The author examines the reaction of alcoholic potassa upon the hydride of anethol; of perchloride of phosphorus upon anethol; of alcoholic potassa upon monochlorinised anethol; and of acetic ether upon diphenol.

Synthesis of Aniline-Black.—M. J. J. Coquillion.—To demonstrate that aniline-black may be obtained without the intervention of a metal the author had recourse to the following precautions:—The slips of carbon which served as electrodes were exposed for three hours to a current of chlorine in a porcelain tube heated to redness. They were then boiled in nitric acid, again submitted to the action of chlorine, and washed in distilled water, when they might be regarded as pure. These points were 1 decimetre in length. To effect the electrolysis two platinum wires were coiled round their upper parts, and were connected with the two Bunsen elements made use of in these experiments. As soon as the lower extremities of the carbon points were plunged in the salt of aniline the positive electrode became covered with black, whilst hydrogen escaped from the negative pole. It seems, therefore, beyond doubt that aniline-black may be produced without the action of any metal. This fact being established, it remains to be seen which salts of aniline are capable of yielding aniline-black. The hydrochlorate and the sulphate alone seem able to produce the black under practical conditions. The author has previously shown that these two salts, when submitted to electrolysis, yield, after the lapse of twenty-four hours, a paste-like mass surrounding the positive electrode. This mass, when

washed and dried, is soluble in concentrated sulphuric acid. It has a blackish violet tint, analogous to a solution of violaniline in the same acid; but if water be added to the dissolved black a greenish mass is immediately precipitated—a phenomenon which does not occur in case of violaniline. This is an important character which seems to distinguish aniline-black. This reaction may be obtained even with a slip of dyed cotton. The greenish flakes, however, resume their original black colour if the acid is neutralised with ammonia or potash. Two other salts of aniline, the arseniate and the phosphate, or rather a mixture of phosphates, likewise yield aniline-black. With two Bunsen elements, however, the operation is slow and difficult. The solution of these salts is syrupy, and after the lapse of twelve hours there are obtained merely small quantities of a black, which likewise is soluble in concentrated sulphuric acid with a red-violet colour, and on adding water deposits greenish flakes. The colours, however, do not appear to be identical with those obtained from the hydrochlorate and the sulphate. These salts are not likely to be used in practice. The black from the nitrate of aniline, and that from the acetate, do not present this reaction, and their molecular constitution is probably different. Thus, from a theoretical point of view, we see that it is possible to form aniline-black by direct synthesis, and that the same method may doubtless realise analogous synthesis. From a practical point of view, the results are also not without importance. For the success of the operation the solutions ought to be concentrated. Practical men should therefore add as little water as possible, and keep within the limits which experience will easily indicate. The other laws of electrolysis have also their application. Every cause which tends to separate the molecules assists the reaction; a more elevated temperature will therefore be favourable, but to ensure uniformity of shade the temperature must be uniform also. A diminution of pressure will have an analogous effect. The printer must therefore beware of employing, as was formerly done, cast-iron drums, where the gases from the reaction, finding no escape, augment the pressure, and thus hinder the formation of the black.

Bulletin de la Societe Chimique de Paris,
No. 12, December 20, 1875.

Spontaneous Coagulation of Blood: Gases of the Blood Before and After the Production of the Fibrin.—M. A. Gautier.—A reply to the last paper of MM. Mathieu and Urbain.

Combination of Phenol with Sulphate of Quinine.—M. S. Cotton.—This compound is remarkable for its stability, and possesses a considerable antiseptic power.

Preparation of Acetate of Ammonia and Acetamide.—M. J. A. Roorda Smit.—Crystallisable acetic acid is placed in a flask and heated in the water-bath, small fragments of carbonate of ammonia being added to saturation. In preparing acetamide the products distilled below 200°, which are always acid, are to be heated and neutralised with carbonate of ammonia.

Reaction of Sulphite of Ammonia and Nitrobenzine.—J. A. Roorda Smit.—The products obtained are sulphanilate and disulphanilate of ammonia.

Correspondence from St. Petersburg of Nov. 21st (Session of the Russian Chemical Society, Oct. 2/14, 1875).—W. Longuinine.

M. Lioubavine gave an account of the changes undergone by aldehyd-ammonia exposed to the air, and of the action of trimethyl-aniline upon aldehyd.

MM. Beilstein and Kourbatoff announced that in the reaction of perchloride of antimony upon nitro-benzine in presence of ether, metachloro-nitro-benzine is produced.

M. Borodine made a communication on the nitroso-amarine obtained by the action of nitrite of potassium upon a solution of amarine in presence of acetic acid.

M. Schmit, on behalf of M. Orłowski, gave an account of experiments undertaken to obtain the tricarbonated

acid intermediate between the methyl-tricarbonic and the carballylic acids.

The *Transactions* of the Society further contain a paper on the order in which the addition and separation of the elements of hydriodic acid take place, by M. A. Zaytzeff. There is also a memoir by MM. Wagner and Zaytzeff, on the "Bromide of Amylene and the Amylenic Glycol of Diethyl Carbinol." Lastly, there is a paper by the same authors on the "Transformation of Diethyl-Carbinol into Methyl-propyl Carbinol," connected with previous researches of Messrs. Erlenmeyer and Wanklyn.

No. 1, January 5, 1876.

Saccharification of Amylaceous Matters.—M. L. Bondonneau.—Already noticed.

On Active Malic Acid.—G. J. W. Bremer.—Malic acid derived from dextro-tartaric acid causes the polarisation plane of light to deviate to the right, its rotatory power being +3.157°. Malic acid from the mountain ash has the rotatory power -3.299°.

On a Secondary Hexylic Alcohol.—W. Echsner de Koninck.—Already noticed.

Sulphocyanates of Acid Radicals.—P. Miquel.—The author has prepared the sulphocyanate of benzoyl, C_6H_5ONS , a colourless liquid, of an odour resembling that of bitter almonds, and the sulphocyanate of acetyl, C_2H_5NOS .

On Camphic Acid.—J. de Montgolfier.

Isomers of Camphor and Borneol.—J. de Montgolfier.—These two papers are not suitable for abstraction.

Mineral Cotton.—H. Meidinger.—This substance, used for coating boilers, steam-pipes, &c., to prevent cooling, is made by passing a current of steam through melted slag. The utmost length of the fibre is 5 c.m. It is a very bad conductor of heat, whence its application. Its price is 7½ francs per 50 kilos.—*Chem. Centralblatt*.

Improvements in Producing Aniline-Black in Dyeing and Printing.—R. Pinckney.—In dyeing the author steeps the goods in the following ingredients, either all together, or first in the solution of the metallic salts, and then in the salt of aniline and the chlorate:—

Hydrochlorate of aniline	150 parts
Salt of uranium or vanadium	18.5 "
Chloride of nickel	20 "
Chlorate of potash	100 "
Water	2500 "

The goods may be dyed either cold or hot. In printing, the proportions employed differ in the chlorate, which varies from 150 to 100 parts, and in the water, which is reduced to 1200 parts. The mixture is thickened with gum or dextrin.—*Moniteur de la Teinture*. [We fear these mixtures will prove too costly for use.]

Formation of Aniline-Black by the Electrolysis of its Salts.—J. J. Coquillion.—Taken from *Comptes Rendus*, lxxxii., p. 408, and already noticed.

M. Reimann's Farber Zeitung, No 4, 1876.

According to R. Wagner resorcin mixed with solution of sulphate of copper, and enough ammonia to re-dissolve the precipitate which is at first produced, yields a deep black liquid which dyes wool and silk, and which may possibly be used as black ink.

Adulteration of Eosin.—Eosin has recently been discovered to be sophisticated with starch, which is less easily recognised than sugar when the sample is treated with water. It is therefore recommended to drench a portion of the eosin with alcohol at 96 per cent, which ought to give a clear solution. The absence of other red colouring matters is ascertained afterwards.

To distinguish eosin from the coal-tar colours, and from alizarin-red, Wagner proposes to moisten the tissues with collodion. If a white spot appears the dye employed is eosin.

PATENTS.

ABRIDGMENTS OF PROVISIONAL AND COMPLETE SPECIFICATIONS.

Improvements in artificial stone. J. C. Sellars, Birkenhead, Chester. December 7, 1874.—No. 4195. This relates to that class of artificial stone made into blocks, bricks, tiles, or other forms by subjecting to pressure or by moulding into form mixed siliceous sand or other granular substances and Portland or other cement, powdered lime, or clay, and consists essentially, as means for hardening and rendering said stone more durable, in wetting or soaking the said blocks, bricks, tiles, or other forms after pressure or moulding, and after they have been exposed to the atmosphere for some time, with or in water containing or holding lime in solution or suspension.

Improvements in the manufacture of gas. R. P. Spice, Parliament Street, Westminster. December 18, 1874.—No. 4365. This invention relates to the manufacture of illuminating gas when what is commonly known as "water gas" is used in combination with the vapour of petroleum or other similar spirit or oil. The apparatus to be employed is not necessarily, but greatly by preference, that for which Letters Patent, No. 4178, dated December 19, 1873, were granted to me; and the present improvements consist in causing the gas immediately after it has left the condenser, or direct from the retorts, and while it is hot, to pass through a vessel containing petroleum, whereby not only the light but the heavier spirits are vapourised and caused to combine with the water gas. By these means, in conjunction with those described by me in a Provisional Specification, No. 3269, dated September 24, 1874, the whole of the vapourisable portion of the ordinary petroleum of commerce may be utilised, and the object of the present invention attained. The gas after passing through the petroleum may be conducted to the condenser if necessary, and subsequently to the purifying and storing apparatus in the ordinary way.

Improvements in the processes of and apparatus for treating and purifying the refuse "pickle" of tin-plate works, also in treating the sulphuric acid obtained, and in purifying the "scouring-water" used at such works. W. J. Pughsley, Kidwelly, Carmarthen. December 19, 1874.—No. 4373. This invention relates to certain improvements in connection with the invention for which Letters Patent were granted to me on the 21st day of June, 1864, No. 1530. In lieu of bottling the sulphuric acid obtained from the refuse "pickle," according to my said invention, direct from the lead tank for the use of the picklers, by the present improvements I cause it to run into the upper compartment or cistern of a filter through a suitable receiving pipe, and from which it escapes (through a coarse flannel strainer having a perforated copper plate at its top) to and permeates a deep layer of charcoal in the lower compartment, and then through a layer of small pebble stones obtained from fresh water, and lastly escapes from the filter through a perforated wooden chamber, and a wood tap fixed therein, into a glass bottle, and it is then ready for re-use by white or black picklers as previously. The discolouration of river- or other stream-water adjacent to tin-plate works is caused by the "scouring water" used at such works, in a tank containing which the plates are placed, after having been taken out of the "pickle," until the acid is removed from them. To prevent this discolouration I construct a filtering apparatus in the watercourse of the "scouring-water," and cause the latter to pass first through a layer of limestone, which will retain any dirt that may have accumulated. The filter I construct of bricks, and form it of an upper layer of charcoal separated by a perforated elm plank from a lower layer of burnt bones. The bones absorb the acid, and prevent any from escaping to the stream. The white pickle I run into a tank (instead of into the river or stream), in which I allow it to settle, and subsequently over into a filtering apparatus of the construction hereafter described, and thence to the black plate picklers to be re-used, mixed with best vitriol. Subsequently I cause all the black and white pickle to pass through the processes described in the Specification of my above referred to Letters Patent, so as again to obtain the sulphuric acid for re-use. The tank lastly above referred to may be made of brick, or of any suitable substitute, such as a palm-oil cask, arranged so as to receive the pickle from the pickler's pot. The filtering apparatus I construct of wood, with a filtering medium of charcoal only. I also provide it with a wood tap, by which the filtered acid can be withdrawn for the black picklers.

New or improved apparatus which may be used for condensing vapours or gases, for heating liquids, for purifying and bleaching oils and gases, and for other analogous purposes. R. Speir, Greenock, Renfrew, N.B., and J. Mather, Gateshead-on-Tyne, Durham. December 21, 1874.—No. 4386. The feature of novelty which constitutes this invention is the arrangement and construction of the apparatus.

Improvements in the manufacture of hydrogen gas, and in the utilisation of the by- or secondary products obtained in such manufacture. J. H. Johnson, Lincoln's Inn Fields, Middlesex. (A communication from J. O. E. Moreau, Paris.) December 21, 1874.—No. 4387. This invention relates to the application in the manufacture of hydrogen gas suitable for illuminating and heating purposes of a material not hitherto used in such manufacture, whereby a considerable saving is effected in the cost of production, and (in the case of lighting-gas) greater illuminating power is obtained; and it consists in the employment of the slime or ooze composed of the detritus of floating plants, aquatic vegetables, and the like, which settle on the bed or bottom of rivers, lakes, and similar watercourses or sheets of water, and there collect, forming strata or layers, which in a short time become decomposed. The invention also relates to the utilisation of the by- or secondary products in the manufacture of coke, artificial fuel, manure, and disinfectants.

NOTES AND QUERIES.

** Our Notes and Queries column was opened for the purpose of giving and obtaining information likely to be of use to our readers generally. We cannot undertake to let this column be the means of transmitting merely private information, or such trade notices as should legitimately come in the advertising columns.

Dry Rot.—Can any of your correspondents inform me if there is any preparation that can be applied to timber in a house to prevent the spread of dry rot, and what is the best method to pursue?—DRY ROT.

Black-Ash Making.—Will some correspondent be kind enough to inform me what weights of salt cake, limestone, and slack are found in practice the best? what is the actual loss of salt cake in black-ash making? and what, if any, is the loss of alkali usually found in lixiviating? What percentage of sodium oxide should be found in an average ball made from whatever is considered to be an average charge?—ENQUIRER.

MEETINGS FOR THE WEEK.

SATURDAY, Feb. 12th.—Physical, 3. (Annual General Meeting. Election of Officers, &c.)

MONDAY, 14th.—Medical, 8.

— London Institution, 5.

— Royal Geographical, 8.30.

— Society of Arts, 8. Cantor Lectures. "Iron and Steel Manufacture," by W. Mattieu Williams, F.C.S.

TUESDAY, 15th.—Civil Engineers, 8.

— Zoological, 8.30.

— Royal Institution, 3. "On the Classification of the Vertebrated Animals," by Prof. Garrod.

— Society of Arts, 8. (African Section). "Ostrich Farming and the Ostrich Feather Trade of South Africa," by P. L. Simmonds, F.S.S.

WEDNESDAY, 16th.—Society of Arts, 8. "The Combustion of Gas, and its Application to Heating Purposes," by John Wallace.

— Meteorological, 7. "An Improvement in Aneroid Barometers," by the Hon. Ralph Abercromby, F.M.S. "Meteorology in India in relation to Cholera," by Colonel J. Puckle, M.S.C.

— Society of Public Analysts, 6.30.

THURSDAY, 17th.—Royal, 8.30.

— Royal Institution, 3. "On the Chemistry of the Non-Metallic Elements," by Prof. Gladstone.

— Chemical, 8. "On some Points in the Analysis of Potable Waters," by Dr. Frankland.

— Royal Society Club, 6.30.

— Zoological, 4.

— London Institution, 7.

FRIDAY, 18th.—Royal Institution, 9. "Action of Light on Selenium," by C. W. Siemens.

— Society of Arts, 8. Indian Section. "Suez Canal," by C. Magniac.

— Geological, 1. (Anniversary).

SATURDAY, 19th.—Royal Institution, 3. "On the Vegetable Kingdom," by W. Thiselton Dyer.

ROYAL SCHOOL OF MINES.

PROF. RAMSAY, LL.D., F.R.S., will commence a Course of Forty Lectures on GEOLOGY, on Monday next, February 14, at Two o'clock, to be continued on each succeeding Tuesday, Wednesday, Thursday, and Monday at the same hour. Fee for the Course £4.

MR. WARINGTON W. SMYTH, F.R.S., will commence a Course of Forty Lectures on MINERALOGY on Monday next, February 14, at Noon, to be continued on each succeeding Tuesday, Thursday, Friday, and Monday at the same hour. Fee for the Course £4.

TRENHAM REEKS, Registrar.

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THE CHEMICAL NEWS.

VOL. XXXII. No. 847.

NEW SULPHURETTED HYDROGEN GENERATOR.

By P. CASAMAJOR.

THE gas generator, which it is my purpose to describe here, has been in use for several months. It possesses two important characteristics, which, as far as I am aware, are not found combined in any apparatus suitable for producing large quantities of gas. These characteristics are:—

1. That it gives sulphuretted hydrogen immediately whenever it is wanted.
2. That it does *not* give it when it is *not* wanted, except for a few minutes after being used.

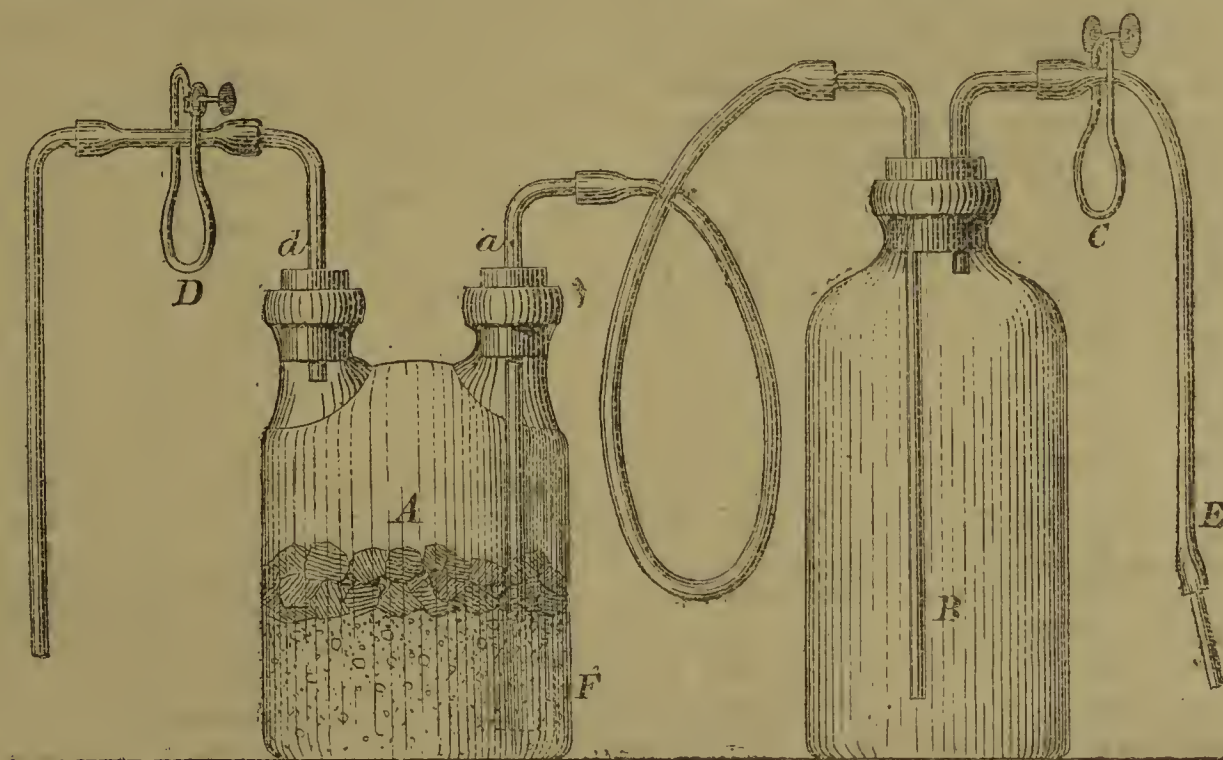
Kipp's apparatus, which is the one most generally used for the production of sulphuretted hydrogen, fulfils the condition of being ready to give off the gas at any time,

This plan is troublesome and inconvenient. If particular care is not taken to wash the sulphuret of iron with water and weak carbonate of soda, and drying it well before putting it away, it will be found oxidised and useless when wanted.

Besides these two generators, which are in general use, others have been proposed, of which I can say that those which have come under my notice have either failed in the important requisite of ceasing to give sulphuretted hydrogen when it is *not* wanted, or are ill adapted to the production of gas in any considerable quantity.

The new apparatus for generating sulphuretted hydrogen is represented in the woodcut accompanying this article. The bottle marked A is provided with two tubulures, through one of which passes a glass tube, ending at its lowest part in an enlarged portion F, which must, however, be narrow enough to pass through one of the tubulures of the bottle A. This tube must pass through a rubber cork capable of closing the tubulure *a* perfectly.

Before introducing the tube through the tubulure *a*, the enlarged portion is filled with some coarse fibrous material, such as coarse tow. After the tubulure *a* shall have been perfectly closed by the rubber cork, a quantity of shot (about No. 3) is poured into the other tubulure so that it will rise in the bottle to a height of two or three inches. After levelling the shot, pieces of sulphuret of iron are



but it has the drawback of being an almost constant generator of gas, which defect is inherent to its construction. One cause of this is that the sulphuret of iron is placed on a wire gauze directly over the dilute sulphuric acid in the lowest globe, and that it keeps falling through and around the wire gauze, causing a constant production of gas, which must eventually escape either through the glass stopcock, or through the safety tube on top of the highest globe.

Another cause of the constant production of gas is that, when the stopcock is closed, the dilute sulphuric acid is driven back to a height of about 15 inches. This maintains a pressure in the apparatus which forces the gas out at some part of the ground joints. After a certain portion of the gas has escaped, the dilute sulphuric acid rises in the lowest globe so that it reaches the sulphuret of iron on the sieve, and a fresh supply of gas is produced, driving the liquid to the upper globe as before, and re-establishing the pressure in the apparatus.

The use of Kipp's apparatus having been found inconvenient, I adopted for several years the plan of putting up an apparatus when one was wanted, emptying it out again when not required for use. This generator was merely a bottle with two tubes, one for the egress of the gas, and the other for the introduction of dilute sulphuric acid, which latter served also as a safety tube.

introduced in the bottle, where they will lie on top of the shot.

The presence of shot in contact with the lower part of the tube before mentioned accounts for the necessity of providing this tube with an enlarged portion F, as by this means sufficient space is left for the passage of liquid between the grains of shot, which otherwise would close almost entirely the lower end of the tube. The object of stuffing the enlargement F with tow is to prevent the shot from rising up to the narrow portion of the tube.

After the shot and sulphuret of iron have been introduced, the other tubulure *b* is closed tightly with a rubber cork provided with a tube to lead the gas generated to a wash bottle. The two portions forming the outlet tube are united by a rubber tube, which may be entirely closed by means of the screw pinchcock D.

The bottle B is provided with a wide mouth, bearing a rubber cork with two tubes, one of which extends down to the bottom of B and communicates by means of a flexible rubber tube to the glass tube which enters into the bottle A through the tubulure *a*. The other glass tube of bottle B terminates in a flexible rubber tube which may be tightly closed by means of the pinchcock C. The bottle B is filled to about two-thirds of its height with dilute sulphuric acid, which is allowed to go to the bottle A whenever the gas is to be generated, and which

returns to the bottle B when the apparatus is not in use.

To generate sulphuretted hydrogen with this apparatus, we may observe that if the tubes by which the bottle A communicates to the bottle B are full of liquid, it will merely be necessary to open the screw pinchcock D, which will remove the pressure from the bottle A and allow the dilute sulphuric acid in bottle B to flow into bottle A. If these tubes of communication, including the flexible tube, are not full of liquid, the screw pinch D should be kept open, and air be driven into the bottle B from the mouth through the tube E by opening the pinchcock C. The pressure exerted in this way on the surface of the bottle B drives the liquid it contains into the tubes of communication, and, after the blowing of air through the tube E has ceased, the liquid continues to flow into A until it reaches the sulphuret of iron, when sulphuretted hydrogen is given off.

On account of the offensive nature of the gas, care should be taken not to draw air from the bottle B into the mouth. This is easily avoided by filling the mouth and lungs with air before blowing into the bottle B. If care is not taken to open the screw pinchcock D before blowing, the gas in the bottle will not be driven forward, but will be mixed with the air from the lungs, and partly find its way into the mouth of the operator. This screw pinchcock D is specially useful in regulating the outlet of sulphuretted hydrogen, and consequently its production.

When no more gas is wanted the screw pinchcock should be closed entirely, after which a certain pressure is produced in the apparatus from the gas which continues to be formed. After a minute or two the pinchcock C should be opened to remove the pressure from B, and allow, not only the liquid in A to flow back into B, but also a certain quantity of gas, by which means the liquid connection between the two bottles is interrupted, and remains so while the apparatus is not in use.

471, Lafayette Avenue,
Brooklyn, December 20, 1875.

ON THE USE OF THE SPRENGEL VACUUM PUMP FOR FILLING BAROMETER AND THERMOMETER TUBES WITH MERCURY.

By ERNEST FRANCIS,
Government Laboratory, Trinidad, B.W.I.

THE difficulty of filling tubes with mercury so that air may be excluded is well known, and instruments in which this condition is attained are highly prized. The ordinary process of filling barometers by boiling is tedious and unsatisfactory, more especially to those unused to the operation.

It has been found that the improved form of the Sprengel pump affords an admirable means of accomplishing the operation, and adds another to the numerous good qualities for which the instrument is famed. The process is easy and would enable barometers to be filled in the laboratory with perfect accuracy. It has the additional advantage of being applicable to tubes of any calibre.

The operation is performed by connecting and exhausting the barometer tube; the outflow orifice of the pump being then stopped, mercury passes in and fills the exhausted tube.

Further details may be gathered from the accompanying diagram, but the arrangement would vary slightly with the shape of the tube to be filled. The diagram shows a Bunsen's syphon barometer, connected to the pump at A by vulcanised tubing, with the joint surrounded by a tube filled with mercury. After exhaustion the end of the pump C is closed either with the finger or by a specially furnished clamp or stopcock. The mercury which is kept flowing from the reservoir then ascends and completely fills the barometer. The mercury falls over the bend to

the point B, but without sufficient force to break the tube. The inflow of mercury is regulated by the clamp D. When full the barometer can be safely disconnected with a little care, and the excess of mercury poured out. For filling straight tubes the part A can be bent and connected to the barometer inclined downwards. It is almost needless to add that the barometer during the filling must be supported by a clip or otherwise.



A barometer filled in this manner answered every test most satisfactorily. The tube became completely filled with clear bright mercury, no trace of air being visible at any part. Tested by repeated gentle tiltings it gave no dull sound, and finally the vacuous part being surrounded by hot water, produced no alteration in the height of the mercurial column.

In conclusion I would suggest that the same process might prove satisfactory for filling *thermometer* and other tubes with mercury.

December 30, 1875.

ON THE NATURE AND ORIGIN OF METEORITES.

By DR. MOHR.

Of this long and valuable memoir we can only insert a few portions:—

If we compare all the facts it appears as the final result that the meteoric silicates and iron masses have been formed simultaneously in the moist way, and the iron by reduction effected by organic bodies. The reasons for this view are as follows:—

1. The silicates contain small quantities of water.
2. The silicates decrease in specific gravity by strong heating and fusion.
3. Different silicates are crystallised together; some parts being soluble in hydrochloric acid, and other parts are not, as in basalts and phonolites.
4. The olivin, containing protoxide of iron, is green, and not black.
5. The igneous crust is black in contradistinction to the interior of the meteorite.

6. Certain meteorites contain organic bodies, analogous to terrestrial hydrocarbons.

7. Meteoric iron contains no chemically combined carbon, even when graphite is present.

8. The sulphide of iron is contained in single particles distinct from the iron, and not diffused through the whole mass.

9. Schreibersite, composed of phosphorus, iron, and nickel, is likewise found in distinct particles.

10. Brittle meteoric iron becomes soft by ignition if no sulphide of iron is present.

11. Malleable meteoric iron, containing sulphide of iron, becomes hot-short on fusion.

12. Meteoric iron, if heated to whiteness in a vacuum, evolves hydrogen.

13. The "Figures of Widmannstätt" give proof of an undisturbed crystallisation.

14. No silicium is present which agrees with theory, since silica cannot be reduced by organic bodies.

The author here solicits possessors of meteorites to forward him small fragments, &c., of no value as specimens, for the purpose of extending his observations. He next proceeds to the question of the origin of meteorites.

The view formerly maintained that they were projectiles from the moon, in which it was supposed that volcanoes were recognised, is quite untenable. It is fatal to this theory that the meteorites coincide with the periodically recurring swarm of shooting stars, which have a planetary orbit in space, and also, that, as appears from the above, they display no igneous structure, and cannot, consequently, have sprung from a volcano. That such things can have been formed in the air is a notion* built of air.

The constituents of meteors, such as olivin, augite, anorthite, and their organic matter prove that these bodies must have been formed upon a planet, warmed by the sun, or by a sun in absolute rest, and in the lapse of an enormous length of time, like the terrestrial silicates. Under what circumstances this planet has been shivered in fragments does not appear. It must have had a large collection of waters, a sea, which has likewise been dispersed, and which now is to be found in meteoric swarms, and in comets, as already shown.

The author maintains with Galle and Förster (see *Pogg. Annalen*, 148, 172) that the shooting stars of November 27, 1872, consisted of particles of Biela's comet, whose orbital plane was intersected by the earth at that precise time, and whose direction agreed within a degree with that of the meteors.

It is striking that such cosmic bodies as we can take in hand and examine, namely the earth and meteorites, show not the smallest trace of an igneous formation if we regard volcanoes, and the fiery crust of meteorites as subsequent modifications, and not as original features.

The peculiarity of meteorites as compared with our globe, consists in the circumstance that we find in the former more products of reduction, and except the earths, no perfect oxides. Thus in meteorites we find no ferric oxide, but metallic iron, sulphide of iron, and phosphide of nickel-iron. Upon our globe phosphorus occurs only as phosphoric acid. Hence the hypothetical planet where the meteorites originated must have been smaller than our globe, and have had a less dense atmosphere containing less free oxygen. The sp. gr. of most meteorites, 3.275, agrees with the calculated density of the planetoids between Mars and Jupiter.

Dr. Mohr does not accept the view of J. R. Meyer that the heat of the sun is maintained by the infall of meteorites. He considers that in an infinite universe, filled with radiating suns, our sun can lose nothing which it does not receive back every moment from its fellows, since the void space of the universe has been for infinite ages filled with that sum-total of rays which it is capable of receiving.—*Liebig's Annalen der Chemie*.

* "Aus der Luft gegriffen,"—a common German phrase for any view lacking a substantial basis.

REPORT ON THE DEVELOPMENT OF THE CHEMICAL ARTS DURING THE LAST TEN YEARS.*

By Dr. A. W. HOFMANN.

(Continued from p. 58.)

FOR the generation of cold by evaporation liquids are most suitable which require a technical preparation and possess a considerable value. In the manufacture of ice on the large scale it is therefore needful to restore the escaping vapours to their original condition, *i.e.*, liquids capable of re-evaporation so that a given quantity of material may serve again and again, circulating continually. This restoration can be effected by two processes different in form and action of which the ether machine and the ammonia machine are respectively almost the sole existing representatives.

The ether machine is arranged as follows:—A double-action air-pump worked by some especial source of power (generally a steam-engine) draws incessantly the vapour of ether out of a vessel filled with liquid ether (ice-generator or evaporation-receiver.) By the return of the piston the vapour is compressed and driven into a worm cooled by water. As the vapour which has been heated by compression cools, it condenses to a liquid which is collected in a suitable vessel whence it is driven by the pressure of the condensed vapour back into the evaporation-receiver where it recommences its function.

The principle of the ether machine was patented in England by Jac. Perkins, of London, as early as 1834. His apparatus contains all the parts requisite for continuous action—evaporation-receiver, air-pump, and worm-condenser. The first-mentioned part, according to the drawings, consists of a vessel like a boiler formed of two segments of a sphere and surrounded with water. This arrangement is not very suitable, possibly the reason that nothing further has been heard of the development of the apparatus. Or, possibly the time was not yet come for the utilisation of the principle, the demand for ice being not important enough to render it a remunerative business.

The next patent for an ether ice machine was taken out in 1856 by John Harrison, of Geelong, in Victoria. In September, 1857, he obtained a patent for improvements, according to which latter the machine is arranged as follows:—The evaporator has the form of a horizontal tubular boiler, with numerous narrow tubes. Through these tubes a concentrated solution of common salt which is pumped up at the top streams down in a zigzag direction, the tubes being divided in three sets from above downwards. The ethereal liquid streams out of the condenser into the boiler outside the tubes. The solution of common salt passes from the boiler into a long tank in which are suspended vessels of the water to be frozen (ice-boxes), passes through it, and is pumped up again into the boiler. The arrangement is perfectly rational. Harrison states in his specification that he can, by means of his machine, produce a temperature of -29° ; but from an economical point of view he prefers -2° to -5° . The process of freezing is then slower, but the expenditure of power is much less, and the ice is transparent like natural ice. At the end of the year 1859 Lawrence established works at Liverpool for the production of artificial ice, and sold it at one halfpenny per lb. Dullo† and Grünberg‡ have described the process, the latter with illustrations. From 40 to 60 cwts. of ice were prepared daily by means of a steam-engine of 15 horse-power. In 1860 Laboulay§ described an ether ice machine by F. Carré, of Paris. In

* "Berichte über die Entwicklung der Chemischen Industrie Während des Letzten Jahrzehends."

† Dullo, *Dingl. Journ.*, clviii., 115.

‡ Grünberg, *Pol. Centralbl.*, 1863, 656.

§ Laboulay, *Bull. Soc. d'Enc.*, 1860, 129.

it the ether acted directly upon the water to be frozen.* It was soon abandoned by Carré after he had succeeded in carrying out the ammonia machine, which is far more efficacious. In March, 1862, Dr. Siebe, of Lambeth, obtained an English patent for an improved ice machine. The general arrangement is the same as Harrison's. The boiler, instead of being horizontal, is vertical. There are also changes in the air-pump and the cooler, which do not affect the principle. The ice-boxes are so arranged that when the first one, which is exposed to the influx of the cold liquid, is frozen and taken out the entire series slides forwards, and the new box filled with fresh water comes in last. From this date we find Siebe's name alone connected with the machine in question, which, however, is still spoken of as Harrison's principle. Siebe's machine figured at the London Exhibition of 1862.

(To be continued.)

THEORY OF THE PRODUCTION OF ANILINE-BLACK BY MEANS OF THE SALTS OF VANADIUM.

By M. A. GUYARD (HUGO TAMM).

SINCE the interesting discovery of M. Pinkney is known it may be permissible to say a few words on the question of the formation of aniline-black, hitherto obscure, but which the reaction of the salts of vanadium completely elucidates.

If we introduce into a normal mixture for aniline-black, made up of water 100 grms., hydrochlorate of aniline 8 grms., chlorate of potash or soda 3.5 to 4 grs., about 1 centigram. of vanadous chloride or vanadate of ammonia, we see with surprise the liquor darken in a few moments, and then gradually deposit an abundant precipitate of aniline-black. After about forty-eight hours the reaction is nearly complete, and the liquor is taken up in a thick paste, almost solid, in consequence of the formation of almost all the aniline-black which it is able to furnish. This reaction is so delicate that 1 part of vanadous chloride transforms 1000 parts of hydrochlorate of aniline into aniline-black, and that in practice 500 parts may be thus advantageously transformed by 1 part either of the chloride or of the vanadate of ammonia. This important discovery renders dyeing with aniline-black as easy as printing, and nothing can equal the beauty of the blacks thus obtained. Since the discoveries of Lightfoot and Lauth there is no reaction which is calculated so much to generalise the use of aniline-black both in dyeing and printing.

But this reaction is chiefly interesting in a chemical point of view. It is one of the most elegant reactions of chemistry. The author thinks he may give its true image by saying that the vanadium is a *fluid* spark which determines the combustion of the fluid mass of salts of aniline and of chlorate. We make aniline-black by means of a drop of a salt of vanadium, just as we set fire to fuel by means of a match. The power of the vanadium salts in the production of aniline black is more than a thousand times greater than that of copper. The reason of this is intelligible if we know the properties of vanadium. There is no metal which passes more readily from the lowest to the highest stage of oxidation and returns again to the lowest. Under the feeblest reducing influences vanadic oxide becomes vanadous oxide, and under the faintest oxidising influences vanadous oxide is re-converted into the vanadic. This is the whole secret of the power of vanadium—a power so great that the author thought at first he had encountered a new force, or at least one of those mysterious agencies named catalytic; but a closer study of the phenomenon soon led to its explanation. In fact, on dissolving vanadic acid in hydrochloric acid it is transformed into vanadous chloride, and on evaporating

the latter in the air it is partly transformed into vanadic acid. On the other hand, if vanadous chloride and chlorate of potassa are brought in contact, the latter is decomposed with disengagement of chlorine, and the vanadous oxide is transformed into vanadic oxide. Reciprocally, if we introduce vanadic oxide, or an alkaline vanadate, into hydrochlorate of aniline, the vanadous oxide is instantly reduced to the state of vanadous oxide or chloride. If we introduce into a mixture of an aniline salt and of a chlorate 1-1000th of vanadous chloride or of a vanadate aniline-black is produced with the same energy. In fact, in the twofold contact with the oxidising salt and the reducing organic compound the vanadium passes with the speed of an electric spark from the state of vanadic acid to that of vanadous oxide, and reciprocally as long as there remains a trace of aniline to oxidise or of chlorate to decompose.

In order that a metal may aid in the production of aniline-black it must possess at least two degrees of oxidation in the moist way. Thus potassium, sodium, lithium, calcium, magnesium, barium, aluminium, zinc, cadmium, lead, silver, and all analogous metals, are incapable of taking part in the production of aniline-black.

But if a metal has two stages of oxidation in the moist way, it must not pass with too great facility from one of these states to the other.*

Thus protochloride of tin is improper for the preparation of aniline-black. In fact this salt absorbs oxygen and chlorine with such avidity that it decomposes a certain quantity of chlorate of potash, but it absorbs the products of the decomposition, and does not yield even the smallest portion to the aniline. We may thus, at pleasure, retard the formation of aniline-black, even in mixtures containing vanadium. The formation of aniline-black does not begin till all the tin is peroxidised, and when the black makes its appearance it can be made to disappear again by adding fresh quantities of the protochloride of tin. If we take the higher oxide of a metal having two stages of oxidation we may produce bodies analogous to aniline-black, provided that the salt passes readily to the lower oxide. Thus, permanganate, bichromate, ferrate of potassa, and bivanadate of ammonia form with hydrochlorate of aniline bodies analogous to aniline-black, and that without the intervention of chlorates. Alkaline tungstates and molybdates are not favourable to the production of blacks. If we take the lower oxide of a metal having two stages of oxidation we may, in presence of chlorate of potash, obtain aniline-black. The lower oxides of cerium, iron, and manganese are here included, as well as those of nickel, cobalt, and chrome. These latter become peroxidised with difficulty, and are not very fit for the purpose, but in presence of 1-5000th of a salt of vanadium they aid in the formation of black.

The lower oxides of uranium, tungsten, and molybdenum, yield alone very fine blacks. But the higher oxide of uranium, like the tungstates and molybdates, does not act. When the salts of uranium seem to form black, as M. Pinkney considers that he has observed, it is because the salt of uranium has been obtained from a pitch blende containing vanadium.

Copper passes readily from the maximum to the minimum state of oxidation, and still more readily in the inverse direction: consequently, next to vanadium, it is the metal best suited for the production of black, and the one generally employed.

The quantity of salt of vanadium necessary to transform aniline into black is so small that it may be practically disregarded. Hence we infer that metallic salts, though necessary in the formation of aniline-black, do not enter into its constitution. Aniline-black with vanadium is identical with aniline-black with copper, the one containing no vanadium, and the other no copper.

M. Coquillion states that he has obtained aniline-blacks

* This seems scarcely consistent with the previous passage, where the author states that "no metal passes with more facility from the minimum to the maximum state than vanadium."

by electrolysis. The author has obtained them still more readily by introducing into a very concentrated solution of a chlorate and of a salt of aniline a few drops of hydrochloric acid. No metal intervenes, but the hydrochloric acid decomposes the chloric acid; and the products of this decomposition, reacting upon the aniline, transform it into black. In some hours the mass becomes a well characterised paste of aniline-black.

This reaction is of no practical service, because the goods would be destroyed by the concentrated and acid liquids. Nevertheless, it proves that the salts of vanadium and copper serve merely to play in dilute liquids the same part which hydrochloric acid does in concentrated solutions. We may say without hesitation that aniline-black is the result of the action of the decomposition products of chloric acid upon aniline.

The reaction of vanadium enables us to study the behaviour of these decomposition products of chloric acid upon a number of organic bodies, and upon the isomers of aniline. Starch, dextrin, and isinglass are converted into pale yellow substances which do not dye. Extract of logwood, if treated with chlorate of potash and a drop of a salt of vanadium, is transformed into a yellow substance, which dyes silk a splendid gold-yellow. Under the same circumstances the solution of hydrochlorate of toluydin (made from solid toluydin) is transformed into a new substance, which dyes silk a pleasing bronze with coppery lustre.

All these reactions are plainly due to the decomposition products of chloric acid, and have all been reproduced with the substitution of copper for vanadium. However, copper has to be employed in proportions from 1000 to 1500 times greater than vanadium to obtain the same results. Chloride of vanadium is indirectly the best reagent for aniline, and, conversely, a mixture of hydrochlorate of aniline and chlorate of potash is the best reagent for vanadium. If the substance supposed to contain aniline is evaporated with a slight excess of hydrochloric acid, adding chlorate of potash and a drop of a weak solution of vanadous chlorate, aniline-black makes its appearance whatever foreign bodies may be present. (Protochloride of tin?)

On the other hand, the presence of vanadium may be detected by concentrating the solution, acidifying with hydrochloric acid, and treating with a mixture of a salt of aniline and a chlorate. If aniline-black is formed rapidly in the cold the presence of vanadium is a certainty.

The author draws the following conclusions from his examination of aniline-blacks:—

Aniline-black is simply emeraldin dehydrated.

An elevated temperature in the ageing-rooms is necessary, not to form emeraldin, but to dehydrate it, and convert it into black.

Emeraldin may be dehydrated by the application of heat, even in the liquid in which it is formed. In other words, emeraldin is transformed into aniline-black just as the blue hydrous oxide of copper is converted into the black anhydrous oxide by ebullition.

The essential characteristic of hydrated aniline-black, or emeraldin, is that it can be completely dissolved or destroyed by the yellow sulphide of ammonium.

The essential characteristic of anhydrous emeraldin, or fixed aniline-black, is that it is very slightly affected by the sulphide of ammonium. This reagent always points out whether emeraldin has or has not been transformed into black.

The characters just recited are those of the blacks produced from chemically pure aniline—the finest blacks which can be obtained.

The blacks prepared from commercial anilines are formed of emeraldin, mauvein, violanilin, and toluydin bronze.

If the commercial anilines are completely oxidised, the aniline is converted into emeraldin, and the toluydin into bronze. The mauvein and violanilin disappear almost entirely, and the emeraldin, being insoluble in acids and

alcohols, may be separated from the toluydin bronze, which is soluble in the same liquids.

By means of this reaction aniline in commercial aniline oils may be determined, being thrown down as emeraldin, and weighed as aniline-black.

Typical aniline-black is anhydrous emeraldin, but in practice there are as many aniline-blacks as there are mixtures of aniline and its homologues.

The liquid toluydins of commerce behave like mixtures of aniline and of crystalline toluydin, and yield with chlorate of potash and vanadium mixtures of emeraldin and toluydin bronze.

Vanadium will render great service to organic chemistry, and will revolutionise the preparation of aniline-black.—*Bulletin de la Societe Chimique de Paris.*

PROCEEDINGS OF SOCIETIES.

PHYSICAL SOCIETY.

Annual General Meeting, February 12, 1876.

Professor GLADSTONE, F.R.S., President, in the Chair.

THE following candidates were elected Members of the Society:—Mr. W. R. Hodgkinson and Mr. H. M. Hastings.

The PRESIDENT read the Report of the Council, of which the following is an abstract:—

The Council points with satisfaction to the activity with which the work of the Society has been carried on during the year, as is shown by the number of papers read; and special reference is made to Lectures which were delivered by M. Cornu, of Paris, and Mr. J. Norman Lockyer. The election of many distinguished physicists during the past year has given the Council much satisfaction, as it affords undoubted evidence of the progress of the Society and of the position it has now attained. The Society has to regret the loss of two members—Mr. Becker, who died on the 3rd of April, 1875, from bronchitis, in the 54th year of his age; and Mr. Waugh, who died on the 12th of October, from epilepsy, in his 40th year. The Society has already published a work by Prof. Everett, on the Centimetre-Gramme-Second System of Units; and the Council is now in communication with the family of the late Sir Charles Wheatstone, with a view to the publication of his papers. Attention is drawn to the benefit which the Society derives from the use of the lecture-room, &c., which were generously placed at its service by the Lords of the Committee of Council on Education. It has been considered desirable to arrange that the Council may grant admission to all meetings of a Session to approved persons who are not members of the Society. In concluding, the Council records its thanks for the services which Dr. Guthrie has rendered in his office of Demonstrator,—an office which was formerly an important one in the Royal Society,—and the Council believes that much might be gained if arrangements could be made for reproducing before this Society the experiments described in original papers which appear from time to time in this country and abroad.

Several alterations in the Bye-Laws were then discussed and adopted, and the following Officers and Council were elected for the ensuing year:—

President—Prof. G. C. Foster, F.R.S.

Vice-Presidents—Prof. W. G. Adams, F.R.S.; and W. Spottiswoode, LL.D., F.R.S.

Secretaries—A. W. Reinold, M.A.; W. C. Roberts, F.R.S.

Treasurer—Dr. E. Atkinson.

Demonstrator—Dr. F. Guthrie, F.R.S.

Other Members of Council—Latimer Clark, C.E.; Prof. A. Dupré, F.R.S.; W. Huggins, D.C.L., F.R.S.; Prof. H. M'Leod; Dr. C. W. Siemens, D.C.L., F.R.S.; Dr. H. Sprengel; Dr. W. H. Stone; Sir William Thomson,

LL.D., F.R.S.; Prof. W. C. Unwin, B.Sc.; and E. O. W. Whitehouse.

The proceedings then terminated with votes of thanks to the President, the Lords of the Committee of Council on Education, the Demonstrator, Secretaries, and Treasurer.

NOTICES OF BOOKS.

Food; its Adulterations, and the Methods for their Detection. By A. H. HASSALL, M.D. London: Longmans, Green, and Co.

SETTING aside those estimable gentlemen who revere sophistication as an *avatar* of their great goddess, competition, authorities on the adulterations and impurities of food and water may be divided into two schools. One of these—which we shall take leave to call the sensational—loves to go to the very extremity of the truth, and to make statements, not perhaps overcoloured, but at any rate very hard to prove: it lays down hard and fast lines defining what is permissible and what is to be branded as impure, and proposes very stringent measures for the repression of the evil. The other, or common sense school, adopts a totally different system: its adherents keep well within the truth in their statements as to the nature and the extent of adulteration; where there is even the shadow of a doubt they prefer to be silent: they are content, for the present, to attack the grosser, more dangerous, and more distinctly characterised forms of sophistication, and seek gradually to educate the public mind up to a sounder view on commercial purity. Which of these two systems is best adapted to the real exigencies of the case there can be little doubt. Dr. Hassall unfortunately shows a leaning to the former school, which may possibly explain his evident sympathy for the late Rivers Pollution Commission.

The work before us reaches the goodly extent of 896 closely-printed pages. But we shall be greatly mistaken if we suppose that the whole of this matter consists of plain directions for the detection and estimation of impurities in articles of food and drink. The strictly analytical portion is so lost among historical matter, controversial passages, descriptions of well-known articles, and outbursts of virtuous indignation, that we cannot help being reminded of Falstaff's tavern-bill with its halfpenny worth of bread to an intolerable deal of sack. Nor can we discover, in the instructions given, much that is at once valuable and new to the chemical profession. This is the more to be regretted since the author informs us, in his preface, that during the last eighteen years he "has been unceasingly occupied with the subject, and has made numberless analyses." A few passages require our more especial notice. In his comparison between the methods of Frankland and Wanklyn for the analysis of water, Dr. Hassall states—"Frankland's method has a real scientific basis; it is good and sound in principle." How any method can be pronounced "good and sound" where the amount to be determined lies within the limits of error we have yet to learn. In the section on coloured confectionery we find indigo enumerated both among the permissible and the dangerous colours. Suppose this work falls into the hands of a conscientious confectioner, anxious not to poison his customers, which of these two conflicting statements is he to accept? Litmus, we are told, is frequently adulterated with "common arsenic and peroxide of mercury." To what purpose are these unlikely substances added? If such a sophistication really happens, why not make use of orchil, which is certainly free from poisonous ingredients?

On the detection of foreign fats in butter Dr. Hassall speaks with much confidence. We fear—judging from a case lately before a police-court—that we are still far from certainty on this important point.

On the detection of alum in bread the author observes—

"The perusal of all that has been written on this subject would lead an ordinary observer to form the opinion that the detection and estimation [? determination] of alum in bread constituted one of the most difficult processes in chemistry. This is really not so, however, and there are several processes whereby this salt may be estimated with ease and undoubted accuracy. We shall notice only those methods which are the most practical, and at the same time accurate." Easy as the process may be, it is certain that alum has been before now detected where none existed. We think we could point to printed instructions admirably calculated to lead to such a result.

In describing the estimation of total solids in milk, Dr. Hassall says—"We have therefore not found it necessary to make use of a weighed quantity of sand or hydrated sulphate of lime, which were formerly much employed." We were really of opinion that the superfluity of these additions had first been pointed out by another chemist, but from this passage it would seem that Dr. Hassall claims it, and indeed the whole process for the analysis of milk, as his own! This is the more remarkable as he is generally not remiss in stating his authorities. The sections on the lactometer and the creamometer might well, we think, have been omitted.

Perhaps the most valuable portions of the book are the denunciations of the present law on the adulteration of food and drugs, and the facts and admissions which throw so questionable a light on the ability of the chemists now placed as referees over the heads of the public analysts. A feature which we mention with reluctance is the author's quotation of passages in his own praise—*e.g.*, p. 855. What should we think if we came upon some similar extract in Fresenius or Plattner? Among the advertisements of various articles at the end of the book we find twenty displaying Dr. Hassall's certificate of excellence. Such a state of things may be "as it is," but certainly not "as it ought to be," and is assuredly not in keeping with good professional taste.

Year-Book of Pharmacy, with the Transactions of the British Pharmaceutical Conference at the Twelfth Annual Meeting, Held in Bristol, August, 1875. London: J. and A. Churchill.

THE former portion of this work consists of a number of papers extracted from scientific journals and the transactions of learned societies, and arranged under the heads of Pharmaceutical Chemistry, Materia Medica, Pharmacy, and Notes and Formulæ. A number of valuable processes are given for the detection of impurities and adulterations, and for the quantitative examination of substances of pharmaceutical interest. In a paper on the detection of dye-wares, taken from *Dingler's Journal*, we notice a substance designated as "saffron carmine." We conjecture that the translator means carthamin.

The remainder of the volume is taken up with an account of the Pharmaceutical Conference held at Bristol in August last, with reports of the speeches delivered and the papers read on the occasion. These were by no means strictly limited to subjects of direct bearing upon pharmaceutical questions. Thus an interesting paper was given by Mr. Stoddart on the geology and mineralogy of the Bristol district.

Mr. A. H. Allen read a paper criticising what he designated as the "Horsley-Stoddart method of estimating the fat of milk," which gave rise to a somewhat warm discussion. On a former occasion a Mr. Ekin declared that "the dealers in milk on a large scale already looked upon the Public Analysts, he thought rightly, as their very best friends. The analysts had set up an arbitrary standard for milk, and the milk dealers on a large scale availed themselves of it. There was hardly an instance in which the milk was not toned down to the analysts' standard and a large profit thereby made."

Here, therefore, the standard adopted by the Public Analysts is plainly pronounced too low. The complaint

generally made is precisely the opposite—that they have fixed their standard too high, and have thus caused honest tradesmen to be unjustly fined!

In a discussion on the growth of saffron a Mr. Williams made the following interesting statement:—It was believed that the feathers of certain birds lost their colour unless the feathers were kept well oiled, and that if the bird were prevented oiling its feathers the rain would wash out brilliant and delicate colours." We should like to know if other species of birds besides the touraco are here referred to, as, according to recent researches, the colours of many birds depend not on the presence of a pigment, but merely on the optical effects of their structure, just as in labradorite, opal, mother-of-pearl, &c.

We think it is impossible to glance through this volume without being struck with the amount of influence and prestige which have accrued to the pharmacutists of Britain from union. Will the "analytical and consulting chemists" never follow this example to protect themselves against the encroachments which they suffer on various hands? We have often harped upon this string, but we wish to tune it.

Air and its Relations to Life; being the Substance of a Course of Lectures Delivered in the Summer of 1874 at the Royal Institution of Great Britain. By W. N. HARTLEY, F.C.S. London: Longmans, Green, and Co., 1875.

MUCH labour and very much iteration will be required before our "respectable and intelligent classes" and even the gentlemen who review physical, chemical, and physiological works for the daily press, become possessed of even moderately correct notions on scientific subject. Such being the case, every man who can convey to the public clear and accurate information of this nature "in a light and popular manner" is worthy of high commendation. In the work before us Mr. Hartley gives a very full and correct account of the air in its bearings upon life. Besides the mere narration of facts he has aimed, not unsuccessfully, at imparting to his readers some insight into the methods of scientific research. All this is done in plain language, technicalities being as far as possible avoided. It is, by the way, a remarkable fact that whilst the use of technical language is freely conceded with every trade, every interest, every amusement even, scientific men are accused of seeking to conceal their knowledge from the world if they employ a special terminology.

The subject is not one of mere speculative interest. In the third chapter of the work Mr. Hartley treats of ventilation in principles and practice; of its neglect in most public buildings; of the evil effects of foul air; of the "ground air," or air permeating the soil; and of its passage into our dwellings, as well as of the danger from leaky sewers and gas-pipes.

An irreverent foreigner once observed that there were two subjects on which every Englishman was enthusiastic:—The first was the ventilation of his dwelling; and the second, the conversion of the Chinese. But even if this be true our zeal has not always been accompanied with knowledge, and hence has rarely been crowned with success. Many of our private houses convey the impression of "stuffiness" to anyone entering from the fresh air. Our public buildings, churches, theatres, courts of justice, &c., are simply discreditable. When ventilation is attempted it often consists merely in an abrupt and irregular influx of cold air, "cold" and "fresh" being by some unfortunate usage considered as synonymous. Frequently, too, the supply of fresh air is taken in, at, or near the level of the ground, where there is great possibility of its being contaminated with the "ground air," and the emanations of decomposing animal and vegetable matter.

But ventilation, important as it must be pronounced, is only one of many topics ably handled by the author. We find an account of the discovery of oxygen; of the

methods for its extraction from the air; of the discovery and properties of carbonic acid; of aqueous vapour; of atmospheric ammonia; of that chemical Proteus, ozone; of atmospheric germs; and of the great question of spontaneous generation. We can cordially endorse the judgment of a contemporary that "all persons of decent education, be they young or old, may read this book with pleasure and advantage," and we may add that many persons of high education, commonly so-called, are grievously in want of the knowledge which Mr. Hartley conveys in so pleasing a manner.

CORRESPONDENCE.

BUNSEN'S VACUUM PUMP.

To the Editor of the Chemical News.

SIR,—I am sorry that the person who signs himself "F.C.S." in the CHEMICAL NEWS (vol. xxxiii., page 63) has not preceded these letters by his name, so that I might have relieved my mind of any doubt as to the advisability of taking notice of his communication.

When I mentioned "Bunsen's vacuum pump" I wished to convey the idea of the "water pump," the principle of which was partially discovered by Sprengel, and applied by Bunsen for filtering purposes, in distinction to "Sprengel's vacuum pump," which I believe everyone understands to mean the "mercury pump." I think I assumed correctly that every chemist knows the exact history of "Bunsen's vacuum pump," and that Sprengel therefore loses no credit; but "F.C.S." seems to have his doubts about this. Possibly, if I had used the words your anonymous correspondent suggests, he would have found that Torricelli was the first to produce a vacuum by this means, and would have given his "advice" accordingly.

In conclusion, let me advise the person who signs himself "F.C.S." that the next time he criticises he will save the space of your valuable journal, and probably make his communication appear more courteous, by keeping better to the point he wishes to discuss; but "doubtless in this he sins in company with" *some scientific men.*—I am, &c.,

W. THOMSON.

Royal Institution, Manchester,
February 14, 1876.

THE NEW PHASE OF ELECTRIC FORCE.

To the Editor of the Chemical News.

SIR,—I enclose some drawings that may be of service in any experiments you or any of your scientific friends may make with the newly discovered force.

Some excellent experimenters fail to obtain the spark at all, not fully understanding the conditions; they have tried too large magnets, or magnets with too large cores, from which, according to the experiments of Mr. Edison and myself, thus far, the force cannot be obtained. Some fail from insufficient battery power, others from carelessness, and others still from lack of patience.

It is, perhaps, scarcely necessary to say that all the possible conditions by which the force can be developed are not yet fully understood by any one; but thus far the spark has been obtained most conveniently from telegraph sounders converted into self-vibrators, and from small coils of a few ohms resistance.

When the phenomena are once admitted, and it is claimed that they represent any known form of electricity, it seems to me that the burden of proof is shifted, and on that issue it rests with those who make that claim to take some known form of electricity, giving a spark like the spark of this force, and obtain therewith all these phenomena.

Those who hastily conclude that the extra current, or statical electricity of low potential, account for this

spark, form their opinions, I fear, from insufficient study of the subject or inability to look on all sides of it at once. My physiological experiments, which seem to be important and convincing, have not yet, so far as I can learn, been repeated in detail.

The drawings which I enclose were made by Mr. Edison; they are from the advance sheets of a monograph on the subject now in preparation.—I am, &c.,

GEO. M. BEARD.

New York, 36, West 33rd Street,
January 24, 1876.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Seances de l'Academie des Sciences. No. 4, January 24, 1876.

Decomposition of Water by Platinum.—MM. H. Ste.-Claire Deville and H. Debray.—If we heat in a glass tube raised to 500° to 600° cyanide of potassium near to a boat full of warm water, a vacuum having been made beforehand, the pressure reaches at most half an atmosphere, and remains constant for some hours. But if we mix with the cyanide of potassium some platinum-sponge hydrogen gas is evolved in quantity, and there is formed a double cyanide of platinum and potassium. The hydrogen is impure, being accompanied not merely with ammonia, but with 4½ to 12 per cent of carbonic oxide. If the heat exceeds dull redness carbonate of ammonia sublimes. A concentrated solution of cyanide of potassium attacks platinum at the boiling-point. The metal is converted into the double cyanide, with an escape of pure hydrogen. Cyanide of mercury dissolved in water is not precipitated by platinum, even at the boiling point; but if a little cyanide of potassium is added mercury is immediately liberated, and combines with the platinum.

Action of Monohydrated Sulphuric Acid upon the Alcohols.—M. Berthelot.—A thermo-chemical paper, not adapted for abstraction.

Application of the Mechanical Theory of Heat to the Study of Volatile Liquids: Simple Relations between Latent Heats, Atomic Weights, and Vapour-Tensions.—M. R. Pictet.—For all liquids the cohesion is constant. The latent heats of all liquids, raised to the same pressure, and multiplied by the atomic weights at the same temperature, give a constant product. The latent heats of all liquids are multiples of the specific heats.

Action of Ammonia upon Rosanilin.—M. E. Jacquemin.—Inserted in full.

Researches on the Constitution of the Gelatinous Substances.—MM. P. Schützenberger and A. Bourgeois.—These researches relate to isinglass, ossein, gelatin, and the chondrin obtained from the costal cartilages of a calf. As in the albumenoid bodies and fibroin, the nitrogen evolved as ammonia on the one hand, and the carbonic and oxalic acids on the other, are in such ratios that the simultaneous production of these three bodies may be considered as connected with the hydration of urea and oxamide.

Action of Cold upon Milk and its Products.—M. E. Tisserand.—The author finds that the cream separates the more rapidly the nearer the milk has been brought to the freezing-point, its volume being also increased. The yield in butter is also greater when the milk has been exposed to a low temperature, when both skimmed milk, butter, and cheese are of better quality (?).

Magnetic Action on the Rarefied Gases of Geissler's Tubes.—M. J. Chautard.—The author states

that it is perfectly well known that gases, and even metallic vapours, present spectra which differ according to the conditions in which they are accidentally placed, among which are temperature, pressure, and the combinations in which the substances occur. The experiments which he has made enable him to add to these modifying causes the action of a powerful electro-magnet. He insists that every spectroscopic observer, before attempting to calculate the numerical results of his experiments, should first determine his peculiar personal error.

Spectrum of Nitrogen, and on that of Alkaline Metals in Geissler's Tubes.—M. G. Salet.—The rays described by Schuster have not been found by Stearn and Wüllner. The rays described in the memoir of 1872 may possibly have been due to the vapour of sodium.

Action of Heat in Magnetisation.—M. L. Favé.—The following phenomena have been observed:—The conservation of magnetism at any temperature as long as such temperature is constant; the decrease of the magnetism, slow at first and then rapid, and becoming very rapid at the expiration of a time which varies with the temperature of the magnetisation; the increase of the residual quantity of magnetism after cooling, when the magnet is heated anew.

New System of Electric Light with an Independent Regulator.—M. E. Girouard.—This paper requires the accompanying illustrations.

Influence of Different Manurial Elements on the Growth of the Beet, and on its Proportion of Sugar.—M. H. Joulie.—Phosphoric acid increases the proportion of sugar. Potash has not this effect, but renders the roots more saline and of worse quality. Nitrate of soda is favourable to the gross yield without injuring the quality. Assimilable nitrogen is favourable to the yield, without injury to the quality, if applied in moderate doses, beyond which it is hurtful both to quality and weight. Nitrogen in nitrates is preferable to nitrogen in ammonia.

Bulletin de la Societe Chimique de Paris,
No. 2, January 20, 1876.

Determination of the Alkaline Metals in Silicates and Substances not Attacked by Acids by means of Hydrate of Baryta.—M. A. Terreil.—The author attacks silicates with perfectly pure hydrate of baryta, fused and pulverised, using 7 to 8 parts to 1 of the silicate. The operation is performed in a silver crucible at 350°, the temperature being raised a little when the fused mass has again solidified, but so as not to reach dull redness. The mass, after cooling, is boiled in pure water in the crucible, and the filtrate is treated with a current of well washed carbonic acid, raised to a boil, and filtered. The alkalies will be found in the filtrate.

Part Played by Acids in Dyeing with the Colouring Matters of Madder and their Artificial Substitutes.—M. A. Rosenstiehl.—Already noticed.

Theory of the Production of Aniline-Black by means of the Salts of Vanadium.—M. A. Guyard (Hugo Tamm).—See page 70.

Separation of Arsenic from Sulphuric Acid by means of Hyposulphite of Soda.—M. Thorn.—This method has been adopted in several manufactories of sulphuric acid. The arsenic is chiefly present in the chamber acid in the state of arsenious acid, but by the action of hyposulphite of sodium it is transformed into sulphide of arsenic and sulphate of soda. The manner of operating is as follows:—The chamber acid at 50° B. is heated in a leaden vessel to 70° or 80°, with the addition of the necessary amount of hyposulphite (according to the arsenic present), either dissolved in water or as a powder. The whole is then well stirred. Sulphide of arsenic separates out, and collects in flakes on the surface of the acid, which is then drawn off from below. Only a very slight escape of sulphurous acid occurs if the operation is

properly conducted. The purified acid contains 0.30 to 0.40 per cent of sulphate of soda, which, for most purposes, is not prejudicial. The quantity of arsenic in the raw acid at 50° B. varies from 0.098 to 0.004 per cent.—*Dingler's Journal*.

On the Same Subject.—R. Wagner.—To obviate the presence of sulphate of soda in the sulphuric acid when purified, the author proposes the use of the hyposulphite of barium, prepared by the reaction of hyposulphite of sodium and chloride of barium. A deposit of sulphide of arsenic and sulphate of baryta is formed, and the purified acid is run off.

Phosphuretted Copper.—M. Schwartz.—The author lines the crucible to be employed with a paste made of 14 parts silica, 18 parts bone-ash, 4 parts charcoal powder, 4 parts soda, and 4 parts powdered glass, mixed with a solution of gum. The crucible is dried, the copper introduced and covered with the same mixture, and the lid luted on. The whole is then raised to a bright red heat. The copper is afterwards found to contain 3.25 per cent of phosphorus. In preparing phosphor-bronze the phosphide of copper is melted with such proportions of tin and copper that the whole may contain 0.5 per cent of phosphorus.

Use of Xanthate of Potash as a Remedy for the Phylloxera.—T. Zoeller and A. Grete.—The authors propose to use the xanthate in place of the sulpho-carbonate.

Gazzetta Chimica Italiana, Anno v., 1875, Fasc. vii., viii.

Chemistry at the Twelfth Congress of Italian Men of Science, held at Palermo.—The meetings of the Chemical Section, six in number, were held in the Chemical Lecture-Hall of the University, on the 30th of August, and on the 1st, 3rd, 4th, 5th, and 6th of September. The introductory discourse was delivered by Prof. Cannizzaro, and is not adapted for abstraction.

Composition of Certain Italian Minerals and Rocks.—Prof. Alf. Cossa.—The author, in this first portion of his memoir, describes the analytical methods employed. For opening up the silicates insoluble in the ordinary acids he has quite abandoned the use of solutions of hydrofluoric acid. For the determination of all the constituents, except the alkalies, he has had recourse to fusion with mixed carbonates of potash and soda. For the alkalies he has followed, with some modifications, the method proposed by Ste.-Claire Deville.

New Method of Formation of Benzylated Phenol.—E. Paterno and M. Filetti.—The authors mix 40 grms. benzoic alcohol with 36 grms. of crystalline phenic acid, dilute the mixture with 140 grms. of glacial acetic acid, and then cool with water whilst gradually adding about 1½ volumes of a mixture of equal volumes of commercial sulphuric acid and acetic acid.

On Two Isomeric Amido-cuminic Acids.—E. Paterno and M. Filetti.—The authors have obtained the two isomers in question by the reduction of nitro-cuminic acid prepared in the ordinary manner. They suggest the possible existence of two isomeric nitro-cuminic acids.

Action of Light upon Nitro-cuminic Acid.—E. Paterno and M. Filetti.—Nitro-cuminic acid is profoundly affected by the influence of light. Whether it is exposed to the direct solar rays or to diffused light, it takes a red colour. The authors have traced the formation of a red substance of a decidedly acid character, and having an elementary composition very near that of the original acid.

Benzyl Derivatives of Urea and Sulph-urea.—E. Paterno and P. Spica.—The method employed by the authors has been to cause a salt of dibenzylamin to react upon the cyanate and sulphocyanate of potassium. In this manner they have prepared mono-benzyl-urea and mono-benzyl-sulphurea.

Cyanide of Acetyl.—M. Filetti.—The author having heated, for four to five hours, equivalent proportions of chloride of acetyl and dry cyanide of silver, and then submitted the result to fractional distillation, obtained three liquids. The first contained chloride of acetyl; the second portion, containing the cyanide, boiled at 93°, and the third went over about 200°.

On Paratoluylic Amide.—P. Spica.—This substance forms fine prismatic crystals, perfectly colourless, and slightly soluble in cold water, chloroform, and benzin, but freely soluble in boiling water, alcohol, and ether.

Action of Chloride of Cyanogen, Gaseous and Solid, upon Cuminic Alcohol.—P. Spica.—The author by the action of the gaseous chloride obtains cumilic carbamate, containing 68.34 per cent of carbon and 7.53 of hydrogen.

New Reagent for Morphia.—Prof. E. Selmi.—The author takes glacial acetic acid, stirs it up for fifteen minutes with minium in fine powder, filters, and places a drop upon a plate of glass laid upon white paper. Upon this drop are put 2 or 3 drops of an aqueous solution of acetate of morphia, drying each time at a very gentle heat, so as to have at last a spot formed of the dry acetate of the alkaloid. From the first instant there appears a slight yellowness, which goes on increasing as the acetic acid evaporates, passing into a bright yellow, an orange, and a dark yellow. If left to spontaneous evaporation the yellow gives place to a violet, which grows paler, and finishes by taking the colour of lees of wine.

On an Alkaloid found in the Brain, the Liver, and in the Green Heads of Wild Poppies.—Prof. Selmi.—A preliminary notice.

On a New Method of Preparing Diphenyl.—Dr. A. Cristomanos.—Not suitable for abstraction.

Presence of the Peroxide of Hydrogen in the Juices of Plants.—G. Bellucci.—The author's experiments, undertaken to verify the statement of Clermont (*Comptes Rendus*, 1875, lxxx., p. 1591), have led to a negative result.

Supposed Transformation of Cellulose into Gum.—M. Mercadante.—The author holds that this alleged metamorphosis is not admissible.

On the Reagent of Barreswill, Fehling, and Trommer, for the Determination of Glucose.—G. Missaghi.—An examination of the modifications of this test recently proposed by Lagrange and Boivin.

Arrangement of a Filter for the Separation of Crystalline Substances Entangled in Large Amounts of Extractive Matter.—G. Missaghi.—The author selects a funnel with a long neck, and puts into it a layer of pieces of porcelain large enough not to enter into the neck; upon this comes a layer of gradually smaller and smaller pieces, so as to sustain a disc of filter-paper, with its edges adhering to the sides of the funnel. Over this is placed fine pure silica, filling the hollow made by the paper. Over this comes another disc of paper. The funnel is then connected with an aspirator.

Persistence of the Germinative Power in Seeds Steeped in Water and Tumified when kept in an Atmosphere of Carbonic Acid constantly Saturated with Moisture.—G. Missaghi.—A few grains of wheat were first allowed to swell in distilled water, and were then placed in an atmosphere of carbonic acid. Other grains, rubbed first when dry, and washed with distilled water, were then allowed to swell in the same apparatus, in which was a stratum of water saturated with carbonic acid. The seeds were then planted in separate pots filled with the best vegetable mould. Those of the first lot all putrefied, whilst the second germinated and grew.

Evolution of Hydrogen during the Vegetation of Mould.—G. Missaghi.—The author was unable to detect any free hydrogen in the atmosphere in which the mould was growing.

Certain Processes for Freezing Iodide of Potassium from Iodate.—G. Pellagri.—The author has tried successfully the action of an electric current.

Preparation of Bicarbonate of Potassa.—Dr. L. Pesci.—The author passes a current of carbonic acid into a solution of pure potassa in alcohol (absolute, or of 80 per cent), so that not all the carbonate may be transformed into bicarbonate. From the filtrate pure bicarbonate is deposited.

On Certain Reactions of Chloral.—D. Amato.—Not suited for abstraction.

On Nitrosotimol and Certain of its Derivatives.—R. Schieff.—Not adapted for abstraction.

On Indigotin in Animals, or the Purple of the Ancients.—A. and G. De Negri.—Already noticed.

New Spectroscopic Method for Discovering in Gaseous Mixtures and Liquids the Smallest Quantity of a Gaseous or Very Volatile Hydrocarbon.—A. and G. De Negri.—Into a Geissler tube is introduced a small quantity of a gaseous mixture, which ought not to contain oxygen, carbonic oxide, or carbonic acid, exposing it to a barometric pressure not greater than 20 m.m. If in the gas under examination a hydrocarbon is present, on causing it to be traversed by a spark from a Ruhmkorff's coil, a sky-blue light suddenly appears, which, if viewed with the spectroscope, presents the spectrum of carbon, and generally so brilliant as to mask totally the spectra of other gases present, not excepting nitrogen.

Production of Ozone by means of the Electro-phoric Discharge.—G. Giannetta and A. Volta.

Reaction by means of which Sulphur destroys the Oidium of the Vine and on the Emission of Free Hydrogen from Plants.—E. Pollacci.—These two papers do not admit of abstraction.

PATENTS.

ABRIDGMENTS OF PROVISIONAL AND COMPLETE SPECIFICATIONS.

Improvements in the manufacture of black for painting or printing, which is also applicable to discolouring sugar, and for use as a disinfectant. D. C. Knab, Saint Denis, France. December 23, 1874.—No. 4421. Caustic lime, perfectly crushed and sifted, is intimately mixed with gas-tar or other body giving carbon by distillation in closed vessels. The proportions are from 30 to 50 parts of tar to 100 parts of lime. As dry caustic lime is difficult to crush, it is slacked with the least possible quantity of water, so as to obtain a well pulverised powder, which is thoroughly mixed hot with the tar. The intimate mixture thus formed is placed in a gas-retort, or in earthenware or iron pots, and heated to red-heat until gas ceases to be given off. The pots are then withdrawn and allowed to cool, and the cold matter extracted from the pots is emptied on a brick floor, and taking up the moisture of the air falls in powder as pure caustic lime. During carbonisation in the black pots the gases which ignite on removing the closed pots produce an immense smoke, which, being collected, by this means two blacks are obtained, one analogous to bone-black, the other similar to the black of ordinary smoke. The black thus produced is a rough or raw black, suitable for employment like powdered bone-black, such, for instance, as blacking and common black colours; and by treating this rough black with muriatic acid or any other lime solving acid the lime is completely removed, and perfectly pure and beautiful black is the result.

An improved method of and apparatus for ascertaining or proportioning and titrating the quantity of tannin in oak-bark and other tanning substances. W. E. Gedgc, Wellington Street, Strand, Middlesex. (A communication from L. A. Beaudet, Paris.) December 23, 1874.—No. 4423. This process of proportioning the tannin consists in the precipitation obtained, and consequently in the volumes of the precipitates contained in gauged and graduated testers. The operator takes, say, 5 grms. of pulverised oak-bark, or 5 grms. of any other tanning material which it is desired to analyse; boils it in several waters, so as to extract all the tanning principle; then brings the volume of this solution to the measure of a half litre, either by reducing it or by adding the necessary volume of water; 30 grms. of this solution, reduced to a half litre, are then poured into a conical tester gauged according to this invention. The tester is then filled with clear water up to a volume of 125 grms., and the solution precipitated by means of neutral or ammoniacal salts, salts of lead, copper salt, salt of Titan, or organic alkalies, such as salts of "cinchonine," quinine, or strychnine, in the proportion of 15 grms. for the 125 grms. of liquid above mentioned. There is thus obtained the volume of precipitate which indicates exactly the quantity of tannin,

which is the object sought, which quantity is engraved in centimetres on the conical test-glass. The tannin may also be proportioned or titrated by the combination of the weights, volumes, and graduations, whatever the form of the vessels employed.

Improvements in the purification of water. J. A. Wanklyn, Charlotte Street, Middlesex. December 23, 1874.—No. 4431. The object of my invention is to extend the application of the Clark process to those other waters to which it is not at present applicable, and this object I attain by the use of carbonate or bicarbonate of soda, or of other soluble carbonates or bicarbonates which will effect the decomposition of the salts of lime and magnesia, and yield carbonates or bicarbonates of lime or magnesia, to which the usual Clark process is then applicable. After having thus treated the water containing soluble salts of lime or of magnesia other than or in conjunction with bicarbonate of lime, I then submit the same to the treatment of what is known as the Clark process. It will be evident that the softening process which I have described may be employed either as a preliminary process or in conjunction with the lime or Clark's process in order that the desired results may be simultaneously obtained. The amount of soluble carbonates or bicarbonates to be employed for effecting the softening of the water will, of course, depend upon the amount of the salts of lime and magnesia contained in the water in a form other than that of bicarbonate, and the amount of the carbonates or bicarbonates employed will be in equivalent proportions to those salts or compounds contained in the water to be treated.

Improvements in dyeing and printing, and in improved compounds for such purposes. J. S. Sellon, Hatton Garden, and R. Pinkney, Bread Street Hill, London. December 24, 1874.—No. 4433. This invention relates to improvements in dyeing and printing, and in improved compounds for such purposes; and consists in the employment of compounds of vanadium and of soluble salts or compounds of vanadium, for the purposes of dyeing and printing in conjunction with other dyeing and printing agents, such, for example, as the organic vegetable animal or artificially-produced dyeing or printing materials. In carrying out this invention the salts or compounds of vanadium are mixed with the dyeing and printing materials, and in such quantity as shall produce the desired result, so that the colour produced when the material to be dyed or printed shall be preserved and intensified, and that the dyeing or printing materials employed shall be economised.

Improvements in treating ores, minerals, and other bodies containing metals to obtain gold, silver, and other products therefrom, and in the apparatus employed therein. T. Clark, M.D., Wilmslow, Chester, and E. Smith, F.C.S., Torquay, Devon. December 26, 1874.—No. 4448. An improved method is described of treating metalliferous bodies or fluids with alkaline hyposulphite, preferably hyposulphite of soda, or with caustic or liquor ammonia, or salt containing free ammonia, or with a mixture of any alkaline hyposulphite with any of these ammoniacal compounds; the solution of silver, &c., obtained is then run into a tank properly prepared to induce galvanic action. A means is described of aiding such action by superheated steam: also of chloridising tin ore in a dry state, and a special arrangement of tanks suitable for such purposes is also described.

Improved process and apparatus for the treatment of oily and fatty matters, and of materials containing oily and fatty matters, and in the subsequent recovery or separation of the oil or fat or oily and fatty matters from the agent employed, and of the agent itself, and in drying the materials treated. G. W. Bentley, Kennington Park Road, Surrey. December 28, 1874.—No. 4452. Some or all of the following particulars, that is to say, drying by air passed over chloride of calcium, extraction of grease (whether oily or fatty) by carbon bisulphide or some hydrocarbon, or amyl alcohol distillation for the recovery of the oil and fat and of the agent, washing or cleansing, and drying. Suitable arrangement of two cylinders or vessels one revolving one within the other.

Improved apparatus to be used in the manufacture or production of carbonate or bicarbonate of soda, and for other purposes. H. Müller, Dusseldorf, Rhenish Prussia. December 28, 1874.—No. 4458. The invention relates to apparatus for the manufacture of carbonate and bicarbonate of soda by the so-called ammonia process, the said apparatus being also applicable for use in other chemical processes in which it is required to bring fluids in intimate contact with gaseous matters.

Improvements in the manufacture of iron and steel. J. T. Kirkwood, Cheltenham, Gloucester. December 29, 1874.—No. 4463. These improvements consist in using and mixing peroxide of iron or any other oxide of iron with the iron or steel contained in any converter or other suitable vessel: and also at the same time, or at any more convenient period, the inventor introduces atmospheric air, steam, or other gases or vapours, mixed or unmixed with atmospheric air, which impinge upon the surface of the slag and molten metal for the purpose of purifying the same.

NOTES AND QUERIES.

. Our Notes and Queries column was opened for the purpose of giving and obtaining information likely to be of use to our readers generally. We cannot undertake to let this column be the means of transmitting merely private information, or such trade notices as should legitimately come in the advertising columns.

Refining of Rape Oil.—Can any of your readers inform me of the best material for coating the iron tanks and pipes used in the refining of rape oil by means of sulphuric acid?—RAPE OIL.

Dry Rot.—(Reply to "Dry Rot").—Dry rot is both prevented in new buildings and cured in old ones by filling up the space between the floor joists with "tank-waste" from alkali works. This can also be applied to the ends of beams resting in walls.—GEO. LUNGE.

THE CHEMICAL NEWS.

VOL. XXXII. No. 848.

ON THE

LOSS OF COPPER THROUGH VOLATILISATION IN THE CORNISH COPPER ASSAY.

By JOSEPH ROSKELL, Widnes.

My attention has been drawn to an article in the CHEMICAL NEWS (vol. xxvi., p. 243), by Cornelius A. Mahoney, on the above subject, in which the writer attributes the whole loss to the use of salt (NaCl) in the process of assaying. I think there is no doubt but that it is the cause of a part of the loss, but in my opinion not the whole of it.

When calcic fluoride (CaF_2) is acted upon by sulphuric acid or an acid sulphate (SO_2HONaO or NaHSO_4), hydrofluoric acid (HF) is formed, and this again by reacting upon silica (SiO_2) produces volatile silicon fluoride (SiF_4), and if a borate ($\text{B}_4\text{O}_5\text{NaO}_2$) is present it produces volatile boric fluoride (BF_3). Now in the crucible we have when fusing for regulus the necessary ingredients for producing these reactions; thus we add fluor-spar and borax as fluxes, and the sulphur necessary to produce the acid or the sulphate is either present in the ore as a sulphide, or else is added to it in the form of sulphur; and I therefore think it probable that these reactions do take place while fusing, and so produce these volatile products, which would have the same tendency to carry off the copper as the salt would, and it is well known what a vast amount of fumes are given off during the fusion.

To ascertain if any of the copper was carried off or not during this fusion, I decided to make a few experiments upon different samples of ore and containing different percentages of copper, and consisting of sulphides and carbonates. The samples were all assayed by myself against two of the recognised assayers in Cornwall, and were selected for these experiments because their assays agreed either exactly with my own assay or did not differ by more than 1-8th per cent.

I knew that if no copper was carried off during the fusion for regulus that I ought to have the same quantity in the regulus and the slags that the ore itself contained; but if I obtained a less quantity, then some of it must have been carried off. In the fusion for regulus I used no salt, so that any loss could not be attributed to its use; the weight of ore used in each experiment was for those above 12 per cent, 1000 grs., and for those under 12 per cent, 2000 grs., which was fused in crucibles containing 500 grs. in each. The regulus was weighed and a portion taken and assayed by the usual "dry" method, and another portion used to ascertain the produce by "wet" assay; two assays were made by each method from each sample of regulus, the weight of which corresponded to 200 grs. of the rich ores and 400 grs. of the poor ores. The slags were also weighed and tested, and the quantity of copper found in them added to the percentage. The "wet" assay was performed by the "cyanide" test so diluted that each degree of the burette was equal to 0.05 gr. of copper. The sample of regulus, after dissolving, was made up to 1000 grain measures, from which I took various quantities for each test. The ore itself was treated similarly, and its percentage ascertained. The ammonia used was measured and the same quantity was used for standardising as was used in each test.

I also assayed three of the poor samples without using any salt whatever in any part of the process, the result being in one case the same, and in the other two slightly below the percentage of the ordinary assays.

It will be seen by the table here given that in no case does the quantity of copper found in the regulus along

with what is contained in the slags, correspond to the quantity found in the ore, while at the same time it is more than what is given by the finished "dry" assay. This I think is sufficient proof that some portion of the copper is lost without the use of any salt whatever, and must I think be put down to the volatile products formed in the fusion for regulus.

I am also inclined to think that some of the copper is also lost during the calcining by being carried off mechanically by the sulphur. This I intend to experiment upon again, and will let you know with what results.

No. of Sample.	Quality.	Ore, Dry Assay.	Ore, Wet Assay.	Difference.	Regulus, Wet Assay.
1.	Purple ore	49.875	50.750	0.875	50.300
2.	{ Sulphide } { yellow }	30.125	31.000	0.875	30.560
3.	"	17.825	18.500	0.675	18.340
4.	"	16.214	17.000	0.786	16.855
5.	"	9.275	9.750	0.475	9.425
6.	"	11.500	12.000	0.500	11.746
7.	"	11.275	11.725	0.450	11.500
8.	"	7.875	8.452	0.577	8.000
9.	"	8.180	9.000	0.820	8.410
10.	Carbonate	27.250	27.750	0.500	27.400
11.	"	19.510	20.250	0.740	19.833
12.	"	27.500	28.750	1.250	27.725

REPORT

ON THE

DEVELOPMENT OF THE CHEMICAL ARTS DURING THE LAST TEN YEARS.*

By Dr. A. W. HOFMANN.

(Continued from p. 70.)

SIEBE's machine appeared at the London Exhibition of 1862. Schmidt† published an illustrated description of the machine exhibited, remarking that another machine in the possession of the patentee worked by a 24 horse-power engine produced 5 tons of ice in 24 hours, which, under the most favourable circumstances, may be regarded as 4 kilos. ice per kilo. coal consumed. In this machine the evaporator is like a boiler with horizontal tubes. It is stated that‡ the cost of producing the ice amounted to one and a half marks (about 1s. 6d.) per cwt. A further account of Siebe's machine is found in *Engineering* for 1868, No. 483.¶

According to this journal such a machine was in use in Truman and Hanbury's brewery in London, yielding 6 tons of ice per twenty-four hours, and worked by a high-pressure engine of 15 horse-power. 1 cwt. of coal produced 4½ cwt. of ice. The solution of salt is said to have a temperature of -8° to -12° . In 1870 appeared a final description of Siebe's machine.§ A new and very compact little apparatus is mentioned driven by a 1 horse-power engine consuming 5 to 6 lbs. of coal per hour, and yielding 12.5 to 15 kilos. of ice hourly, or 5 kilos. of ice to 1 of coal.

No intelligence has been obtained concerning ether machines from other countries and by other makers. It has never, to our knowledge, been brought into use in Germany. At the Vienna Exhibition this principle was represented by one machine by Siebe and Gorman, of London.

Ether is a liquid which, under ordinary pressure, boils at 35°C .; under other circumstances the relation between temperature and pressure is as follows:—

Temperature	-20°	0°	$+20^\circ$	40°	90°	120°C .
Pressure	0.09	0.24	0.6	1.2	5	10 atmos.

* "Berichte über die Entwicklung der Chemischen Industrie während des letzten Jahrzehends."

† Schmidt, *Dingl. Pol. Journ.*, clxviii., 434.

‡ *Dingl. Pol. Journ.*, clxvii., 397.

§ *Dingl. Pol. Journ.*, xcii., 189.

¶ *Prakt. Mech. J.*, 1870, 251.

If allowed to evaporate much below the freezing-point of water the tension of the vapour is very low, perhaps only one-tenth of an atmosphere. Hence the external air exerts a great and permanent pressure upon the evaporator, and upon the air-pump which draws out the vapour. The joints and fastenings must therefore be made with extreme care, lest a trace of air should enter, which would have the most pernicious influence on the working of the machine, and especially on the speed of evaporation. On compressing the vapours the tension rises to several atmospheres, and a rise of temperature occurs amounting certainly to more than 60°. If cooled at this pressure the vapour re-condenses to a liquid. More accurate statements as to relative pressure and temperature of the vapour of ether cannot be found. The basis for the calculation of the theoretical duty of the machine is the same as that of the air-machine to be described below, for which the requisite data are furnished and to which we may refer. From the known magnitude of 90 heat-units (the latent heat of the vapour of ether), it is possible to calculate how much ether, theoretically, must be evaporated to yield a given weight of ice.

Substitution of Methylic Ether for Ethylic Ether.—Methylic ether is formed by the action of sulphuric acid upon methylic alcohol or wood-spirit, a homologue of the ordinary ether produced by the action of sulphuric acid upon spirits of wine, and distinguished by its far greater volatility. Methylic ether is gaseous at ordinary temperature and pressure, and can be condensed to a liquid only by great pressure or by cold. The liquid, at the pressure of one atmosphere, boils at -21° . Tellier, of Paris, has used this ether as an agent for the production of cold in his ice machine, which is constructed exactly like that of Siebe.*

(To be continued.)

ON ELECTROLYTIC ANILINE-BLACK.

By M. FR. GOPPELSRÆDER.

If a galvanic current, weak or strong, passes through an aqueous solution of the hydrochlorate, sulphate, or nitrate of aniline, whether cold or hot, dilute or concentrated, neutral or acid, there appears—in a longer or shorter time—a green deposit at the positive pole, which passes by way of violet and bluish violet to a deep indigo-blue. The tartrate, oxalate, and acetate of aniline yield merely a brown deposit, accompanied by a little green. If we reverse the poles decolouration appears at that which was previously positive, and the same colours are reproduced at the pole which had been negative.

The reaction is very sensitive, for 1 m.grm. of hydrochlorate of aniline, dissolved in 60 c.c. of water, gives in a few hours a green deposit at the positive pole. With a solution of 1 m.grm. of the same salt in 30 c.c. of water, there appears not merely the green, but also the blue and the violet reaction. With 2.5 m.grms. of hydrochlorate of aniline in 30 c.c. of liquid there was produced, in the course of two hours, a violet-blue deposit partially of a greenish grey; an hour later a brown colouration of the liquid, and still later a very distinct green reaction.

The liquid into which the positive electrode dips exhibits very different colours—sometimes yellow, orange-red, or violet. When the salt of aniline is entirely decomposed the liquid becomes colourless.

The cotton, filter-paper, wool, or silk, employed to conduct the current from one vessel to another, is coated with the same colour as the electrode, and is even dyed a salmon-brown, a green, gray, or red, the liquid containing different colouring matters, which are separated by the capillarity of the fibres. In place of these conductors amianthus may be employed.

The negative electrode is merely covered with a slight black shade, and there is produced at most a light yellowish brown deposit at the bottom of the vessel. The liquid at this pole is coloured a yellow or reddish brown, the conductors being dyed analogous shades.

The green deposit obtained in the first place at the positive pole is a body unalterable by ozone when dry, but in ammonia it turns a greenish blue, and subsequently blue. After the ammonia has evaporated it becomes again blue. The green deposit from the positive pole, when moist, is affected by ozone, and becomes a deep violet-blue if heated with a solution of bichromate of potassium, but if treated with a strong acid turns green again. As for the deep indigo-blue deposit, it is a mixture of several colouring matters, among which is aniline-black, from which the other colours may be separated by ordinary solvents, such as water, alcohol, ether, benzol, acids, and dilute alkalies. The quality and quantity of these colouring matters accompanying the black depend on the nature and concentration of the liquid, the strength of the battery, the temperature, and other accessory circumstances.

The deposit at the positive pole after purification is a fine crystalline black, of metallic lustre, not capable of sublimation; insoluble in water, the alcohols, benzol and its homologues; unalterable by weak acids, even at the point of ebullition, but turning green if boiled with concentrated acetic acid. It resists the action of reducing and oxidising agents; it is unaffected by ozone, either in the dry or the wet state,—by electrolytic oxygen and hydrogen, by nascent hydrogen, and by chlorine-water. It is not soluble in alkaline solutions, but is in part modified, since alcohol subsequently extracts a blue colouring matter, which becomes green with ammonia and yellow with acids.

The electrolytic black heated with alcohol, under pressure, colours it a deep violet, which becomes more beautiful if treated with alkalies, and is unalterable in dilute acids. The electrolytic black dissolves in sulphuric acid. The solution is violet, blue-green, or brown, according as the sulphuric acid reacts more or less energetically upon the black. The violet, blue, and green sulphuric solutions, if poured into water, give a green precipitate. The filtrate is colourless or reddish, and in the latter case contains a red substance the alcoholic solution of which, mixed with ammonia, is of a fine rose-colour, having a beautiful fluorescence like that of naphthalin-rose.

The green precipitate obtained from the sulphuric solution of the black poured into water is insoluble in ordinary solvents, but it may be suspended in water so finely that it appears to be dissolved. This green dissolves in hot sulphuric acid with a dirty violet colour, and is re-precipitated by water. If submitted to prolonged heating with sulphuric acid the water takes a rose-colour, and with ammonia becomes of a bluish colour with a yellow fluorescence. With caustic potassa it becomes bluish, and the filtrate is red. Ammonia renders the green violet, and even black, but acetic acid converts it to a green again. After the addition of ammonia, or of a fixed alkali, the aqueous liquid in which the green is suspended becomes of an intense blue, but the colouring matter is still merely suspended, a minimum portion only dissolving, for the filtrate has a faint violet-blue colour. Nascent hydrogen decolourises it by degrees.

If heated to redness in a combustion-tube, with a mixture of lime and soda, electrolytic black disengages white vapours, which have the odour of aniline, and which turn turmeric brown. If a stronger heat is applied ammonia is obtained. If the layer of soda-lime is not very long a violet sublimate is obtained at the same time, soluble in alcohol, with a violet-blue colour by transmitted daylight, but violet-red in artificial light. The liquid is rendered green by hydrochloric acid, and the blue colour is restored by alkalies. The presence of nitrogen in the electrolytic black has also been demonstrated by the potassium reaction.

An optical examination of the electrolytic black has

* *Engineering*, 1871, 179. *Dingl. Pol. Journ.*, cciii., 191. *Pol. Centralbl.*, 1872, 38.

shown that it is more black than the other aniline-blacks with which it has been compared.

The author announces his intention of shortly laying before the Academy the results of an elementary analysis of these bodies most carefully purified.—*Comptes Rendus*.

ON THE
RELATIONS OF CHEMISTRY TO PHYSIOLOGY
AND PATHOLOGY, WITH SPECIAL
REFERENCE TO THE BRAIN.*

By CHARLES T. KINGZETT, F.C.S. London and Berlin, &c.

THE vast advances that physiology has made in recent times are in no small measure due to the influences exerted by the discoveries and writings of Justus Liebig. He was among the first to expose the non-scientific method of enquiry employed by many physiologists of his day, and he perhaps, more than any other, impressed inquirers with the importance of tracing back effects to causes. Thus he has shown (Liebig's "Animal Chemistry," third edition) how little is conveyed by stating the effects of the nervous system as due to a nervous force, or by calling a body which exerts a cooling action on the system an astringent. Such notions he demonstrated to have no more worth than the old notions of Lemery, that the particles of an acid had the form of spear heads with barbs, while those of the alkalis were porous like sponge.

The processes of organic nature are chemical processes, and can be discovered only by chemical methods, aided by a logical interpretation of facts. Even Liebig, in the preface to his work already referred to, acknowledges "how greatly he was indebted to the study of Mills's 'System of Logic,'" and states, further, "he can claim no other merit than that of having applied to some special cases, and carried out further than had been previously done, those principles of research in natural science which have been laid down by that distinguished philosopher."

Thus, then, we see that the relations of chemistry to physiology are clear and well defined, and may be expressed as the investigation of those chemical processes which are continually occurring in the human economy, and by the performance of which alone man can live in health. Obviously, therefore, chemistry in this aspect must commence with the study of the constitution of the various tissues, organs, and juices of the body: for without such knowledge we cannot study the processes in which they participate, no more than we could express in unintelligible language the action of hydrochloric acid upon chalk without having previously ascertained the composition of these substances. Having once attained such a position we may view the results in the light of "normal chemical standards."†

In the same way the investigation of pathological processes must follow after the chemical standards of health have been determined, for by pathology we mean the doctrine of the causes and nature of disease, the very idea of which implies that the ordinary vital or chemical processes of life are deflected from the normal course obtaining in health. The method of investigation, however, is the same, and the results may be viewed as the measure of the "morbid declensions" from the ordinary standards.

It would be vain on my part to attempt to review, in the space of one hour, all the relations of chemistry to physiology and pathology, and I have thought it best to treat only of those relations which regard the most important organ of our bodies—the Brain. In doing this I shall chiefly concern myself with the recently published researches of Dr. Thudichum, which, again to quote from the "Report of the Medical Officer of the Privy Council"

(New Series, vol. iii., p. 9), show such "very remarkable evidences of success."

In the choice of my subject I have been guided chiefly by the consideration that I am well acquainted with this work, and in fact, as professional assistant to Dr. Thudichum, have been associated with him in the work itself.

The object of these researches was to unfold the chemical constitution of the brain, in order that in time to come it may be in our power "to guide or correct" those processes in disease, through the normal operation of which the brain performs its functions in health.

"It is confidently believed that its entirety has been explored in such a manner that fundamental truths cannot have escaped observation, and that what remains to be done is essentially of the character of detail, which however vast by multiplicity it may become will not alter the broad outline (to) which this investigation has led" ("Report," p. 203).

Those who may desire to study more in detail this subject I would refer to the published research, which they will find in the "Blue Book" from which I have quoted. For such a large enquiry recourse must be had to the brains of oxen, although I should state that the examination has also been conducted upon normal brains from healthy human subjects.

The brain of an ox weighs, on the average, 356 grms., or from 12 to 13 ounces; and in the research of which I am treating more than 2000 such brains have been employed.

I will first review the method of procedure in obtaining from brains the principles which I shall presently describe.

The brains are first carefully skinned, and freed by means of forceps from the arachnoid and pia-mater, after which they are cleansed from clot and blood by a temporary submersion in water, which has next to be removed by means of strong alcohol (of 85 per cent). This operation, which has to be repeated several times, dehydrates the brain matter, and eventually furnishes a watery solution, which—when freed from albumen, &c., by boiling and concentration—is found to contain, in part, the *water extracts of the brain*.

When the brains are well hardened in the way described, they are passed through a mincing machine, and the pulp so resulting is then made to pass as a paste with alcohol through a fine hair sieve.

The product is now ready for those operations which are intended to extract from it its chemical principles. It is therefore extracted now with alcohol of 85 per cent at a temperature of about 50° C., and filtered through cloths. By repeating this extraction five or six times there is finally left an *albuminous insoluble residue*, which forms next to water the body of greatest percentage in the brain.

The alcoholic extracts are united, and on cooling deposit a white crystalline and granular precipitate, to which we have given the name of *white matter*. After filtration from this, the extracts are concentrated by distillation, and the concentrated liquor allowed to cool, whereby is deposited a semi-solid viscous mass, which we have designated *buttery matter*. From this the mother-liquor is freed by filtration, after which it is entirely freed from alcohol by distillation, and is then still further concentrated by evaporation in open dishes. During this operation oily drops and lumps form; but these, when cold, become semi-solid and flaky, and can be isolated by filtration in the hot. This matter, which has for the most part the same composition as buttery matter, we shall consider under that heading.

The filtrate from the "last oily" (so-called) is concentrated to a thin syrup, and constitutes *water extracts of brain matter*, containing in solution the salts, extractives, and soluble immediate principles.

There are several methods of extracting from white matter and buttery matter the principles of which they are composed; but here I think it best to confine myself to one method, and one which is the most workable in practice; and as white matter and buttery matter are

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† "Reports of the Medical Officer of the Privy Council, &c.," New Series, No. iii., p. 8.

identical, except as regards the *relative* amounts of the various constituents, the one process is applicable to both. White matter consists principally of bodies belonging to the group of cerebrins, with cholesterin, kephalins, myelins, and lecithins: so also does buttery matter, but it contains less of the cerebrins. To isolate these various principles white matter and buttery matter undergo extraction with ether, which furnishes a red-coloured solution having a strong fluorescence, and leaves the mass of cerebrins behind as a snow-white pulverulent whole, which can be afterwards differentiated into the several varieties composing it by suitable treatment.

The ether extracts contain kephalin, to which body the fluorescence is due, cholesterin, lecithin, and some myelin. They are concentrated, and on cooling deposit much of the cholesterin in white, pearly, crystalline plates, from which the solution is separated by filtration, after which it is treated with an equal volume of absolute alcohol, which precipitates the kephalin as a reddish brown viscid mass. This kephalin can be purified by re-solution in ether, and re-precipitation by alcohol; but to obtain it in a perfectly pure state it has to undergo a special treatment, viz., solution in water, and re-precipitation therefrom by hydrochloric acid, &c.

The mother-liquor from which the kephalin has been precipitated is free from the excess of ether by distillation, and yields, on cooling, another deposit of cholesterin, after separation from which it is thoroughly precipitated by a solution of chloride of cadmium in alcohol of 85 per cent. This treatment yields a precipitate consisting of the cadmic chloride salts of kephalin, lecithin, and myelin, and a mother-liquor which is concentrated nearly to dryness, yielding a further vast mass of cholesterin.

It may be at once stated that cholesterin may be readily purified by re-crystallisation from alcohol. Sometimes, however, it is necessary to add potash in this operation to free it from certain bodies of a fatty nature, and charcoal to take out certain colouring matters.

The cadmic chloride precipitate above described is split up into its component parts as follows:—It is first extracted with anhydrous ether, which dissolves the kephalin compound, but leaves the salts of lecithin and myelin as a white pulverulent mass. From these products the immediate pure principles can be respectively isolated only by long and tedious processes.

In addition to the substances I have enumerated there are present in the brain, and are obtained from "white matter," "buttery matter," &c., certain neutral lipid matters, the history of which has not been yet thoroughly studied. In the extraction of "white matter" by ether there is left, as I have stated, a snow white pulverulent mass of cerebrins, which contains also some myelin.

The myelins differ from the cerebrins in being phosphorised, and have to be separated from the latter by special methods. The so-called cerebrins consist of at least three bodies, namely, true cerebrin, phrenosin, and kersin, and they can be respectively isolated by fractional precipitation on cooling from a hot alcoholic solution, supplemented by the application of more involved processes.

We have yet to treat of the water extracts of brain-matter. They contain hypoxanthin, creatin, lactic and perhaps other extractive acids, phosphates, sulphates, and chlorides of the metals potassium and sodium. When the brain-matter is derived from the ox, also inositol or so-called muscle sugar, is present in quantity. In addition to these matters, there are also invariably present in the brain, calcium, magnesium, iron, copper, manganese, &c., &c.,

I have thus sketched briefly the mode of isolating from brain-matter the various principles it contains, but only one form of one process has been enunciated.

TABLE SHOWING CONSTITUENTS OF THE BRAIN.

Group of Sulphurised Principles.

Albumen $C_{72}H_{112}N_{18}SO_{22}$

Group of Phosphorised Principles.

Sub-Group of Kephalsins.

Kephalin $C_{42}H_{79}N PO_{13}$
Kephaloidin $C_{42}H_{79}N PO_{13}$
Oxy-kephalin $C_{42}H_{79}N PO_{14}$
Peroxy-kephalin $C_{42}H_{79}N PO_{15}$
Amido-kephalin $C_{42}H_{80}N_2 PO_{13}$

Giving compounds with $PtCl_4$; $CdCl_2$; Pb ; acids, bases, and salts.

Sub-Group of Myelins.

Myelin $C_{40}H_{85}N PO_8$
Oxy-myelin $C_{40}H_{75}N PO_{10}$
Amido-myelin $C_{40}H_{82}N_2 PO_{10}$
Compounds as with kephalin.

Sub-Group of Lecithins.

Lecithin $C_{42}H_{83}N PO_9$
Compounds as kephalin.

Group of Nitrogenised Principles.

Cerebrin $C_{34}H_{68}N_2 O_8$
Stearoconote $C_{34}H_{68}N_2 O_8$
Phrenosin $C_{34}H_{67}N O_8$
Kersin $C_{46}H_{91}N O_9$

Extractive and secretory acids.

Uric acids and congeners.

New acids.

Extractive alkaloids (several new ones).

Urea and amido-acids.

Group of Oxygenated Principles.

Cholesterin $C_{26}H_{44}O$
Inositol $C_6H_{12}O_6$
Lactic acid, &c.

Fats and fatty acids.

Group of Inorganic Principles.

H_2SO_4 ; HCl ; P_2O_5 ; CO_2 ; H_2O ; K ; Na ; NH_3 ; Ca ; Mg ; Cu ; Fe ; Mn .

The albuminous matters from the brain offer no specific difference from similar matter derived from other parts of the body, and exist mainly in the form insoluble in water.

The kephalins are all soluble in ether to a red colour, and possess a great affinity for oxygen, and also exhibit a wonderful power of combination, which is well explained by the constitution of the bodies. They contain their phosphorus in the form of glycerophosphoric acid, which they yield along with neurine when split up by boiling with baryta water. The first member of this sub-group, viz., kephalin, $C_{42}H_{79}NPO_{13}$, has been especially well studied—so well, indeed, that of its individuality there is no more doubt than of the individuality of each of ourselves. It is the more interesting seeing it forms one of the bodies discovered in brain-matter for the first time by Thudichum.

The myelins are white and are not liable to atmospheric oxidation. They are not to be confounded (from their formulæ) with lecithin, which in the form described by Strecker (as obtained from eggs) is very unlike the myelins. They combine with lead, and from the lead compounds can be obtained the free principles in a purified condition by decomposition with sulphuretted hydrogen in hot alcohol, from which solution on cooling they are deposited in forms easily recognisable under the microscope.

In this lecture the name of lecithin has been repeatedly used as significant of a certain body present in and obtainable from brain-matter. It has been obtained mainly in combination with platinic chloride as hydrochloride, but this salt proves so unstable as to defy in great measure those attempts which have been made to elucidate its nature. But it has proved at least to possess characters different in some respects from those attributed to the lecithin described by Strecker and Goble, although it appears to yield similar products of decomposition. My personal belief is that the lecithin of Strecker does *not* exist in brain-matter: meanwhile it may be allowed that

much more work will have to be done in this direction before very absolute statements can be made regarding this really marvellous substance.

Cerebrin, stearoconote, phrenosin, and kersin, are all well characterised substances, and have been deeply studied. The first two are isomeric, stearoconote being formed from cerebrin, when the latter is heated in alcohol, at a greater speed than it can be dissolved, when it fuses and becomes insoluble. But the product is readily soluble in benzene, while cerebrin is absolutely insoluble in cold benzene; from the benzene solution the stearoconote is precipitated by alcohol.

Cerebrin seems to be the di-amidated form of a fatty acid, whilst phrenosin appears to constitute the mono-amidated form.

Kersin has a microscopical character extremely easy to recognise, and of such form as to readily enable one to discover the presence of foreign matters.

These four substances are especially distinguished by their solubility in hot alcohol, and by the purple colour they give with sulphuric acid and sugar, a reaction similar to Pettenkofer's test for bile. This reaction I will now illustrate.

Inosite is a sort of sugar, and so far we have not specially sought for it in human brain-matter, although it is probably present, while in the brain of the ox it exists in very decided amount.

Of the other bodies to which we have alluded, as occurring in the brain, I shall only dwell on the presence of copper and manganese, because it has been represented to me that by some scientific men this statement has been regarded with doubt. We have worked up several thousand brains, and whenever we have searched for them we have always found them, equally when the brains were those of oxen or from human beings. In the case of copper we have reduced it to the metallic state, from a suitable solution, by an electric current, and before you there is a solution of copper (from one and a half human brains) in which we will now demonstrate its presence.

Moreover, the presence of copper in the brain is after all not so remarkable, for who does not know that Church discovered copper in the colouring-matter of the feathers (wing) of a bird, and recently M. Bergeron and L. L. Hôte (*Comptes Rendus*, lxxx., 268) have published experiments which they say have demonstrated the presence of copper in the kidneys and livers of fourteen human bodies.

But these experiments of Bergeron and Hôte were, we may say, unnecessary, for they simply amount to repetitions of those performed by Odling and Dupré many years ago. These chemists published a most interesting paper "On the Existence of Copper in Organic Tissues," in which they described whole series of experiments made upon comparatively large quantities of materials, in the years 1856 and 1857. These observers also refer to literature previously existing on the subject, which seems to indicate that so long ago as the latter end of the last century the presence of copper in organic tissues and products had attracted the attention of Margraff, Gahn, and Vauquelin.

Odling and Dupré detected copper in bread, flour, wheat, straw, liver, kidney, blood, flesh, eggs, cheese, &c., and in many instances estimated the amount by most reliable methods.

From one of the Tables published by these chemists we take the following determinations:—6925 grains of human liver furnished 0.013 grain of oxide of copper, while 6682 grains of sheep's liver furnished 0.281 grain of cupric oxide. 1830 grains of human kidney gave 0.015 grain of copper oxide, while human muscle and blood furnished traces. In short, they conclude that "the tissues, particularly of the liver and kidney, usually contain copper in very notable quantity," and that "the blood usually contains but very minute traces of copper." At the same time they thus express themselves:—"We are scarcely

prepared to maintain that copper is a necessary and invariable constituent of living organisms."

There is one property common to many of the principles derivable from brain-matter, but particularly striking in the case of kephalin. When treated with water, kephalin swells and forms an emulsion, which ultimately becomes an imperfect turbid solution,—that is to say, it dissolves in a certain manner and in a certain measure, although it passes through many folds of Swedish filtering-paper in its turbid state. This substance, however, will not dialyse, and is therefore practically a colloid.

On the other hand, the bodies under the heading "Nitrogenous Group"—including cerebrin, phrenosin, and kersin—behave somewhat differently towards water, in which they swell to gelatinous masses without afterwards dissolving. Thus a few grammes of cerebrin will swell to one hundred times (or more) its original volume. Here I have a little cerebrin in water, and on hastening the operation by the aid of heat, it swells like starch, and becomes a solid mass.

The phosphorised principles of which we have treated are possessed of acid, alkaline, and alkaloidal characters,—that is to say, they combine with alkalies, acids, and salts, with great avidity. In the several vessels before you there is contained a weak (1 per cent) solution of kephalin in water, and I will now add hydrochloric acid to the first, sulphate of copper to the second, sodium arsenite to the third, and platinic chloride to the fourth, when there is produced a precipitate in each case. This power of combination, however, can be overcome by water, so that if these precipitates were placed in water, on a dialyser, the respective combinants would again pass out, leaving behind pure kephalin. Although this affinity for water that kephalin exhibits is so strong, yet it is not so strong as that exhibited towards metallic oxides,—as lead, manganese, copper, and iron, which can be dissociated only by mineral acids.

I cannot do better here than quote two paragraphs from the summary Thudichum has given of his researches (see p. 199 of "Blue Book") :—"We have therefore here a diversity of affinities such as is not possessed by any other class of chemical compounds in nature at present known; and the exercise of these affinities being greatly influenced by the mass of reagent and the mass of water which may be present, the interchange of affinities may produce a perfectly incalculable number of states of the phosphorised, and consequently of brain-matter. This power of answering to any qualitative and quantitative chemical influence we may term the state of *labile equilibrium*; it foreshadows, on the chemical side, the remarkable properties which nerve-matter exhibits in regard of its vital functions.

"From this it also follows that nerve-matter (if only characterised by the phosphorised bodies) must yield obedience to every, even the slightest, external chemical influence which may reach it by way of the blood. It must take up metals, acids, salts, alkalies, and alkaloids presented by the blood; it can retain only oxides when the serum is again free from the combinants; a watery serum will wash the brain; a more watery one will make it swell and displace mechanically within physiological limits what it can; a still more watery one will make the brain dropsical, and produce all the conditions of mechanical pressure on the brain. All these processes are the necessary consequences of the affinities of the phosphorised substances, and, these being known, the phenomena could be predicted, if they were not sufficiently known as phenomena, though hitherto destitute of an explanation. Thus the so-called brain-fungus, the continued protrusion of brain-matter through apertures of the skull produced by mechanical injuries, may, in certain cases, find a physical explanation in simple excessive hydration of the phosphorised (and nitrogenised) principles, producing general inter-cranial pressure."

Bearing in mind what I have just read to you, in cases of poisoning by arsenic or mercury, we should expect to

find these bodies present in nerve-matter after death had resulted, in such instances, and there are records which bear out the truth of these observations. Indeed but recently D. Scolosuboff (*Bull. Soc. Chim.* [2], xxiv., 124), (*Journ. Chem. Soc.*, Ser. 2, xiv., 92) has published some experiments in which he administered doses of sodium arsenite to dogs, rabbits, and frogs, which show that *the poison concentrates principally in the nervous substance*, smaller quantities being found in the muscles, liver, and marrow.

These experiments following after the research of Thudichum only confirm, in the strongest possible way, the statement of Thudichum that "nerve-matter must take up metals, acids, salts, alkalies, and alkaloids, presented by the blood."

The phenomenon of the assimilation of arsenic had therefore been predicted, and D. Scolosuboff, by his experiments, have proved the truth of the prediction.

"These few examples," again to quote the words of Thudichum, "show that the acquisition of chemical statics leads almost necessarily and very easily to chemical dynamics of the brain; and these will, in their turn, furnish data for physiological and pathological conclusions. But these deducive arguments must be most sparingly and cautiously used until the statics are in a state of perfection and completeness. To argue too far from incomplete data would—seeing the history of biological chemistry during the last thirty years—be a deplorable error."

Liebig clearly showed how absolutely essential to plant growth was the minute amount of saline ingredients which plants leave in the form of ash when burnt, and it may now be accepted as an absolute truth, that, however minute the percentage proportion of each of these ingredients, they are individually as essential to the healthy plant-life as a supply of the most weighty constituents. Who can say, therefore, that the small amounts of inorganic ingredients of the brain are not equally necessary to its perfect development, and consequent functions, and are not present merely by *accident*? That this may be so is the more probable when we reflect that these saline matters are in combination with some of the principles; for unless special methods have been employed to rid cerebrin, phrenosin, kephalin, &c., from these matters, you will find them invariably present, no matter how many times you have re-crystallised from alcohol the first two named principles, or dissolved in ether the other, and re-precipitated it by alcohol!

It is perhaps less probable that the small amount of copper in the brain fulfils any useful function, and it is perhaps, therefore, present as accident only.

Again, in certain cases of softening of the brain, Dr. Thudichum has observed the presence of glycerophosphoric and fatty acids in the free state. As these matters are *never normally* present, and as they constitute the decomposition products of some of the phosphorised principles, the idea is forced upon us that softening of the brain may be expressed chemically as the decomposition of one or more of the phosphorised principles into proximate nuclei. There are circumstances, as we have seen, which seem to corroborate the truth of this idea, but it may be absolutely untrue; in short, it is a thought, and not an unscientific one,—*i.e.*, a reasonable hypothesis.

Nerve-matter is made up of heterogeneous principles, arranged in such a manner as to vanish completely from appearance as chemical individuals. I will once more quote from Dr. Thudichum's publication ("Blue Book," p. 197). Dr. Thudichum there writes:—"The first striking fact which meets the enquirer is that nerve-matter contains abundance of water. This, in conjunction with the peculiar manner in which the water is contained, engenders a mobility of ultimate particles within certain limits of movement. It also gives penetrability by liquid diffusion, while excluding porosity and its capillary effects: by which means a ready nutrition by diffusion in one direction, and ready cleansing from the *effete* crystallisable products of life in another are ensured. Consequently the

brain, as a whole, is essentially made up of colloid matter, and may be compared to a colloid septum, on the one side of which is arterial blood and cerebro-spinal fluid of the ventricles; on the other side, however, is cerebro-spinal fluid of the arachnoidal space and venous blood. It follows from this that the large amount of water present in the brain is not there, so to say, mechanically only, like water in a sponge, and capable of being pressed out mechanically, but is chemically combined as colloid hydration water, or, better, *water of colloidation*."

Dr. Thudichum has shown us that the healthy nutrition and functions of the brain depend upon its constitution, in which the character of the water present is most important; and I have pointed out to you that when brain-matter, after life, is placed in alcohol, it loses its water, its mobility of particles, and becomes more solid and firm. Is this sort of thing possible in life? Is it possible that, in cases of *delirium tremens*, so much alcohol has been consumed, as, by its diffusion through the brain in the form of blood, it has robbed nerve-matter of its mobile character, and consequently of its power to assimilate its food, and of its power to throw off the products of its life functions? These are questions which future research may entirely negative, but they constitute the bases of thoughts that arise in spite almost of ourselves, and again they constitute a reasonable hypothesis.

We have seen that the principles of which brain-matter is composed are, *en masse* (with the exception of the albuminous framework), soluble in warm alcohol, although the individual principles are not all soluble under these conditions. Now I have asked myself this question—Can a man consume so much alcohol in the form of stimulants, and retain enough in his blood in the unoxidised condition, to dissolve traces of matter from his brain? However horrible the thought may be, it is less horrible than the disease known as *delirium tremens*.

I have prepared a dilute solution of alcohol, and in it I have placed pieces of brain-matter, as derived from the ox, at a temperature of the blood, *viz.*, 100° F. At this temperature it has been digesting some hours, and I will now filter it. On cooling, the filtrate throws down a white deposit of matter which the alcohol has dissolved,—a phenomenon which would seem to indicate some actual truth in Shakespeare's words "Oh, that men should put an enemy in their mouths to steal away their brains."

I will now submit to you a few instances which are calculated to exhibit, in regard to other vital functions, the relations of chemistry to physiology and pathology. In doing this I will choose the most familiar ones, namely, the function of the blood and its aëration, and the chemical processes of digestion.

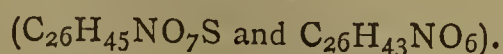
The obvious function of the blood is to supply nourishment to, and carry away, the products of life from all parts of the body. Its constitution is extremely complex, and may vary—according to the nature of the food eaten and other causes—within certain limits; but, whatever its constitution, there are invariably present in healthy blood those small formations known as blood-corpuscles. The composition of these corpuscles depends upon the exercise of their own powers in relation to the surrounding serum of the blood, and is complicated. The most striking constituent is a body which has received the name of hemato-crystalline—a body which gives to the corpuscles their red colour. This body always contains 4-10ths per cent of iron, and its analyses lead to a formula for it of $C_{600}H_{960}N_{154}FeS_3O_{177}$, with an atomic weight (the greatest ever assigned to a principle) of 13,280. This formula may not be absolutely correct, but that the atomic weight is extremely high there can be no doubt, on account of the complicated function it fulfils in the body, and its probable constitution. That constitution can only be learnt by chemolysis,—that is, by splitting it up by suitable means into the proximate constituents, and these again into their more ultimate derivatives. Omitting other substances present in corpuscles, let us centre our

attention upon this hemato-crystalline, which is literally crystalline and of the rhombohedric system (see Thudichum's "Physiological Chemistry," p. 27). A solution of this body in water, when diluted sufficiently and examined spectroscopically, shows two bands of absorption, and "as the blood of all vertebrate animals, when viewed within the living blood-vessels, shows the same bands, we can assume that hemato-crystalline is present in it as such, and not formed by the process of preparing the crystals."—(Thudichum.) Now Stokes many years ago showed that these two bands were characteristic of arterial blood, for venous blood exhibits only one band; and that these properties are attached to the hemato-crystalline is proved by depriving it of its oxygen, when its spectrum furnishes the one absorption-band of venous blood; but by shaking again with oxygen the double-band spectrum is restored. With these data before us, let us endeavour to follow these properties of hemato-crystalline contained in blood-corpuscles into its life functions as displayed in the process of respiration.

That these corpuscles are the carriers of oxygen there can be little doubt; for the oxygen is not held in the blood like as carbonic acid exists in seltzer-water (that is, by solution), but it exists there in combination. In combination with what? We have seen what affinity hemato-crystalline has for oxygen, and it is reasonable to suppose that these bodies combine in a loose way; and in this form of combination the oxygen is carried by the blood circulation to the most distant and every part of the body, where it is given up to the tissues, and they in their turn return carbonic acid, water, urea, and other substances which are the products of the metamorphoses of the tissues to the blood. Now this carbonic acid of which we have just spoken is in part dissolved by the blood in a similar manner to that in which it exists in soda-water, but not entirely so, for to a great extent it is combined with alkaline bases, particularly sodium (Thudichum). Now Thudichum supposes that when the venous blood reaches the small breathing cells, the hemato-crystalline is oxidised partially into an acid, which he calls hematic acid, which—passing into the serum when the corpuscles arrive in the breathing-cells—decomposes the carbonates in the blood, setting free the carbonic acid, which, with water-vapour, escapes through the lung tissue into the respiratory passages.

We shall return to this subject when we come to speak of certain inductive processes of reason which J. Stuart Mill developed.

In the meantime let us pass on to consider certain changes of matter which occur in the stomach during digestion, a process which is mainly carried on through the agency of hydrochloric and lactic acids, which would seem to be formed by the decomposition of chloride and lactate of sodium in the glands of the stomach into free acids and caustic soda. But inasmuch as we nowhere meet with caustic soda in the animal economy, at first blush the above explanation would seem to be untrue; but it is easy to comprehend, under the ingenious theory of Dr. Thudichum (see his "Chemical Physiology," p. 16, and *Brit. and For. Med. Chir. Review*, October, 1861, p. 429; see also an enquiry by Pavy in *Phil. Trans.*, 1863), who supposes that before the soda enters the blood, and is carried away thereby, it has a function to perform, viz., "to protect the stomach against the corrosive action of its own secretion." Under certain abnormal conditions this corrosive action obtains, and gives rise to gastric ulcer and other pathological processes. In the blood the caustic soda soon becomes carbonate, and the presence of sodium in the bile may be proved by adding to it, say, hydrochloric acid, when at the end of the experiment sodic chloride will be found, whereas it was not present before, but the sodium had been in combination with the binary acids—taurocholic and glykocholeic—



Bearing on the processes we have just been considering,

allow me to quote a few words written by Mill in 1856 ("System of Logic," fourth edition, 1856), and which resulted from a consideration of Graham's researches, which demonstrate "that gases have a strong tendency to permeate animal membranes, and diffuse themselves through the spaces which such membranes enclose, notwithstanding the presence of other gases in those spaces." Mill then says (vol. i., p. 523):—"The exchange of oxygen and carbonic acid in the lungs is not prevented, but rather promoted, by the intervention of the membrane of the lungs and the coats of the blood-vessels between the blood and the air. It is necessary, however, that there should be a substance in the blood with which the oxygen of the air may immediately combine, otherwise, instead of passing into the blood, it would permeate the whole organism; and it is necessary that the carbonic acid as it is formed in the capillaries should also find a substance in the blood with which it can combine, otherwise it would leave the body at all points instead of being discharged through the lungs."

Now although respiration is to be considered as an act of secretion, yet you have seen how true were these prophecies of Mill. The oxygen of the air combines with the hemato-crystalline of the blood, and the carbonic acid evolved in the system combines with the soda (in some measure) present also in the blood. Liebig, however, disputed the presence of carbonates in the blood, on the evidence of some analyses he made of the ash of blood; but in all probability the phosphorus which we now know to be present in blood, both in the form of phosphates and as phosphorus in organic combination, had determined the substitution, and consequent expulsion, of the carbonic acid in Liebig's experiments.

"The physiologist, J. B. Willbrand, represented disease not badly as the battle of the chemical process with the organic process of life. The chemical process is victorious in death" (Thudichum, Cantor Lectures, *Journ. Soc. Arts*, vol. xxiv., p. 127); but it also makes sad ravages during life, and perhaps no disease illustrates in a better way the fact that in disease the chemical processes obtaining in health are deflected from their course than the disease designated "locomotor ataxia." In this disease the sense of pressure is either entirely or partially destroyed, and doubtless all have seen at times such cases in which the sufferers proceed step by step, and with that heavy tread which seems to indicate that they are not quite certain when their feet first touch the ground; and in truth this is so, although they are still susceptible to the senses of pain, heat, and cold. Well, in this disease, the spinal marrow undergoes what is termed "degeneration." There is, that is to say, a chemical process at work in the substance of the marrow which results in the production of a body, not yet well studied, but which is quite abnormal, and consists of matter like to cellulose and starch corpuscles. What is the nature of this body, and how has it been produced? These are the questions that chemistry must solve, and they can only be solved when it is in our power to make a quantitative analysis of nerve-matter. We are already far advanced on this particular road of research, and truths which now take the form of nebulous patches of ideas will develop into the constellations of truth. In fact, we stand mentally in the condition of one who, having been blind, receives some degree of light, and "sees men like trees walking."

When we can answer the questions I have asked, then comes in the possibility of guiding or correcting those processes constituting "amyloid degeneration," either in a preventative or curative way, but till then never, except it should be hit upon by sheer accident.

In Mr. Simon's "Report" from which I have previously quoted (page 79), he writes:—"Especially a leading fact in typhus, one which may be in intimate relation to the killing power of the disease, and which it is on all accounts necessary to have as completely as possible understood, is the fact of the action on the nervous system; and clearly no true chemical knowledge of that

morbid action would be possible till greatly improved knowledge of the normal brain chemistry should be supplied."

Mankind in general little reflects how much it owes to chemical science and to those chemists in particular who have devoted their lives to the sake of truth. What familiar substances are chloroform and chloral, and how much gratitude ought to be experienced by us towards the eminent discoverer of these bodies—the late illustrious Liebig? While nearly all of us have escaped from pain by the application of the one, or enjoyed sleep by the use of the other of these substances, there are many who are ignorant alike of their discoverer, and the processes by which they are obtained, caring still less to learn how these wonderful bodies exert their benign influences. These influences are doubtless the result of reactions on brain- and nerve-matter, but what these reactions are is a question from the solution of which we are perhaps as far removed as from any ultimatum in science. But we shall know hereafter. Meanwhile science is struggling hard to obtain an insight into these profound processes, and perhaps this is the right place to briefly notice those recent researches of Messrs. J. G. M'Kendrick and Dewar on the physiological action of certain bases of the so-called chinolin and pyridin series.

These observers administered these bases, which are derived from quinine and allied bodies by distillation with potash, and which constitute two homologous series, by subcutaneous injection. Now M'Kendrick and Dewar find that the physiological action of these bases, which consist of carbon, hydrogen, and nitrogen in varying atomic proportions, is entirely upon the various parts of nerve- and brain-matter, but further, they have encountered some striking facts which seem to show that the intensity and sort of action is related in each case to the chemical composition and constitution of these bases. Here I shall only quote one conclusion they infer from their observations. (See *Proc. Roy. Soc.*, vol. xxiii., p. 297). It is as follows:—

"On comparing the action of such bases as C_9H_7N (chinolin) with $C_9H_{13}N$ (parvolin) or $C_8H_{11}N$ (collidin), with $C_8H_{15}N$ (conia from hemlock) or $C_{10}H_{10}N_2$ (dipyridin), with $C_{10}H_{14}N_2$ (nicotine from tobacco), it is to be observed that, apart from difference in chemical structure, the physiological activity of the substance is greater in those bases containing the larger amount of hydrogen."

Here the conclusion drawn is only the fact expressed in another way, but of course much more work is necessary before this can constitute or prove to be a law. This work must endeavour to ascertain what becomes of the substances subcutaneously injected, and *how* they act on the centres of the brain organisation. Let us consider that 1 grain of some of these bases per pound of weight (for a rabbit) is sufficient to cause death, and we can readily conceive the difficulty encountered in endeavouring to trace the substance itself through the system, although we know it must act by way of the blood. Beyond this human efforts have not yet extended, and in these particular cases the effects have been witnessed, while the causes are almost entirely hid.

It is not necessary for me to dwell long on the importance of acquiring knowledge relative to the brain in particular; for seeing it is the "seat of powers, essential to the production of those phenomena which we term intelligence and will" (Huxley); seeing that from it spring those nerves through the functions of which we enjoy the uses of our principal and subordinate senses; seeing, indeed, that the whole body of man is in a sort of subordination to his brain and nerves,—is it necessary for us, who are the beings concerned, to ask what importance is to be attached to research, be it chemical, physiological, or otherwise, conducted upon these matters? We should rather take up the maxim of the old alchemists, of "those mistaken but often grandly energetic men, who said to the disciples of their art," as Mr. Rodwell has told us (in his

"Birth of Chemistry"), "Ora! Lege, Lege, Lege, Relege, Labora, et Invenies."

Our slight knowledge of the constitution of bodies occurring in the human system, and of the processes by which they are built up from food, and transformed into ultimate products after fulfilling the vital functions, only shows how much yet remains to be learnt. We soon perceive that we stand but on the threshold of knowledge, and many must be the thinkers and workers, and generations upon generations will pass away, before the processes of life shall stand forth entirely revealed; before it shall be known how from a few simple matters, tolerably well known themselves, and administered as food, is built up man, with his marvellous and beautiful structure,—with his power of thought, feeling, and action. That such a time will come scientific men have no doubt; we shall know more to-morrow of the sun that shines to-day!

Let us for one moment consider the value of knowledge which must prove so powerful in preventing or curing those diseases which now so often fritter away men's lives in misery and suffering. All knowledge leads to happiness of mankind; the discovery of a new dye gives rise to a new industry, and places in the hands of man that which—by pleasing his taste—increases his happiness; and so, to quote from Bacon, all those "industrious observations, grounded conclusions, and profitable inventions and discoveries" (see Macaulay's "Essay"), are securities for the health and comfort of man.

The processes of life are involved in such darkness at present as to make one very cautious in expressing or accepting statements made regarding them, but when there is scientific evidence, as there is in the cases we have considered, and when expressed views explain well what occurs, those views are entitled to respect. For although I have said there is darkness around the vital phenomena, that darkness exists only because Science as yet has not attacked Nature sufficiently in these directions. But a continuation of work will dispel doubts, will abolish untruth and mystery often amounting to superstition, and will substitute a rational explanation of life and the chemical processes which sustain it. There will always exist men who will doubt the possibilities of science,—who will call in question matters which have been established as truth, just as men—even to Bacon—rejected the doctrines of Galileo, and as Liebnitz spurned the philosophy of Newton on gravitation. But, in spite of all, the sun of Science will dispel the morning clouds of ignorance and prejudice, and will unfold to us the laws of Nature, unbiassed by the poetry of man's mind and free from the superstitions of churches.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, February 17th, 1876.

Professor ABEL, F.R.S., President, in the Chair.

AFTER the minutes of the previous meeting had been read and confirmed, and the list of presents to the Society announced, the President, in accordance with the bye-laws, read out the names of the Officers and Council proposed for the ensuing year. Messrs. J. S. Walton, G. Head, J. H. Starling, P. J. Winsor, J. Davison, P. Holland, and A. A. Nesbit were then nominated; and Messrs. George William Typke, Francis S. Earp, Henry Mitford Faber, B.A., Gerrard Ansdell, John May Herbert Munro, N. Bettson Cooke, Walter Pearce, George Harrow, John Parry, William Ashwell Shenstone, and Charles Williams were balloted for and duly elected, after their names had been read the third time.

The PRESIDENT said he had been requested by the Committee of the Council for Education to call the attention of the Fellows to the proposed loan exhibition of scientific instruments, and to invite the Fellows to send chemical specimens or apparatus of historical or scientific interest. He also had to announce that Prof. Andrews, of Belfast, would deliver a lecture "On Certain Methods of Chemical Research," on Friday, April 28. The thanks of the Fellows were due to Mr. James Duncan for the handsome marble bust of Dr. Hofmann which he had given to the Society. It was a most lifelike and spirited likeness of that eminent chemist.

The PRESIDENT then called on Professor Frankland to deliver his lecture "*On Some Points in the Analysis of Potable Waters.*"

The LECTURER said it was now eight years since he had the honour to lay before the Fellows an account of the principal methods for water analyses. The result of that investigation was that of the four processes used for the estimation of the organic constituents, viz., the "ignition," "permanganate," "albumenoid ammonia," and "combustion" methods; the last mentioned was the only one yielding trustworthy quantitative results. The two chief objects to be kept in view were the estimation of *present* organic impurities, and the evidence of *past* pollution, both of which were of incalculable importance from a sanitary point of view. This the Lecturer illustrated by a most striking case of the propagation of typhoid fever at the village of Lausen, in Switzerland. The previous animal contamination of water, as deduced from chemical analysis, must always be regarded as a minimum quantity, since we look in vain for the full evidence of such contamination in the effluent water from fields irrigated with sewage: aquatic vegetation also slowly removes these compounds from water, and they are likewise destroyed by putrefaction. The actual present organic matter in water can only be ascertained by the estimation of the carbon and nitrogen; and experiments made by adding known quantities of organic substances to pure water, and then analysing them, showed that the average error did not exceed 1-22nd of the total amount present. This difference would have no appreciable effect on the opinions of an analyst respecting the quality of a water.

The Lecturer then proceeded to describe the improvements which had been made in the process as originally described by Dr. Armstrong and himself; the chief of which consisted in the evaporation of the water under a glass shade. The water by this means was brought in contact with but a limited quantity of air, and yet the evaporation took place almost as rapidly as when it was freely exposed.

The Lecturer then described the diagrams, the curves in which illustrated the amount of organic matter in the water supplied during the past eight years by the eight London water companies, pointing out how floods were invariably accompanied by an increase in the amount of organic matter present in the water; besides many other interesting points, such as the effect of the decay of vegetation in autumn; the advantages and disadvantages of large storage power; and the danger of surface leakage in improperly constructed reservoirs, as most strikingly shown by the analyses of the Kent company's water in the years 1868 and 1869. These diagrams, giving as they do the results of more than eight hundred analyses, showed most distinctly that the combustion method of analysis was competent to reveal the finer shades of quality in water drawn from the same source, but treated differently by the various water companies. Against these advantages it must be acknowledged that the process involved more trouble and more careful manipulation than were usually bestowed upon "commercial analyses;" but, at the same time, there was no more simple method by which trustworthy quantitative results could be obtained.

The albumenoid ammonia method of determining organic nitrogen rarely gave the total nitrogen present in

organic bodies as ammonia, and, what is of more importance, the proportion evolved varied very widely with different kinds of organic matter. This was shown by the results obtained by Messrs. Wanklyn, Chapman, and Smith with twenty-five different nitrogenous substances, of which only five yielded approximately the whole of their nitrogen as ammonia: of the remainder, some evolved one-half, some one-third, one-fourth, or even none of their nitrogen as ammonia. From this it was evident that this process could give no evidence whatever touching the proportion of organic matter present in waters; neither could it indicate the relative quantities of organic nitrogen, even in different samples, unless the proximate constituents of the organic matter happened to be of the same kind, and in the same relative proportions; a coincidence which could obviously seldom or never occur. It was not surprising, therefore, that great irregularities and discrepancies should occur in the results obtained by this process, and those furnished by the combustion method. An opinion based upon the albumenoid ammonia method must almost necessarily be erroneous, like that to which the authors of the process themselves were led when they declared the water of Bala Lake and that supplied to Manchester from the Derbyshire hills to be no purer than Thames water. To sum up:—

1. The albumenoid ammonia process affords no evidence of the absolute quantity either of the organic matter or of organic nitrogen present in potable water.
2. It does not indicate, even approximately, the relative quantities either of organic matter or of organic nitrogen in different samples of such water; and—
3. It affords no indication either of the presence or of the proportion of *albumenoid* as distinguished from other nitrogenous organic compounds.

The applause with which the lecture was received having subsided,

The PRESIDENT said whatever difference of opinion might exist amongst those present as to the comparative value of the method advocated by Dr. Frankland, there could be no doubt as to the unanimity with which they had expressed their thanks to him for his excellent lecture. He thought it would be as well to confine the discussion, as far as possible, to three principal points—(1) The question of pre-existing sewage contamination; (2) the direct determination of carbon and nitrogen by combustion; and (3) the value of the so-called albumenoid ammonia process. Although the advanced hour precluded their entering fully into the subject, he would at all events ask Mr. Wanklyn if he would be good enough to open the discussion.

Mr. WANKLYN said it was pretty nearly eight years ago since he had been at any of the meetings of the Society, and almost the last time he was there he had urged objections against the process of Frankland and Armstrong, which had not been answered. There were objections to it as a process of manipulation. If a sufficiency of an organic substance were taken, say, 0.200 grms., by careful manipulation the carbon could be determined within 1-200th of the amount, and the process for nitrogen was about as accurate. The limit of error being about 0.5 milligram on the carbon when 0.2 grain substance was taken, whilst Dr. Frankland only took about 0.02 grain, it was necessary to show that the possible error was diminished by at least one-tenth, or to within 0.05 milligram. He did not believe that any improvement in the manipulation would overcome this difficulty. In the experiments which the Lecturer had made to test his process by weighing out known quantities of nitrogenous substances, and adding them to pure water, he had omitted to add any nitrate. Now one of the greatest difficulties which were encountered was to destroy the nitrates without affecting the organic constituents. He would also advert to one other objection which was fundamental, as it was inherent to the process, and could not be avoided by any manipulation, however careful. If sawdust was moistened, and

a current of air passed over it, it was found that carbonic acid was produced: prolonged exposure to moisture in presence of atmospheric oxygen changed it. How much more likely that the prolonged exposure of the organic matter in water at a high temperature in presence of air should change it; a portion of the organic matter of the water was sent into the air, and Prof. Frankland made a combustion of the remainder. An objection which the Lecturer urged against the albumenoid ammonia process was that the results obtained by it and by his method did not agree: he never expected they would. It was not professed that the process showed the total organic nitrogen. By taking albumen, and treating it by one modification of their process, you could obtain the whole of the nitrogen as ammonia, and by another modification only a definite fraction of it, and yet he had deliberately rejected the former and adopted the latter for the determination of nitrogen in waters. When he obtained ammonia from water by this process, the amount of ammonia expressed rigidly the total amount of albumen and albumenoid substances in the waters.

Dr. WARREN DE LA RUE said he did not rise to make any remarks on water analysis, but thought that as the subject was one of great interest it would be better to adjourn the discussion until the next meeting.

The PRESIDENT accordingly adjourned the discussion until Thursday, March 2.

CORRESPONDENCE.

PRODUCTION OF ANILINE-BLACK BY MEANS OF VANADIUM SALTS.

To the Editor of the Chemical News.

SIR,—In the CHEMICAL NEWS, vol. xxxiii., p. 70, I observe an article copied from the *Bulletin de la Société Chimique de Paris*, "On the Production of Aniline-black by Means of the Salts of Vanadium," by M. A. Guyard, which commences thus:—"Since the interesting discovery of M. Pinkney is known," &c. Permit me to say that although this may be a discovery of M. Pinkney, it was known and published in 1871 by Mr. John Lightfoot, the original inventor of aniline-black.

In a pamphlet entitled "The Chemical History and Progress of Aniline-Black," printed for private circulation, and of which a copy was sent to every calico-printer in Great Britain who was accustomed to print this colour, at page 32 Lightfoot gives the result of experiments on producing aniline-black, in which he successively tried the following metals:—"Copper, iron, vanadium, uranium, nickel, lead, zinc, antimony, tin, manganese, chromium, bismuth, arsenic, titanium, tungsten, cadmium, tellurium, molybdenum, mercury, silver, gold, platinum, palladium, rhodium, iridium, aluminium, osmium, cobalt, ruthenium, thallium, magnesium, indium, rubidium, cerium, glucinum, zirconium, lanthanum, didymium, erbium, yttrium, selenium, tantalum, niobium." The cloth was aged for twelve hours in a warm moist room, then washed in soap and water. The result was this:—"The greatest development of the black was due to the vanadium; next in order was copper; then uranium; and, lastly, iron. Except very slight shades of blue no action was apparent from any of the other metals."

I know that the late John Lightfoot went to very great expense to procure specimens of all the metals above enumerated in a pure state.—I am, &c.,

JAMES HIGGIN.

Manchester, February 21, 1876.

VANADIUM.

To the Editor of the Chemical News.

SIR,—Referring to a paper which appeared in the CHEM. NEWS (vol. xxxiii., p. 70), written by M. A. Guyard, it may interest your readers to know that we have been

engaged for some years in developing the sources of the rare and hitherto costly metal vanadium, and in the extraction and preparation of its salts on a large scale, which we are now in a position to supply at a price, and in sufficient quantities, to make it commercially available.—We are, &c.,

JOHNSON, MATTHEY, AND CO.

Hatton Garden, February 23, 1876.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Seances de l'Academie des Sciences. No. 5, January 31, 1876.

Thermic Researches on the Formation of the Alcohols, and on Etherification.—M. Berthelot.—A thermo-chemical paper, not suitable for abstraction.

Account of Experiments undertaken for the Determination of the Work Performed by the Magneto-Electric Machines of M. Gramme, used for the Production of Light in the Establishment of MM. Sautter and Lemonnier.—M. Tresca.—As regards the relative expense of different modes of lighting the following figures may have a certain interest:—1850 Carcel burners would require a consumption of 1850×0.040 kilo. of oil = 71 kilos. of oil per hour; or of 1850×0.105 cubic metre of gas = 194 cubic metres; or of 7.56×4 kilos. of coal (used in furnishing power for the Gramme machine) = 30.24 kilos. of coal. Under these circumstances the outlay in fuel does not represent more than the one-hundredth part of the cost of oil, or the fiftieth part of the cost of gas, taking Paris prices. The applications of the magneto-electric machine for lighting are multiplying.

Researches on Rotatory Magnetic Polarisation.—M. H. Becquerel.—It results from these researches that the molecules of bodies submitted to the action of magnetism interfere by their specific action with the phenomenon of the rotation of the plane of polarisation of light, and that for highly magnetic bodies this action varies with the mutual distance of the active molecules.

Formation of Hail.—M. G. Planté.—This paper requires the accompanying illustrations.

Congelation of Mercury by the Use of a Mixture of Snow and Hydrochloric Acid.—M. G. Witz.—If the acid is previously cooled to -15° or -16° the final temperature attained, with equal weights of snow and acid, is from -32° to -35° . If the acid is cooled down to -18° mercury may readily be frozen on the large scale.

Ferment of Urea.—M. Musculus.—The ferment of urea has none of the characteristic properties of organised ferments. It has much resemblance to the soluble ferments, such as diastase, saliva, &c. It is not capable of converting acetamide, oxamide, &c., into ammoniacal salts.

Elements of Inverted Sugar, and on their Presence in Commercial Sugars.—M. E. Maumené.—Not suitable for abstraction.

Reimann's Farber Zeitung,
No. 3, 1876.

Recognition and Examination of Eosin.—Although eosin in body is distinguished from the red coal-tar colours—magenta, saffranin, and corallin—by its greater solubility in water, and by the splendid fluorescence of the solution, it may often be necessary to examine if this costly dye-ware is unsophisticated. For this purpose the best reagent is sulphuric acid diluted with four volumes of water. With this liquid magenta and corallin yield a golden-yellow, and saffranin a violet-blue solution, whilst eosin remains quite undissolved, forming an orange-red coagulum. If previously in solution it is precipitated by

the dilute acid, whilst possible impurities remain dissolved, and can be subjected to further examination. To detect eosin upon the fibre a concentrated aqueous solution of sulphate of alumina (1 part in 4 of water) is proposed, the reaction ensuing being produced by no other dye-ware. The lakes—i.e., the compounds of tinctorial acids with basic mordants—are well known to be soluble in the solutions of certain salts of alumina and tin. On treating a pattern of dyed cloth with a hot solution of sulphate of alumina the lakes of cochineal, and all other natural red colours, are stripped; tar colours—such as magenta, corallin, and saffranin—dissolve as such, whilst eosin-red remains almost completely untouched. Eosin yields lakes with the oxides of the heavy metals, which, unlike the lakes of cochineal and the woods, are soluble in water, but insoluble in precipitants, as in this case in the sulphate of alumina.

Les Mondes, Revue Hebdomadaire des Sciences,
No. 4, January 27, 1876.

This issue contains no chemical matter.

No. 5, February 3, 1876.

Among the instances of progress in applied chemistry mention is made of artificial butter!

MEETINGS FOR THE WEEK.

- SATURDAY, Jan. 26th.—Physical, 3. "On some Problems connected with the Flow of Electricity in a Plane," by O. J. Lodge. Exhibition of a Tangent Galvanometer for Projection, by A. Haddon.
- MONDAY, 28th.—Medical, 8.
— London Institution, 5.
— Royal Geographical, 8.30.
- TUESDAY, 29th.—Civil Engineers, 8.
— Royal Institution, 3. "On the Classification of the Vertebrated Animals," by Prof. Garrod.
- WEDNESDAY, March 1st.—Society of Arts, 8. "Aquaria, their Construction and Management," by W. Saville-Kent, F.L.S., F.Z.S.
— Microscopical, 8.
— Pharmaceutical, 8.
- THURSDAY, 2nd.—Royal, 8.30.
— Royal Institution, 3. "On the Chemistry of the Non-Metallic Elements," by Prof. Gladstone.
— Chemical, 8. Discussion on Dr. Frankland's Lecture on "Water Analysis."
— Royal Society Club, 6.30.
— London Institution, 7.
- FRIDAY, 3rd.—Royal Institution, 9. "The Paraffins and their Alcohols," by Prof. Odling.
— Geologist's Association.
— Society of Arts, 8. (Indian Section.) "The Fall in the Price of Silver; its Causes, its Consequences, and their Possible Avoidance, with especial reference to India."
- SATURDAY, 4th.—Royal Institution, 3. "On the Vegetable Kingdom," by W. Thiselton Dyer.

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TO ANALYSTS.

GLAMORGANSHIRE.

The Justices of the County of Glamorgan having at their last Quarter Sessions resolved to appoint Analysts for such County, invite Candidates for the Appointment to send Testimonials and Certificates of Qualification, addressed "Application for Analyst," under cover, to the Clerk of the Peace, Cardiff, on or before the 1st day of March next. The duties of such office are prescribed by the Act of 38 and 39 Vict., cap. 63, and the following Resolutions were adopted by the Court:—

That the Court proceed to the Election of an Analyst or Analysts for the County, at the next Easter Sessions, and that the Rules laid down by the County of Oxford be adopted in this County, and that the Fees to be paid be as follows:—

	£	s.	d.
To the Analyst for each Statutory Report to Quarter Sessions	2	12	6
For every Analysis and Certificate	0	15	0
To be paid to Inspector of Nuisance by every purchaser requiring an Analysis, and seeking it through the medium of the said Inspector	0	6	
To each Analyst upon compulsorily attending as a Witness at the place where he resides, in each case ..	0	10	6
If elsewhere Threepence per mile, going and returning, and	1	1	0

The Clerk of the Peace was directed to advertise in one or more of the London, and the usual County papers, inviting applications from persons desirous of being elected Analyst, requesting them to send him their Testimonials by the 1st of March next.

THO. DALTON,
Clerk of the Peace.

Cardiff, January 24, 1876.

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THE CHEMICAL NEWS.

VOL. XXXII. No. 849.

ORGANISATION AMONG CHEMISTS.

As our readers must be well aware, we have for some time embraced every opportunity of calling the attention of professional chemists to the necessity of an organisation, such as has been adopted by every other profession. So far our remarks—in appearance at least—have met with little response. But the able paper by Dr. C. R. Alder Wright (CHEMICAL NEWS, vol. xxxiii., p. 27) encourages us to persevere.

In support of our view that the sphere of the chemical profession is being encroached upon by outsiders, we will bring forward two unmistakable modern instances. The Board of Trade have recently appointed a Water Examiner. Now a water examiner should be a chemist and a microscopist. Yet the gentleman appointed is an engineer! Turning to a closely connected matter, what can be more evident than that the treatment of sewage is a chemical question? The distinction between sewage and pure water is a chemical distinction. The methods of removing the impurities depend on chemical principles, and chemistry alone can decide whether any sewage or refuse water is sufficiently purified to be safely admissible into rivers. Yet at a meeting of the Society of Engineers, recently held in Westminster Chambers, the President, Mr. Vaughan Prendred, in the course of his address observed:—"The sewage question was the great question of the day, and it would require their (the engineers') deepest thought and utmost skill." There exists, in fact, already a Society of Sanitary—not Chemists—but Engineers. Thus, without prompt and vigorous action, we shall see this important matter taken entirely out of our hands, the main credit and the main emoluments falling to those who merely execute the subsidiary portion of the work.

Dr. Alder Wright very justly comments on the low salaries paid to Public Analysts. Of this we have had lately a signal instance. A certain borough munificently awards its analyst fees which range from half-a-crown to ten shillings. Can our readers name any analytical operation which can be fairly and conscientiously performed for half-a-crown? But this evil of low fees—which leads of necessity to slovenly and hurried work and the use of impure reagents—has been fostered by some men of standing. There are chemists holding public appointments so forgetful of what is due to themselves and to their professional brethren as to undertake analyses and investigations at a far lower rate than an engineer, a physician, or a barrister would require for a similar demand upon his time and attention. We have seen advertisements in which a chemist actually offered to give his services gratuitously to any manufacturing establishment. No such cases occur in better organised professions. We must beg to remind manufacturers who feel tempted by such offers that unpaid services are in the long run the dearest of all. Persons who do not need to take fees or salaries, instead of degrading the profession by doing commercial analyses at starvation prices, might serve science better by devoting their time to original research.

To accuse any professional chemist of incompetence is an exceedingly delicate matter. Yet the fact that "high" and "low" analysts exist, and that they are known and sought after when occasion serves, cannot be overlooked. Some of those who make the loudest outcry when led astray by the certificates of such practitioners shun consulting any conscientious and skilful chemist. We know a case where certain speculators, who had an iron mine to dispose of, sent a sample of the ore to a chemist of un-

deniable skill and sterling integrity, with a nicely-worded intimation that if he made it come out satisfactorily he would receive many samples of ores and minerals. In his report he stated the exact truth, which was not very flattering, and as a matter of course never received another sample. We have sometimes heard the opinion advanced that brokers and merchants always detect and shun the incompetent chemist or assayer. We doubt this statement. We have heard of a chemist—probably now long ago resolved *in terram damnatam*, as old Helmont hath it—who in alkalimetry used to determine the point of saturation by placing a drop of the liquid upon his tongue, and who generally made every sample from 1 to 1.5 per cent better than the reality. Yet the brokers clung to him with a faith perfectly touching. Whilst fully of opinion that such black sheep cannot be excluded from the flock except by means of organisation, we must remember that the need for union would still exist even if every member of the profession were skilful and honourable. To belong to an organised body known to be willing and able, on the one hand, to watch over the interests of its members, and on the other to impose a proper restraint upon them if they go astray, is an incalculable benefit. So long as the profession is unorganised clients know that if one chemist will not accept insufficient fees, or give sweeping certificates in favour of questionable commodities, another will be glad of the opportunity. But the moment all chemists are bound, as they ought to be, by a strict professional etiquette, all this will come to an end. Those who are not disposed to pay fairly for chemical advice will not be able to get it at all.

We may add that we are not actuated by any ill will to that profession which has been most conspicuous for its successful encroachments on the province of chemists. It has before it a wide and almost infinite career where there is full and legitimate scope for the talents and energies of its members. But we likewise have our sphere into which none but chemists have the right to enter. The great principle of the division of labour, still more important in intellectual matters than in mere manual affairs, forbids any man from being at once chemist and engineer.

The following advertisement, which we find in the columns of a morning paper, gives confirmation strong of the low position to which chemistry as a profession has fallen:—

Wanted, a Young Man capable of making a thorough chemical analysis of water, competent to direct workmen, must know London well, and have some knowledge of accounts; salary £2 per week; security required.—Apply, &c.

Were the profession organised, no chemist would undertake to analyse waters for a salary amounting to less than half the wages of a collier; or would, on any terms, consent to do the work of a messenger, a clerk, or a foreman, or to find "security."

Mr. A. H. Allen's letter, inserted in this week's issue, supplies additional confirmation—if such were needed—of the necessity for organisation. So long as there exists no method of excluding unqualified pretenders from our ranks we shall never be regarded as a learned profession, either by the legislature or the public. But we do not feel at liberty to adopt Mr. Allen's proposal to take the Society of Public Analysts for the nucleus of the proposed Guild. He himself supplies an argument against his suggestion in remarking that "many of the Public Analysts under the Sale of Food Act are quite unused to general analysis, and would never desire nor expect to be recognised as members of a guild of qualified analytical chemists." But we scarcely see how Mr. Allen's scheme is to be carried out without extending the membership of the guild to all the members of the Society. It will be remembered that when the Society of Public Analysts was first constituted something of the kind was mooted, but the majority of the Public Analysts present deemed it more prudent to confine their views to the object more immediately in hand.

We should like to know the exact nature and aims of a body calling itself the "Chemical Science Association," the existence of which we first learned from the report of certain proceedings taken against a firm of chemical manure manufacturers for an alleged nuisance. One of the witnesses for the prosecution described himself as a member of the Association in question.

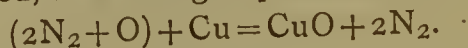
We shall be very happy to receive communications from our readers on the most practical method of effecting what is so urgently required.

LABORATORY NOTES.

By SIDNEY LUPTON, M.A.

1. Preparation of Nitrogen.

NITROGEN in quantity is generally prepared by leading air over copper-turnings ignited in a glass tube. Oxide of copper is formed, and nitrogen passes over alone—



A considerable length of copper must be heated, and the reaction comes to an end when the copper is somewhat thickly coated with oxide.

It was suggested to me by Mr. Vernon Harcourt that if the air was mixed with ammonia by bubbling through a strong aqueous solution, the oxide of copper would be reduced as fast as it was formed, and the reaction would be continuous as long as the solution contained any ammonia. Air and ammonia would give nothing but water and nitrogen—



On trying the experiment I found that about 3 inches of copper-turnings, heated by an ordinary Bunsen burner, served to replace the long furnace and combustion-tube of the ordinary method, and that the slightest excess of air shows itself by tarnishing the surface of the turnings. Indeed, by this method I have several times prepared nitrogen so pure that a stream of it did not alter the surface of fused potassium.

2. Solubility of Naphthalen in Water.

Naphthalen is generally stated—I think on the authority of Garden—to be insoluble in cold water, and but slightly soluble at the boiling-point. It is found, however, that naphthalen, like camphor, moves spontaneously when placed upon the surface of water. According to the theory of Platteau these movements are due to the high surface-tension of a solution of the moving solid. Hence this theory must be laid aside if Garden's statement be absolutely correct.

Naphthalen, purified by re-sublimation through filter-paper, was boiled for some time with distilled water; the solution was allowed to cool to the temperature of the room, and filtered twice through Swedish filtering-paper. A portion of the solution was evaporated to dryness on the water-bath; no residue was left, since naphthalen is very volatile in steam, and this may have occasioned Garden's statement. Another portion of the solution was heated in a glass bulb, and the boiling-point taken by a thermometer immersed in the liquid: it was $103^\circ \text{C}.$, a small piece of clean platinum wire occasioned no change. The boiling-point of freshly-boiled distilled water was then taken under exactly the same conditions; it was found to be $102.4^\circ \text{C}.$, and after throwing in the clean platinum wire $102.1^\circ \text{C}.$

On another occasion the solution was made from ordinary filtered water; the boiling-point, with or without the platinum wire, was found to be $103.2^\circ \text{C}.$, that of the water itself being $101.4^\circ \text{C}.$, or with the platinum wire $101.3^\circ \text{C}.$

From these experiments it seems that sufficient naphthalen remains in solution to alter the boiling-point at least half a degree, and that Garden's statement cannot

be taken as being absolutely correct: hence no doubt is thrown on Platteau's theory. It may be remembered that camphor itself is but very slightly soluble in water.

3. Tests for Aniline.

a. When aniline is boiled with a dilute solution of chloric acid, the colour changes through mauve, magenta, and vermillion to a clear reddish yellow liquid. Naphthylamin, under the same conditions, passes through blue-black to light yellow.

β. When aniline is boiled with excess of potassium ferricyanide, the yellow solution changes to a deep chrome-green. After longer heating a blue-black precipitate falls, apparently similar to that obtained by Letheby by the electrolysis of salts of aniline. Naphthylamin, under the same circumstances, forms a yellow-green solution, with separation of a red resin.

γ. When aniline is heated with a 1 per cent solution of osmic acid a thick, black, flocculent precipitate falls. Naphthylamin, under the same circumstances, forms first a purple solution, and after longer heating a brown precipitate.

4. Tests for Succinic Acid.

a. Nitrate of uranium, when added to a neutral succinate, forms a very sparingly soluble pale yellow precipitate, which is soluble in acetic acid, but insoluble in solution of oil of amber, alcohol, excess of succinate of ammonia, or acetate of soda. Uranium benzoate is almost identical in appearance and properties.

β. When nitrate of cobalt is added to a solution of a neutral succinate, the liquid changes to a peculiar purple or "peach blossom colour," and if the solution be concentrated a precipitate falls. On the addition of ammonia the solution so precipitated becomes more and more blue. This precipitate is soluble in acetate of soda. The presence of oil of amber seems greatly to facilitate the precipitation; as also does alcohol, but in this case the precipitate is pink.

Benzoate of cobalt is red when formed in the cold, green when produced at the boiling-point; it is very soluble.

ESTIMATION OF MANGANESE IN CAST-IRON.

By SERGIUS KERN, St. Petersburg.

THE following method is proposed for the estimation of manganese. The process is easily executed, though very accurate results are not obtained; but, however, in laboratories of iron works this method may be used, especially for the analysis of spiegeleisen.

0.5 grm. of the sample is dissolved in a high glass in 15 c.c. of hydrochloric acid, 1.12 sp. gr. At the end of this operation about 0.2 grm. of potassium chlorate is added in order to convert all the iron into ferric chloride. If silica occurs in the sample it is found in the form of a precipitate which is filtered from the solution. The liquor then contains ferric chloride (Fe_2Cl_6), and manganous chloride (MnCl_2). A solution of caustic potash is next added; $\text{Fe}_2(\text{HO})_6$ and $\text{Mn}(\text{HO})_2$ fall down as precipitates; to the solution is immediately added about 40 to 50 c.c. of a concentrated solution of ammonium chloride (NH_4HCl), and the mixture is boiled for about ten to fifteen minutes. The liquor is then filtered from the brownish-red precipitate of hydrated ferric oxide, and to the colourless solution ammonium sulphide (NH_4SH) is added; a flesh-coloured precipitate of manganese sulphide (MnS) is obtained, which is filtered from the solution, quickly washed, placed in a porcelain crucible, and heated with sulphuric acid. Manganous sulphate (MnSO_4) is then obtained, which, evaporated to dryness and next ignited, yields red mangano-manganic oxide (Mn_3O_4). This oxide is weighed, and as it contains 72.05 per cent of manganese, the percentage of manganese in the sample may be easily calculated.

This process is based on the solubility of freshly precipitated Mn(OH)_2 in a concentrated solution of NH_4HCl , and on the insolubility of the hydrated ferric oxide in the same solution. My method has been used in several analyses of spiegeleisen with success, and thus may be proposed for the use of analysts.

REPORT ON THE DEVELOPMENT OF THE CHEMICAL ARTS DURING THE LAST TEN YEARS.*

By Dr. A. W. HOFMANN.

(Continued from p. 78.)

THE difference in the effect can only be explained by the production of a far lower temperature. Within the entire machine also there is an excessive tension, so that the vapour seeks to escape at the joints, thus debarring the air from entering. The air-pump also is of much smaller dimensions as it draws and compresses a far denser vapour, thus notably reducing the loss of power due to the friction of the piston. If, however, the work is carried on at greater differences of temperature than in the common ether machine, the engine must expend more power, as appears from the theory of the air machine. For equal temperatures of evaporation and condensation the theoretical effect of the two machines is equal. Tellier keeps a sufficient quantity of methylic ether stored in cast-iron vessels capable of bearing a pressure of 10 atmospheres. On opening a cock the gas streams out, the liquid is cooled, and if the vessel is set in water this soon becomes frozen. The ether is thus certainly lost. Occasionally this method may be found useful.

Other substances of low boiling-points may, like the above-named ethers, be applied for producing a fall of temperature, but no different result can be expected from their theoretical action. Thus Van der Weyde, of New York, makes use of chymogen, a constituent of natural petroleum, evaporating between 0° and 16°C. , of which, in the United States, a litre costs only 14 to 24 Pfennige (12 Pfennige = $1\frac{1}{2}$ d. English).† Liénard and Hugon, of Paris, are said to use sulphide of carbon.‡ An original proposal by Mort and Nicolle, which may be regarded as a combination of the above-described system with the following, will be considered below.

Application of Carbonic Acid.—Carbonic acid has been repeatedly proposed as an agent for the production of cold. In 1867, a provisional protection for this principle was taken out in England, but the patent was never completed. *A priori* carbonic acid cannot be regarded as a very suitable means for effecting a fall of temperature. It has, indeed, in comparison with all other materials hitherto proposed (except air), the advantage of cheapness, and in contrast to the ethers that of incombustibility, and therefore of freedom from danger. The pressure of the liquefied acid is, however, enormous, and hence the receivers require to be made very strong, and the connections occasion much difficulty. The temperature and tension of liquid carbonic acid show the following relations:—

Temp.—	-60°	-30°	-15°	-5°	0°	$+10^\circ$	$+15^\circ$	$+30^\circ \text{C.}$
	4.5	16	25	33	38	46	51	73

As the temperature at which the carbonic acid is condensed in the cooler cannot be lower than $+10^\circ \text{C.}$ the tension is then 30 atmospheres, and even at -30°C. a tension of 16 atmospheres would ensue.

At the Vienna Exhibition a peculiar attempt was shown to use carbonic acid as a means both for the production of

power and of cold. The machine was constructed by L. Seyboth, of Vienna, and was contrived as follows:—

The carbonic acid, generated from sulphuric acid and iron-spar, was evolved in a closed receptacle at the pressure of 4 to 6 atmospheres.

(To be continued.)

PAPYRUS EBERS: THE EARLIEST MEDICAL WORK EXTANT.

By H. CARRINGTON BOLTON, Ph.D., of New York.

THE most ancient chemical manuscript extant is believed to be a Greek papyrus of Egyptian origin, preserved in the library of the University of Leyden, and supposed to date from the third century A.D. This manuscript has never been fully described; the little known of it is contained in *Kopp's Beiträge der Chemie*, vol. i., p. 97.

Some months ago, however, the literary and scientific world came into the possession of a work far surpassing in antiquity the Leyden manuscript, and of infinitely greater interest and value to the student of the history of chemistry. This remarkable work is a facsimile of an Egyptian medical treatise, written in the sixteenth century B.C., and consequently more than 3400 years old. Though strictly a medical work, it is of no less interest to the chemist than to the physician.

G. F. Rodwell, F.R.A.S., author of "The Birth of Chemistry," in a recent letter to the *CHEMICAL NEWS*, refers to our knowledge of Egyptian chemistry in the following language:—"When we remember that the science originated in Egypt, and that the very name is derived from an Egyptian source, we can but hope that, in the progress of Egyptian discovery, as valuable information in regard to the history of chemistry as has already been found in regard to astronomy may be brought to light."

This Egyptian papyrus is a first and opportune response to the desire herein expressed. The title reads thus:—

"PAPYROS EBERS, *das Hermetische Buch über die Arzneimitteln der alten Ägypter in Hieratischer Schrift*. Herausgegeben mit Inhaltsangabe und Einleitung versehen von Georg Ebers. Mit Hieroglyphisch Lateinischen Glossar von Ludwig Stern. Mit Unterstützung des Königlich Sächsischen Cultus Ministerium. Wilhelm Engelman. Leipzig, 1875. Zwei Bände, folio."

Translated, the title is as follows:—

"PAPYRUS EBERS, the Hermetic Book of Medicine of the Ancient Egyptians, in Hieratic Writing. Published, with Synopsis of Contents and Introduction, by George Ebers. With a Hieroglyphic-Latin Glossary by Ludwig Stern. Under the patronage of the Royal Bureau of Education in Saxony. Leipzig: William Engelman, 1875, 2 vols. folio."

The papyrus of which this work is a facsimile reproduction was discovered by the archæologist Ebers, during his visit to Egypt, in the winter of 1872-73. Ebers and his friend Stern were residing at Thebes, collecting archæological data, and there became acquainted with a well-to-do Arab from Luxor, who brought to them for sale a modern image of Osiris, and a papyrus of no special value. Suspecting that the Arab was holding in reserve objects of greater interest, Ebers offered him a considerable sum for any remarkable specimens in his possession. This induced the Arab to return on the following day, bringing with him a metallic case containing a papyrus roll enveloped in mummy cloths. Ebers immediately perceived he had a prize, but was unable to command the large sum of money demanded for it, until provided with the means through the liberality of a German gentleman, Max Günther, travelling in that vicinity.

According to the Arab's account, the papyrus had been discovered fourteen years previously by a man since dead.

* "Berichte über die Entwicklung der Chemischen Industrie während des Letzten Jahrzehends."

† *Deutsche Industrie*, 1869, 339.

Private communication.

The original papyrus was discovered between the bones of a mummy in a tomb of the Theban Necropolis.

Ebers hastened back to Leipzig with his precious roll, and deposited it for safe keeping in the University Library of that city. And now, with the co-operation of an enterprising publisher, and the assistance of royal patronage, he places it at the disposal of the civilised world by reproducing it in these handsome volumes.

The papyrus, as received by Ebers, consisted of a single solidly-rolled sheet of yellow-brown papyrus, of finest quality, 0.3 metre wide and 20.23 metres long. It formed one enormous book, but was divided into 110 pages, which were carefully numbered. For purposes of preservation and exhibition in convenient form the roll has since been cut into several lengths.

The writing, which is exceedingly clear and regular, is partly in black and partly in red ink, the latter occurring at the heads of sections and in the expression of weights and measures. The characters are known as Hieratic, being a cursive form of the hieroglyphic method of writing, and bearing the same relation to the latter that our ordinary written hand does to printed characters. Hieratic script resulted from attempts to simplify the forms and outlines of the ideographic characters employed in hieroglyphic writing, which is essentially a combination of picture writing with a phonetic system. Hieroglyphics, in ancient Egypt, was the written language of the people, and Hieratic writing was chiefly confined to the sacerdotal caste.

The papyrus Ebers is so remarkably well preserved that not a single letter is lacking in the entire roll. The material of the papyrus itself, the inner bark of *Cyperus Papyrus*, was examined by Prof. Schenck, Professor of Botany in the University of Leipzig, who established its identity with that of similar rolls, and pronounced it of remarkably good manufacture.

The age of the manuscript was determined by a consideration of three points:—

1. Palæographic investigations of the form of the written characters.
2. Occurrence of names of kings.
3. Examination of a calendar which occurs on the back of the first page.

These data enable Ebers to assign the writing to the middle of the sixteenth century, or, more precisely, 1552 B.C. Accepting this date—and it has been established beyond reasonable doubt—the writing was prior to the exodus of the Israelites; in fact, according to the commonly received chronology, Moses, in 1552 B.C., was just 21 years of age.

The authorship of this ancient work is not revealed, but it bears internal evidence of being one of the six Hermetic books on Medicine named by Clement of Alexandria (200 A.D.).

The Egyptian priests, who were also the physicians, in order to give greater authority to their writings, were wont to ascribe them to their gods, and their codified medical knowledge was generally ascribed to the god Thuti (or Thoth). In proof of this Ebers quotes the following passage from page 1, lines 8 and 9, of the papyrus in question:—"Ra pities the sick; his teacher is Thuti, who gives him speech, who makes this book, and gives the instruction to scholars, and to physicians in their succession."

This god Thuti, also written Thoth and Taaut, is the famous Hermes Trismegistus of the Greeks, the same who was regarded by the alchemists of the Middle Ages with superstitious reverence as the father of alchemy. However this may be, all historians accord in representing Hermes as the inventor of arts and sciences. He first taught the Egyptians writing, invented arithmetic, geometry, astronomy, and music; gave laws to the people, and regulated their religious ceremonies.

At the time of Jamblichus, who lived A.D. 363, the priests of Egypt showed forty-two books, which they attributed to Hermes (Thuti). Of these, according to that

author, thirty-six contained the history of all human knowledge; the last six of which treated of anatomy of disease, of affections of the eye, instruments of surgery, and medicines.

The papyrus Ebers is indisputably one of these ancient Hermetic works; a study of the Synopsis of the Contents, given further on, will justify this belief.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

PHYSICAL SOCIETY.

February 26, 1876.

Professor G. C. FOSTER, F.R.S., President, in the Chair.

THE following candidates were elected Members of the Society:—The Rev. R. Abbay, M.A., and Mr. W. Bottomley, sen.

Mr. A. HADDON exhibited and described a form of tangent galvanometer, so arranged that by the aid of an electric lamp an image of the needle can be projected on the screen, and its deflections thus made evident to large audiences. A horizontal beam of light falling on a mirror inclined at 45° is thrown vertically upwards. In its path it meets with a glass box containing a lozenge-shaped magnet about three-quarters of an inch long: above this needle is a graduated semicircle. The pivot supporting the needle is fixed in the centre of the glass plate which forms the bottom of the box. Above this box is a lens, and on the top of the whole is a second reflector parallel to the first. On either side of the needle is a hoop of stout brass wire, 14 inches in diameter, one end of each hoop being insulated by a piece of ebonite, while the other end is in metallic connection with a brass ring, which slides easily over the circular base of the instrument. The hoops are separated from each other by a distance equal to half the diameter of either hoop, *i.e.*, 7 inches. The instrument having been placed at a distance from the screen equal to the focal length of the lens, and the needle brought to zero by rotating the graduated scale, the hoops are placed parallel to the magnetic meridian, and the instrument is ready for action. As an illustration of the manner in which the galvanometer is employed, Ohm's law was proved in the cases of large and small external resistance.

Mr. O. J. LODGE, B.Sc., then described some investigations on which he has recently been engaged in reference to the flow of electricity in plane bounded surfaces, in continuation of a paper read before the Society in the early part of last year, by Prof. G. C. Foster and himself. After some introductory considerations, he pointed out that all the conditions of the flow of electricity are known for any number of poles in an unlimited sheet. The problem, then, consists in reducing cases of poles in bounded plates to corresponding cases in the unlimited plane, such that the flow conditions on the bounding line may be the same in both cases. The determination of these data, however, for limited planes of certain forms, presents considerable difficulty. In studying questions of this nature, there are two kinds of lines which must be considered. These are "equipotential lines" *along* which no electricity passes, and "lines of flow," *across* which no electricity passes. The boundary of any conducting surface will, of course, always be a line of flow, and, in a bad conductor, we can form an equipotential line by laying a band of copper in the required direction. If, therefore, in studying a surface of limited extent in contact with an electrode we can find a point or points outside the surface such that, if they be made electrodes, the boundary line of the surface becomes a line of flow, we are at liberty to treat the surface as part of an infinite plane, and all the circumstances are therefore

known. To take the simplest case: a straight line in an infinite surface will be a line of flow if equal sources be placed in pairs on opposite sides of the line, so that one is the virtual image of the other; but if the components of each pair are of opposite sign it becomes an equipotential line. To make a circle of radius (r) an equipotential circle, we require a source, A, within, and a sink, B, without, such that $CA \cdot CB = r^2$. To make it a line of flow we require two sources, such that $CA \cdot CA' = r^2$, and an equal sink at C, the centre of the circle. The cases of an infinitely long straight strip, and of a surface bounded by two straight lines inclined at an angle δ , were then referred to, and Mr. Lodge showed that the first requires an infinite number of external sources arranged on a straight line, and the second an infinite number on a circle, except when δ is a sub-multiple of π , the number then becoming finite. Diagrams of the images for certain cases of triangles and squares were also shown. The dimensions of the electrodes in contact with conducting surfaces are not matters of indifference. In a plane bounded by straight lines the electrodes within and without the boundary are of equal size, but when the boundary is a circle the areas of electrodes vary as the squares of their distances from the centre. It was then pointed out that not only the poles may be reflected in this way, but also every point in the sheet; and if the lines of flow or of potential are drawn inside a given circle for any arrangement of poles, the lines outside can be immediately obtained from them by inversion with regard to the centre of the circle by means of a Peaucellier cell. The author then described the manner in which the principle of Wheatstone's bridge can be employed for tracing out lines of equal potential. If A and B be a source and sink on a conducting ring, and P any point on the ring between A and B, and Q any point between B and A, then P and Q are of equal potential whenever—

$$\frac{PA}{PB} = \frac{QA}{QB}.$$

If now the wire under the point P be flattened out into a surface, the above expression holds good for a certain line on that surface, which is therefore an equipotential line. Similarly, by flattening out the wire under the point A, the line for which the expression then holds good is a line of flow for a certain distribution of poles. [At this point the reading of the paper was adjourned to the next meeting of the Society.]

Prof. McLEOD exhibited a glass plate covered with a film of silver, which had in places been deflagrated by means of Leyden jars; the poles being placed at varying distances apart. The form of the surface acted upon tended towards the lemniscate of Bernouilli.

NEWCASTLE-UPON-TYNE CHEMICAL SOCIETY.

General Meeting, December 24th, 1875.

JOHN PATTINSON, President, in the Chair.

THE minutes of the previous meeting were read and confirmed.

The name of Mr. W. F. Henderson, 35, Leazes Terrace, was read for the first time.

Mr. RICHARDSON said he wished to make a correction with respect to what he stated at the last meeting on the subject of working the tanks in continental chemical works. He said that the first liquors which came from the tanks were used for making carbonate of soda, and that the later liquids containing sulphide were used for making caustic soda. He had since referred to his notes, and he found that the cold liquor first drawn off contained caustic soda. Afterwards the liquor contained carbonate of soda with a small quantity of sulphide; then carbonate of soda comparatively pure; and afterwards, when heat was applied, carbonate of soda largely mixed with sulphide.

Mr. J. W. SWAN exhibited one of the balances con-

structed by Herr Bünge, Mechanikus, Hamburg, the detailed description of which would be unintelligible without a drawing. The advantages claimed for the new construction are:—

1. Extreme sensitiveness combined with "quickness."
2. Convenience in working.
3. Non-liability to derangement.

The PRESIDENT said he had found the five-inch beam exhibited to turn with 0.001 gr. when loaded with 100 grs., and with 0.002 when loaded with 1000 grs. in each pan.

Mr. SWAN said it was guaranteed to carry 200 grms. and turn with 0.0001 grm. The balance shown had been carried about Newcastle, in bringing it to the meeting, exactly as it then stood. Mr. Swan also exhibited his own improvement on Scheibler's electric thermostat, and several of Mr. Fletcher's gas furnaces in action.

The thanks of the meeting were offered to him.

NOTICES OF BOOKS.

Report of the Public Analyst to the Town Council of the Borough of Portsmouth for the Year ending September 30, 1875. Portsmouth: H. Lewis.

MR. G. TURNER, Public Analyst for the Borough of Portsmouth, has officially examined one hundred and forty-five cases. Only four persons have been prosecuted, but he considers that there has been a marked improvement in the articles collected this year, especially milk and coffee. This is very satisfactory intelligence.

In the analysis of butter Mr. Turner finds the method of Angell and Hehner satisfactory, though it requires much time. This difficulty he has succeeded in overcoming by the addition of methylated spirit.

On the subject of milk he considers that the average total solids in milk are 12.5, or 1.5 higher than the standard adopted by the Society of Public Analysts. He evidently attaches no value to the results obtained by Dr. Voelcker and so eagerly welcomed by the champions of sophistication. In estimating the fat he prefers benzine to ether as a solvent, and allows the residue to digest over night. Mr. Horsley's process he does not find satisfactory. In one sample of milk, which otherwise came well within the standards, he found 1.617 of mucin and a substance insoluble in cold alcohol, possibly vegetable mucin. This subject, he thinks, would well repay investigation.

A Set of Chemical Labels for the Laboratory, Alphabetically Arranged. By PHILIP HARRIS and Co., Manufacturing Chemists, Bull Ring, Birmingham. January, 1876.

THESE labels are well arranged, boldly printed, and of convenient size for laboratory use. In our opinion, however, the compilers would have been wise to have simply stated facts. Present chemical theories will probably be old in the course of a year or so, and the system of notation adopted by Messrs. Harris, instead of helping the young student, will be a hindrance to him, unless indeed chemists make it a rule to re-label all their tests and reagents whenever a new system of notation becomes fashionable.

Production of Silk in France.—The gross weight of cocoons for the year 1873 was 9,883,580 kilos., valued at 68,614,802 francs. Of this amount 94 per cent was furnished by the eight southern departments of Le Gard, la Drome, l'Ardeche, Vaucluse, Bouches, du Rhone, Isère, and Hérault. In twenty-five other departments the production is insignificant, and in fifty-four others it is unknown. In 1874 the yield did not differ much from that of 1873.—*Les Mondes*.

CORRESPONDENCE.

ORGANISATION AMONG ANALYTICAL CHEMISTS.

To the Editor of the Chemical News.

SIR,—The article by Dr. C. R. A. Wright "On the Necessity of Organisation Among Chemists," ventilates a subject of the utmost importance, and it is therefore to be regretted that the ball set rolling by Dr. Wright should be in danger of stopping, as from the paucity of subsequent letters on the subject seems to be the case. No chemist who observes the signs of the times can doubt the necessity for some such guild as that suggested by Dr. Wright, but I think its advantages may be conveniently discussed in detail.

The formation of a guild of Analytical chemists would tend to reduce the number of unqualified men now calling themselves analytical chemists, and would effectually prevent further encroachments of a similar kind.

As an instance of the annoyance to which professional chemists are now subjected, I may mention that some years ago I heard a druggist say in the witness-box that he was an analytical chemist, though I have good reason to believe he never made an analysis in his life. The extent of his information may be imagined when I say that a youth, since then a pupil of mine, once went to his shop for some "hydrosulphuric acid" for private use, and was instructed by the analytical chemist to "put some water to sulphuric acid, which will make hydrosulphuric acid"!

Another class, generally very ready to quack as chemists, though remarkably tenacious of its own privileges, is that of the pseudo-scientific medical men. One of my acquaintance belonging to this class is very fond of talking of the "quantitative analysis he saw when assistant to Professor Brande, forty years ago," while another recently expressed an opinion that "iodide of potassium could not be accurately estimated in admixture, but he supposed it would be best accomplished by means of starch"! Unfortunately, the ignorance of these pretenders is not understood by the public, and their results and opinions are liable at any time to be quoted as gospel.

I have myself suffered considerably from my liability to jury-service, and have no doubt other chemists have been similarly inconvenienced. Pharmaceutical chemists, medical practitioners, &c., are exempt from jury-service, and I fail to see why analytical chemists should not have similar advantages. Of course no such concession can be hoped for while *anyone* can call himself an analytical chemist; but if the proposed guild becomes an accomplished fact, I think we should have a good chance of exemption.

I see the Attorney-General intends bringing in a jury bill during the current session, so there is no time to be lost if chemists are to benefit by it.

It appears to me that the necessary nucleus of the proposed guild already exists in the Society of Public Analysts; of course, I do not intend to assert that all members of that Society are fit to become members of the guild. The Public Analysts' Society was originally formed to discuss and influence the proposed legislation on Adulteration, and admirably it has answered its purpose. It is well known that the Public Analysts under the Sale of Food Act are many of them quite unused to general analytical work, and would never desire or expect to be recognised as members of a Guild of qualified Analytical Chemists. Still, the Public Analysts' Society, now numbers either among its honorary or its ordinary members, nearly all the consulting analytical chemists in the Kingdom, and if it were to take in hand the work of formation of a Guild of Professional Chemists, those analysts who are not yet enrolled as members would probably give it their influence and support.

Dr. Wright's proposal of a committee of leading analysts to whom all claims for admission as original members of the Guild should be referred, is, I think, an exceedingly good one. There would be no difficulty in finding half-a-dozen chemists whose decision would give general satisfaction, and I hope we may see something of the kind done without more delay.

Unfortunately, we provincial chemists are too isolated to afford much active assistance in such matters, though we are probably the greatest sufferers; but if the metropolitan analysts will only take the matter up, I think they may rely on receiving the hearty support, both moral and pecuniary, of their brother chemists throughout the country.

Now is certainly the time for action, and the proposal of a corporation of analytical chemists is perhaps more likely to receive the support of a Conservative than of a Liberal Ministry; but while there is a Lyon Playfair in Parliament the interests of chemists are sure to be efficiently represented.

ALFRED H. ALLEN.

Sheffield, February 21, 1876.

SPRENGEL'S WATER-VACUUM PUMP.

To the Editor of the Chemical News.

SIR,—Mr. Thomson (*vide* CHEM. NEWS, vol. xxxiii., p. 73) should not feel any doubt as to the advisability of replying to a letter in a public journal simply because it is anonymous. If he can impugn my statements it matters not if I can sign myself the President of the Society. If, on the other hand, my argument is not refutable, it will not be weakened by having attached to it the name of the merest tyro in science.

Whatever idea Mr. Thomson *wished* to convey by the expression "Bunsen's vacuum pump," the effect produced on the mind of the general reader is that Bunsen was the inventor of the water-vacuum pump, which is simply not the fact. In support of this assertion I think I have already adduced sufficient proof. I may, however, add that in nearly every case in which mercury is mentioned in Sprengel's paper he adds "or any other liquid. He also gives the mathematical data for calculating the proportions of the pump when water is employed. He has since told us that he preferred mercury as the most suitable of the two liquids for *exhibiting* the truth which he had *discovered* by means of a water-air pump in 1863.

Mr. Thomson says the history of this matter is well known to every chemist. Herein I must differ from him, for I cannot believe that, if it were so, such misrepresentation would occur. If Mr. Thomson knows more than has been furnished by Sprengel and Bunsen let him tell us what it is. Till he does so I must believe Sprengel and Bunsen's statement (*Philosophical Magazine*, February, 1873).—I am, &c.,

F.C.S.

SODIUM AMALGAM.

To the Editor of the Chemical News.

SIR,—Were one to judge from the writings of practical chemists (and by way of example I may take the wide range afforded by Roscoe's "Primer" to the excellent paper in the CHEMICAL NEWS, vol. xxxiii., pp. 47, 58, by Dr. Davy "On the Detection of Arsenic.") the preparation of sodium amalgam is not an easy, and may be a dangerous process. The proper *modus operandi* is certainly not original with me (I think I saw it many years ago in the CHEMICAL NEWS), but as it would seem to need recalling, and as I have always used it with much comfort, I think it not inopportune to describe it.

Melt the sodium under solid paraffin, then pour in the mercury in a thin stream. Of course any quantity of mercury may be used from equivalent proportion to that necessary for the production of a fluid amalgam. There

is no spirting, and no explosion, no incandescence even, and therefore no need, as Prof. Roscoe directs, to "always take as much as five times by bulk as much mercury as sodium, and less still to "keep the face away" as Dr. Davy advises. In short, if this simple method be adopted, a few grammes of sodium amalgam are as easily made as are no doubt the many kilogrammes manufactured for well known industrial demands. If the quantity of mercury used be such as to produce a solid amalgam, this will solidify before the paraffin, which is to be poured off, and the amalgam cleaned from it with petroleum ether.—I am, &c.,

HARRY NAPIER DRAPER.

WATER ANALYSIS.

To the Editor of the Chemical News.

SIR,—As some of your readers who may have looked at the account of the recent proceedings at the Chemical Society may surmise, the report of what I said in the discussion on water analysis is in various respects inaccurate. I hope, however, shortly to place in proper form before chemists my rejoinder to Dr. Frankland.—I am, &c.,

J. ALFRED WANKLYN.

Laboratory, 117, Charlotte Street, Fitzroy Square,
London, February 29, 1876.

ADULTERATION OF MILK.

To the Editor of the Chemical News.

SIR,—A case was reported in the *Standard* the other day of a man brought up at the Thames Police Court for selling adulterated milk. The Analyst stated that the milk contained:—

"Solids, not fat.. ..	5.38
Solids, fat	2.40
	<hr/>
	7.78

The amount of water added was 22½ per cent."

If called upon to give an opinion in such a case, would not a man be justified in saying the milk contained nearly twice as much added water as stated by the Analyst?—I am, &c.,

London, February 29, 1876.

CHEMIST.

CONCENTRATING SULPHURIC ACID.

To the Editor of the Chemical News.

SIR,—Our attention has been called to a recent notice in (*CHEM. NEWS*, vol. xxxiii., p. 55) of a system of concentrating sulphuric acid known as "Faure and Kessler's Patent," and as our opinions have been frequently requested, we venture to think that the following remarks may be of interest to many of your readers.

The "patent" claims in question rest mainly upon an adaptation of a leaden dome or hood to open platinum pans, also a leaden refrigerator, and certain arrangements in working such apparatus. Full details as to the validity of the patent (for the plans and use of which a payment or royalty is demanded equal to from 100 to 200 per cent upon the value of the platinum) would occupy too much of your valuable space, but we shall be happy to send full particulars to any of your readers who may apply for them.

The chief point of interest to manufacturers is the relative economic advantage or disadvantage of the old as compared with the pseudo new system, and the following information will enable those who have received Messrs. Faure and Kessler's offers and prices to form their own judgment:—

The present cost of platinum apparatus complete, with platinum dome, head, arm, and cooler, as supplied by us is for—

1 boiler capable of concentrating 2 to 2½ tons ..	£675
2 boilers together 4 to 5 tons	1200
3 7 to 8 tons	1750

The whole cost being for platinum charged at its market value.

As compared with Messrs. Faure and Kessler's system, the cost of the construction of the cumbersome leaden dome, and other lead work, and of the water required for cooling it, is saved; also the great danger of working with lead and water in close proximity with "boiling vitriol," "platinum," and "fire," and the inevitable expense of frequent repairs. The platinum is charged at its market value, and the purchaser has thus as capital always a realisable asset nearly equal to the first cost, instead of paying for the platinum, as he practically does under the F. and R. system (considering it to be the only asset of value), from 50s. to 75s. per ounce. The cooler being of platinum, there is no danger of the acid being contaminated with lead. The consumption of fuel is about the same in each case. In the years 1862 and 1863, this system of working with several small boilers in series was much pressed by us upon the acid manufacturers, but decided preference was given by them to the old form of large single still.

We may mention that we are now carrying out experiments the result of which will, we hope, be shortly to place at the command of acid manufacturers a system of concentration, which will relieve them of a great proportion of the outlay at present necessary for such plant.—We are, &c.,

JOHNSON, MATTHEY, & Co.

Hatton Garden, London,
February 29, 1876.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Seances de l'Academie des Sciences. No. 6, February 7, 1876.

Chemical Actions Produced by means of Discharges from an Induction Apparatus.—M. Becquerel.—On operating upon various solutions, the apparatus being worked with two and sometimes with four galvanic elements on the chromic acid principle, the author has obtained the reduction of copper, nickel, cobalt, iron, lead, bismuth, antimony, zinc, cadmium, silver, gold, and platinum. He has also produced amalgams of aluminium, magnesium, and other metals.

Metallic Reductions Produced in Capillary Spaces.—M. Becquerel.—It is remarked that electro-capillary apparatus formed with split tubes act only when the two liquids which penetrate into the slit by capillarity come in mutual contact, which does not take place when the glass is too thick.

Formation of Ethers.—M. Berthelot.—Thermochemical researches on the formation of the hydriodic, hydrobromic, and hydrochloric ethers of amylene; on the formation of the nitric ethers of alcohol, glycerin, mannite, and cellulose; the acid sulphuric ethers of the methylic, ethylic, propylic, isopropylic, butylic, and amylic ethers; as also of glycerin, and on the formation of ordinary ether.

New Chlorated Propylene.—M. Reboul.—The author has obtained a chlorated propylene, CHCl_3 , distinct from that derived from methyl-chloracetol.

Products of the Action of Chloride of Lime upon the Amines.—M. J. Tschermak.—The crude product of the action of chloride of lime upon hydrochlorate of ethylamin must be regarded as a mixture consisting

principally of hypochlorite of ethylamin and of dichlor-ethylamin.

Difficulties Encountered in the Preparation of Pure Aniline.—M. A. Rosenstiehl.—Reserved for insertion in full.

Bulletin de la Société d'Encouragement pour l'Industrie Nationale. No. 25, January, 1876.

Report Presented by M. Debray on Behalf of the Committee for the Chemical Arts on a Process of Amalgamation for Silvered Mirrors.—M. Lenoir.—Up to 1840 mirrors were silvered exclusively by means of an amalgam of tin—a process most destructive to the workmen employed. An important step was effected by an English chemist, Drayton, who conceived the idea of coating mirrors with a thin layer of silver, obtained by reducing an ammoniacal solution of nitrate of silver by means of highly oxidisable essential oils. This process was subsequently modified by several chemists, but only became really practical when M. Petitjean substituted tartaric acid for the reducing agents formerly employed. The glass to be silvered is laid upon a horizontal cast-iron table heated to 40°. The surface is well cleaned, and solutions of silver and of tartaric acid, suitably diluted, are poured upon it. The liquid, in consequence of a well known effect of capillarity, does not flow over the edges, forming a layer of some m.m. in thickness. In twenty minutes the silver begins to be deposited on the glass, and in an hour and a quarter the process is complete. The liquid is poured off, the glass washed with distilled water, dried, and covered with a varnish to preserve the silver from friction. The advantages are evident. Mercury with its sanitary evils is suppressed; there is a gain in point of cost, as 4 to 5 grms. of silver, costing about 1 franc, suffice for 1 square metre, which, under the old system, would require 700 grms. of tin and the same weight of mercury. A few hours suffice to finish a glass on the new system, whilst the old process required twelve days as a minimum. On the other hand, the glasses thus silvered have a more yellowish tint; portions of the pellicle of silver sometimes become detached, especially if exposed to the direct action of the sun, and despite the protecting varnish the silver is sometimes blackened by sulphuretted hydrogen. M. Lenoir has happily succeeded in overcoming these defects by a process alike simple and free from objections on sanitary grounds. The glass, silvered as above, is washed, and then sprinkled with a dilute solution of the double cyanide of mercury and potassium. The silver displaces a part of the mercury and enters into solution, whilst the rest of the silver forms an amalgam whiter and much more adhesive to glass than pure silver. The transformation is instantaneous. The amount of mercury fixed does not exceed 5 to 6 per cent. The glass thus prepared is free from the yellowish tint of pure silver. It is also less attacked by sulphur vapours and the rays of the sun, in which last respect it is superior to mirrors silvered by the old process.

Bulletin de la Société Chimique de Paris,
No. 3, February 5, 1876.

Reclamation with Reference to a Paper on the Formation of Kermes.—M. Terreil.—A reply to a paper by M. Weppen impugning some of the author's results. Terreil maintains that Weppen has not succeeded in preparing pure carbonate of potash, or baryta and strontia, perfectly free from soda.

Metallurgy of Silver in the Moist Way.—A. Guyard (Hugo Tamm).—There exists in the territories of Utah, and around the Great Salt Lake, enormous deposits of spongy silica, evidently deposited from hot water. These silicas are coloured very variously by oxide of lead, oxide of copper, oxides of iron, hydrated and anhydrous, and they present very different aspects accordingly. All, or

nearly all, are impregnated with oxide of lead and chloride of silver. Their proportion of silver, which varies from 1 to 10 kilos. per ton, is found to average from 2.3 to 2.5 kilos. on samples taken from large lots. The great quantity of silica which these ores contain, rarely less than 90 per cent, renders them very difficult to treat in the dry way, whence these ores are the despair of the smelters. On the other hand, the argentiferous ores of Utah are scarcely rich enough to bear the expense of carriage to the great industrial centres, and must be utilised upon the spot, where salt, pyrites, manganese, oxides of iron, charcoal, galena, and wood are plentiful. The real difficulty lies in the fact that the chloride of silver of these ores is scarcely soluble in alkaline chlorides and hyposulphites, whilst roasting the ore, either at high or low temperatures, renders it still less soluble. The desired object was effected by means of nascent chlorine. If the following mixture is heated to a boil until all the chlorine has escaped the liquor is decanted, and the residue washed once or twice with hot water (or, better still, in urine), almost all the chloride of silver will be found in the solution, whence it is extracted by precipitation with metallic iron:—

Utah ore, ground	1 part.
Oxide of manganese	1 „
Common salt	3½ parts.
Strong muriatic acid	7 „

Along with the silver, lead, a little copper, and other minerals are deposited, forming a metallic paste, from which fine silver is extracted by smelting and cupellation. Ores containing 2.5 and 2.3 kilos. per ton yield, in round numbers, 2.4 and 2.2 kilos. after cupellation, showing that practically all the silver is extracted. This loss appears nearly constant whatever is the original percentage of silver, so that there is an advantage in selecting the richest ores. The following method of operating is recommended:—The ore and the salt are first mixed, half the muriatic acid is added, and the mass is boiled. In this manner all the oxide of lead is dissolved, the masses are disintegrated, and the chloride of silver is laid bare. The second half of the muriatic acid is then added, and the manganese is introduced gradually (about one-tenth at a time), permitting all the chlorine to escape before adding a fresh dose. On a large scale the proportions above given will suffice for 2, and even 3 parts of the ore. The author recommends the introduction of the sulphuric acid manufacture, as the residual burnt pyrites always contain gold. Chlorine will also be useful in treating the auriferous antimony found in the regions between Utah and California. The vessels for the treatment of the silver ore should be of wood, slightly charred in the inside, the contents being boiled by a jet of steam.

Analysis of the Residues from the Sodium Manufacture.—A. Guyard (Hugo Tamm).—The mixture for preparing the sodium was—

Crude soda-ash	56.5
Coal	18.5
Coke	10.5
Chalk	14.5
	100.0

This mixture yields as pure a metal as that obtained from pure dry soda. The residue, after exposure for some time to the air, was found to contain—

Soluble matters	35.0
Water	9.0
Carbon	22.0
Carbonate of lime	18.0
Oxysulphide of calcium	
Alumina	15.4
Ash from the coke and oxide of iron from the retort	

The soluble matter was found to consist of—

Caustic soda.. ..	11'90
Carbonate of soda	44'30
Sulphate of soda.. ..	24'10
Sulphite of soda	11'70
Sulphide of sodium	0'45
Chloride of sodium	7'05
Silica	0'20
Alumina.. ..	0'30
Lime and potash.. ..	traces
	100'00

No hyposulphite of soda was present.

Analysis of Spent Lime from Gas Purifiers.—A. Guyard.—The material in question was obtained from a gas works in London. Column I. gives the author's analysis, and column II. one executed by the late Prof. Graham:—

	I.	II.
Hydrate of lime	15'10	17'72
Carbonate of lime	34'20	14'48
Sulphate of lime.. ..	0'25	2'80
Sulphite of lime	1'50	14'57
Sulphide of calcium	6'90	—
Oxysulphide of calcium	3'20	—
Hyposulphite of lime.. ..	11'80	12'30
Cyanide of calcium	0'25	—
Sulphide of iron	0'55	—
Free sulphur	4'30	5'14
Silica	1'80	0'71
Alumina	0'70	—
Magnesia	traces	—
Coal-tar and oil	0'25	—
Water	19'20	32'28
	100'00	100'00

From these results it would follow that Graham's sample had been exposed to the air for some time, whilst that of the author was submitted to analysis immediately on its withdrawal from the purifier.

New Method of Producing Sulphurised Ureas.—M. P. Miquel.—Not suitable for abstraction.

Dynamite made up with Nitro-glycerin and Nitrate of Methyl.—M. C. Girard.—A proposal to add 10 per cent of nitrate of methyl to nitro-glycerin before incorporating it with the silica.

Applications of Bromine in Hydro-metallurgy, Assaying, and Technological Chemistry.—R. Wagner.—Of all the metals mercury is the one whose extraction involves the most serious losses. Thus, according to official returns, the losses at Idria, in reverberatory furnaces, reach 48 per cent, and in the best muffle furnaces they amount to 10. In consequence it has been repeatedly proposed to have recourse to the moist way, but the methods suggested have not been found adapted to practical conditions. The author digests the ores of Idria and of Deux Ponts with bromine-water, containing 3 per cent of bromine, or with the solution of bromine in hydrochloric acid, containing 13 per cent. After digesting the cinnabar with an excess of the bromine liquid for a few days the whole is dissolved, as is also any metallic mercury present. From the solution the mercury is thrown down by means of sulphuretted hydrogen, and the sulphide of mercury is dried and decomposed. The amount of bromine required is very considerable—for 1 part of mercury obtained 3½ parts of hydrobromic acid remaining in solution. The process is therefore only applicable where the manufacture of bromine compounds can be combined with the extraction of mercury. The attack of cinnabar by bromine is an excellent method of analysing this mineral.

Refining Gold.—Gold is readily dissolved by bromine, and the bromide of gold is resolved by heat into metallic gold and free bromine. It is therefore a valuable agent for freeing gold from foreign metals (lead, bismuth, antimony, and tellurium), which alter its properties: all that

is necessary is to add to the gold a certain quantity of bromide of gold. The author thinks that it may advantageously replace chlorine in the process of refining devised by Miller, and in Plattner's process for extracting gold, especially applicable to the residues from roasting auriferous pyrites.

Extraction of Platinum.—The action of aqua regia upon platinum ores being very slow, a mixture of bromine or hydrobromic acid with nitric acid is recommended as preferable. A simple solution of bromine has no action upon metallic platinum. Bromine completely converts ferrocyanide into ferricyanide without, like chlorine, giving rise to secondary products. It also transforms manganate of potash into permanganate, and arsenious acid into arsenic.

Presence of Bromoform in Commercial Bromine.—M. S. Reymann.—The author finds bromoform present in certain samples to the extent of 10 per cent.

Les Mondes, Revue Hebdomadaire des Sciences,
No. 6, February 10, 1876.

Nickel Mines of New Caledonia.—A controversy having arisen in Australia on the priority of discovery of the nickel deposits of New Caledonia, the Rev. W. B. Clarke, geologist to the Colony of New South Wales, gave a history of the discovery before the Royal Society of Sydney. He showed plainly that the nickel was first discovered by M. Jules Garnier in his exploring expedition undertaken under the auspices of the French Colonial Office. Mr. Clarke has had in his collection, since 1864, specimens of nickel ore sent him by M. Garnier, who, on his return to France, made known the abundant existence of nickel in the island. Mr. Clarke transmitted an account of the discovery to the celebrated mineralogist Dana, who described this ore of nickel as a new mineral species, in the most recent edition of his well-known work. Prof. Liversidge, of the University of Sydney, also described the new substance in a learned memoir. Clarke, Dana, and Liversidge gave the name of Garnierite to this new ore, in honour of its discoverer. The great rise in the price of nickel has latterly drawn the attention of manufacturers to these deposits. The serpentines, and generally speaking all the rocks which accompany them, are often covered with a fine green coating—a silicate of alumina, nickel, and magnesia. The price of the metal is now 40 francs per kilo., and the demand is still increasing. Hitherto it has been extracted from speiss, in which it occurs combined with sulphur, arsenic, antimony, cobalt, &c. With the ore of New Caledonia the extraction of the metal will be simpler and the product less impure, the nickel being here combined merely with earthy matters, as shown in the subjoined analyses:—

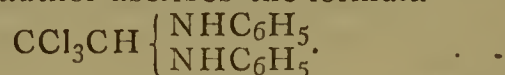
Gangue	39'40	3'00
Silica.. ..	28'60	41'00
Alumina and ferric oxide	0'60	0'60
Oxide of nickel	12'60	19'00
Magnesia	11'40	16'30
Lime	traces	—
Water	7'50	20'00
	99'90	99'90

This new ore, though of a characteristic green, may nevertheless be confounded with carbonate of copper; and perhaps the miner, deceived by this resemblance, may have already met with Garnierite in other countries, and passed it over as a poor ore of copper not worth closer examination. The mines of New Caledonia have already sent to France a ship charged with 500 tons of this mineral.

Gazzetta Chimica Italiana.
Anno v., 1875, Fasc. ix., x.

Essence of Cubebs.—A. Ogliastro.—This paper is not suitable for abstraction.

Action of Chloral, Anhydrous and Hydrated, upon Aniline.—D. Amato.—The result of the reaction is a new base, to which the author ascribes the formula—



Poisonous Nature of the Extract of Dead Bodies (Human).—Prof. A. Moriggia and A. Battistini.—Extracts of dead bodies made with different solvents were given to frogs, either internally or by subcutaneous injection. The effects of the solvent alone were found by a separate series of experiments.

PATENTS.

ABRIDGMENTS OF PROVISIONAL AND COMPLETE SPECIFICATIONS.

An improved press for the extraction of the liquid constituents of fruits and other vegetable matters. E. P. H. Vaughan, F.C.S. Chancery Lane, Middlesex. (A communication from A. Blot, Paris.) January 4, 1875.—No. 37. This press is mainly composed of three Archimedean screws of varying pitch working together, and so arranged in an outer skeleton casing that the material to be treated, fed at one end through a hopper, is subjected to progressively increasing pressure until it reaches the upper end of the apparatus, where the solid residues are expelled, while the liquid constituents, escaping through the perforated casings, are collected by a conduit, which carries them to a reservoir of suitable capacity and form.

A new or improved preparation or dressing for stiffening or finishing textile fabrics and materials, and which is also applicable to other useful purposes. J. Scott, King William Street, London. (A communication from H. Gerard, Paris.) January 4, 1875.—No. 38. This invention relates more particularly to the dressing or finishing of textile fabrics and threads or yarns, and consists in the use of fecula starch, the pulp of tuberous and other roots, and farinaceous and amylaceous substances generally treated with caustic alkalies or alkaline salts, for the preparation of a stiffening material or agent for that purpose, and which it is proposed to designate "apparature." This material or agent so prepared is, however, also applicable as a substitute for gum, glue, or other adhesive material, as a thickening agent in printing fabrics, for the prevention of incrustation in steam boilers, and to a variety of other useful purposes.

Improvements in the construction and working of furnaces for the production of iron, steel, and ferro-manganese, and in processes connected therewith. C. W. Siemens, Queen Anne's Gate, Westminster, Middlesex. January 5, 1875.—No. 43. This invention relates to lining rotary and other reducing furnaces with blocks of wood, rendered incombustible in some cases by impregnation with metallic salts, and set in refractory mortar; to constructing midfeathers of furnaces with vertical air-flues to keep them cool; to arranging regenerative gas furnaces with pockets between the regenerators for collecting dust carried over by the products of combustion; to mounting steel ingot moulds on a skeleton turntable for facilitating the tapping of the metal; to producing ferro-manganese in a rotative wood-lined furnace; to reducing iron ore in the same; and to the preliminary calcining of the ore with the application of the salt and water vapour for aiding the liberation of phosphorus and sulphur.

Improvements in the manufacture of gas for lighting and heating purposes, and in the apparatus connected therewith. J. Kidd, Martin's Lane, London. (Partly a communication from L. Arnold, New York.) January 7, 1875.—No. 66. The novelty of my invention consists, first, in the manufacture of lighting or heating gases in carbon, then carburetting this gas with hydrocarbon vapour, after which I pass the carburetted gas through a red-hot retort; by this means I make the mixture a fixed incondensable gas of high illuminating power. The second part of my invention consists in a novel arrangement and method of setting the retorts. I connect together two or three horizontal gas retorts, one above another, with only one stand-pipe or gas-outlet leading to the hydraulic main from the combined retorts; when using three retorts the gas from the lower retort must pass over the coal placed in the two upper retorts, or the gas from the upper retort before escaping may be forced downwards over the coal in the lower retorts. All the retorts should be charged with coal and operated in the ordinary way.

Improvements in the preparation of dried yeast, and in purifying and removing the bitter taste from the yeast of porter, ale, and beer, and in apparatus to be used for this purpose. T. Ellis, Fulham Road, Hammersmith, Middlesex. January 8, 1875.—No. 69. This Provisional Specification describes thoroughly drying, &c., yeast, so that it will keep a long time, and in apparatus to be used for this purpose.

Improvements in purifying liquids, in obtaining or preparing the purifying agents, in recovering, treating, and utilising matters contained in such liquids, as well as other waste or by-products of manufacturers, and in means or apparatus employed therefor. G. Mackay, Edinburgh, Midlothian, North Britain. January 11, 1875.—No. 91. The features of novelty which constitute this invention are purifying liquids by a solution of per-salts of iron (with or without the addition of sub-salts) and alkali or alkaline earth, utilising the impurities precipitated from the liquids, as well as other waste or by-products, and obtaining a per-salt of iron.

Improvements in means and apparatus for producing or manufacturing iron, steel, and other metals. W. A. Lyttle, Hammersmith, Middlesex. January 12, 1874.—No. 105. The features of novelty are

as follows:—First. The applying to copper, zinc, tin, antimony, and other metals the principle followed in manufacturing iron from crushed or powdered ore intermixed with powdered fuel and fluxes, such intermixed ingredients being consolidated into lumps in accordance with my various Letters Patent of last year for improvements in the smelting of iron. Second. Applying to the above-mentioned and other metals the use of the same principle of dealing with powdered and consolidated ingredients without fusion or smelting, in accordance with the principle laid down in the Specification of my Letters Patent, numbered 2984 of 1874. Third. The use of a peculiar aerating apparatus as the means of refining the various metals smelted by this process, and also of converting the reduced or smelted iron made by this process into malleable merchant iron or steel. Fourth. The use of powdered iron produced by this process as the means of reducing or smelting the ores, oxides, sulphides, or chlorides of various other metals, with or without the admixture of carbonaceous matter.

An improved disinfecting fluid. B. F. Weatherdon, Chancery Lane, Middlesex. (A communication from W. Vigné, ainé, Agen, Department Lot et Garonne, France.) January 12, 1875.—No. 107. This invention consists of a disinfecting liquid formed of sulphate of zinc, acetate of copper, phenic acid, and crystals of baryta.

Improvements in cement. J. C. Sellars, Birkenhead, Chester. January 12, 1875.—No. 112. The kind or class of cement to which this invention relates is that employed for repairing, coating, lining, and setting gas retorts, and other vessels and structures subjected to heat, and is now well known as Sellars' plastic cement. A good quality is produced by mixing siliceous sand and silicate of soda or potash. An excellent quality is produced by mixing siliceous sand, sulphate of baryta, and silicate of soda or potash.

Improvements in aerated liquors. R. R. Holmes, Windsor, Berks. January 13, 1875.—No. 122. This invention relates to a mixture of aerated liquors with spirits in the same bottle, so that one decanting serves to give the mixed drink of the required proportion and quality.

Improvements in preparing certain materials employed in printing and dyeing cotton fabrics and yarns. F. A. Gatty, Accrington, Lancaster. January 13, 1875.—No. 124. My improvements consist in preparing oxidised fatty acids by treating soap or other alkaline compounds of fatty acids with chloride of lime.

MEETINGS FOR THE WEEK.

MONDAY, 6th.—Medical, 8.

— London Institution, 5.

— Royal Institution, 2. General Monthly Meeting.

— Society of Arts, 8. Cantor Lectures. "Wool Dyeing," by George Jarman.

TUESDAY, 7th.—Civil Engineers, 8.

— Zoological, 8.30.

— Royal Institution, 3. "On the Classification of the Vertebrated Animals," by Prof. Garrod.

WEDNESDAY, 8th.—Society of Arts, 8. "Hall-Marking of Jewellery," by Alfred Lutschaunig.

— Geological, 8.

THURSDAY, 9th.—Royal, 8.30.

— Royal Institution, 3. "On the Chemistry of the Non-Metallic Elements," by Prof. Gladstone.

— London Institution, 7.

FRIDAY, 10th.—Royal Institution, 9. "Extinct Animals of North America," by Prof. W. H. Flower.

— Society of Arts, 8. "The Manufacture of Citric and Tartaric Acids," by Robert Warington, F.C.S.

— Astronomical, 8.

— Quekett Microscopical Club, 8.

SATURDAY, 4th.—Royal Institution, 3. "Human Senses," by Prof. Robertson.

— Physical, 3.

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CONTENTS OF No. III.—MARCH, 1876.

Vanadium in Dyeing and Calico Printing—On the Dip-blue Styles of Calico Prints—Kolb on Linen Bleaching—Upon the Former and Present State of Engraving for Industrial Purposes in the Neighbourhood of Rouen—Wool and Silk Dyeing at the Gobelins in 1847—Upon the Action of Acids in Dyeing with the Colouring Matters of Madder, or its Artificial Substitutes—Critical and Historical Notes concerning the Production of Adrianople or Turkey Red, and the Theory of this Colour—Proceedings of Societies—British and Foreign Patents, from the Commissioners of Patents Journal, January 21st to February 13th, 1876—Supplement: The Practice and Principles of Calico Printing, Bleaching, Dyeing, &c., by Charles O'Neill. Bleaching—Printing (continued).

Manchester: PALMER & HOWE, Publishers, 1, 3, & 5, Bond St.
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THE CHEMICAL NEWS.

VOL. XXXII. No. 850.

ON THE TREATMENT OF FILTERED ANTHRACEN OIL.

By A. McDONALD GRAHAM, F.C.S.

THE treatment of the oil filtered from the deposited anthracen presents some difficulties, principally from the fact that the anthracen yielded on a second operation is generally so impure as to be unsaleable.

Many small manufacturers prefer to sell the oil without treatment rather than be at the trouble and expense of separating the anthracen, and by others it is frequently allowed to accumulate to an inconvenient degree. I have at times observed large quantities of this oil, in the yards of some manufacturers, placed in casks, waiting either for a favourable sale or a convenient time for operating upon it.

Without pretending to have entirely solved the problem of its successful treatment, in a commercial and economic point of view, I may yet be allowed to throw out some suggestions, which may perhaps lead to a response from some one of your numerous readers, and thus be the means of affording additional light on a subject of some importance to manufacturers.

At present I believe there are two methods of extracting the anthracen from the filtered oil employed by tar distillers. One of these methods consists in subjecting the oil to fractional distillation, retaining only that portion of the distillate coming over between 300° and 360° C. Some manufacturers, however, prefer to re-distil the oil in a cast-iron retort, rejecting the first portions, and continuing the operation until the residue is coked.

As to the first of these methods, viz., purification by fractional distillation, anyone who has made the trial will, I think, agree with me that it is a work of some difficulty and expense, and not to be attempted if an easier method can be found.

The second mode of operating on the oil, viz., distilling to a coke, has the merit of extracting all the anthracen, and was, I believe, in general use by tar distillers when the anthracen was sold by the petroleum and bisulphide test. The quantity of real anthracen contained in the distillate of course varies according to the nature of the oil operated on; but it is usually very small, amounting on an average to about 12 per cent.

The method which I have found to give good results, and which would I think at once suggest itself to anyone who had had any experience in such matters, is to condense the oil, and allow the residue to cool, and the anthracen to crystallise out as at first. In order to do this I place, say, 1500 gallons of the filtered oil in a wrought-iron still, and distil until crystals of anthracen begin to appear in the distillate on cooling: the distillation is then stopped, and after the temperature of the remainder has become sufficiently reduced I run it out into a tank, and allow the liquid to cool, when the anthracen crystallises out in large quantity. A second and a third condensation can be made if necessary; but I have usually found that the oil was sufficiently exhausted in one operation.

The solid portion deposited in the tank will now be found to contain at least 17 per cent of real anthracen, and will be much easier to treat either by fractional distillation or washing, being comparatively free from hydrocarbons coming over at a higher temperature than anthracen. I have found no difficulty in obtaining 36 per cent anthracen by this method, and others by care may arrive at better results.

Should washing be resorted to, it must not be overlooked that the crystals of all the substances dissolved are deposited according to their solubility in the dissolving medium, and by acting upon a knowledge of this fact the best results may be obtained.

Turnchapel, Plymouth, March 4, 1876.

ON THE DIFFICULTIES MET WITH IN THE PREPARATION OF PURE ANILINE.

By M. A. ROSENSTIEHL.

THE separation of bodies of similar properties often presents considerable difficulties; I have experienced it, in these latter years, in trying to prepare aniline free from pseudo-toluydin. At the time when I discovered this last alkaloid I proved its presence in all anilines, of whatever origin they may be, and especially in that of indigo, which passed then for one of the purest. I indicated also the means of preparing aniline, not giving, with chloride of lime, ether, and acidulated water, the characteristic reaction of pseudo-toluydin. I have since succeeded in increasing the sensibility of this method of testing, and, with its aid, I have still been able to detect the presence of this alkaloid in the same aniline, which then appeared to me pure. I have sought for a proper procedure to remove this little quantity of pseudo-toluydin, so as to obtain a product no longer giving a reaction with my new method. The unexpected difficulties met with form the subject of this note.

To test an aniline I prepare a watery and limpid solution. Dissolving 3.2 grs. in 100 grms. of water, at 17° C., to 10 c.c. of this solution, I add 10 c.c. of chloride of soda (obtained by the double decomposition of liquid chloride of lime of commerce, at 7° Baumé, and a cold saturated solution of carbonate of soda). The proportion of chloride of soda may vary from single to double without inconvenience. The fugitive colouration of Runge immediately manifests itself: I agitate with 10 c.c. of ether, which I preserve; I reject the watery liquid. The ethereal solution is washed with a little water; the washing waters are in their turn agitated with a little ether, which is added to the principal portion; this is then shaken with a little acidulated water. If we had pure pseudo-toluydin or aniline, say prepared from indigo, the acidulated water would take subsequently the violet-red colouration of pseudo-toluydin; but with purified aniline the characteristic reaction is no longer observed: we may say that the colouring matter formed is so weak that it is masked by brown matters and the greenish blue precipitate which the aniline produces.

This acidulated watery solution is now the first matter containing signs of the presence of pseudo-toluydin. It is then shaken several times with small portions of ether, which carry off the brown matters; they are rejected. The watery portion is rendered alkaline by some drops of caustic potash, and agitated with ether, which seizes the colouring matter; the watery portion is rejected, and the ethereal liquid is added to it with a little acidulated water; we agitate it, and let it settle. In the first moments the colouration is not perceived: a slight greenish blue precipitate in suspension hides entirely the violet-rose solution of pseudo-toluydin, but after it has settled some hours this colouration will appear in all its beauty.

The pure aniline which was left from my former experiments was converted into oxalate, and the salt crystallised four times in water, then dissolved in alcohol, from which it was precipitated by ether, in which oxalate of pseudo-toluydin is soluble. This operation was repeated twice, but without success. I have not obtained aniline which did not give the rosy colouration.

Benzine from benzoic acid yielded an aniline with which I obtained very strongly the reaction from pseudo-toluydin. Fifty grms. of anthranilic acid, well crystallised, were dissolved at 150° to 160° in vacuum; the colourless aniline

was distilled into water; the yield was 60 per cent—calculation requires 60.5 per cent. The splitting up is very definite, and, notwithstanding this, the aniline gave distinctly the reactions of pseudo-toluydin.

Twenty kilos. of crystallised benzine, melting at $+4.2^{\circ}$, was melted, congealed, pressed ten times in succession, during the cold weather of the winter 1872-3. The point of melting rose little by little to $+5.5^{\circ}$ C., at which point it remained stationary. There finally remained 5 kilos. of benzine, which was submitted anew to fractional crystallisation: the mother-liquor and the crystals present the same melting-point. I transformed this benzine into aniline; the oxalate of this base was re-crystallised three times in alcohol, then decomposed by caustic soda. This aniline having still given strongly the reaction of pseudo-toluydin, I made with it several trials to remove the latter. I utilised, in the first place, the solubility of pseudo-toluydin in water, of which 100 parts at 17° dissolve 1.3 parts. By six successive washings the half of the aniline was taken up by the water; what remained gave a reaction, of which the intensity was only half that of the original aniline: it was not possible to pass this limit by new washings. Fractionated saturation with sulphuric acid, followed by distillation in a vacuum, gave no better result.

We know that aniline exposed to air grows brown; if it is then exactly saturated with an acid, there arrives a moment when all the mass is coloured red: this colouration is due to pseudo-rosanilin, which is formed in the cold by slow oxidation of the alkaloids. I have finally used this reaction to get rid of the pseudo-toluydin. After preliminary trials I was obliged to renounce the employment of warm air traversing boiling aniline. I finally stopped at this method of working:—We pour aniline in a flask filled with blotting-paper, so as to wet all the paper, and to offer to the air a great surface of action. This flask, stoppered up, is exposed to the air and to the sun during three months: at the end of this time, during which it was frequently opened and agitated, it is fitted with a connecting tube, placed in an oil-bath, a vacuum is made in it, and the aniline withdrawn by distillation. The paper, after this operation, is sprinkled with acetic acid, and colours quickly to an intense rose (rosanilin). The alkaloid thus treated still gives the reaction of pseudo-toluydin, but very faintly.

To sum up, this investigation has taught me that the repeated crystallisation, in water, alcohol, and ether, of salts of aniline or of matters which serve for its preparation, such as anthranilic acid and benzine, is an insufficient method for separating its homologue. There is only the chemical action of the air which has led to an approximately satisfactory result: from the difficulty of removing pseudo-toluydin we may conclude what it would be to remove toluydin, although its presence has not been shown for want of a sensitive reaction.

All the methods of separation that I have employed are less perfect than the method of analysis. By the substitution of this method for the old one I have discovered pseudo-toluydin where formerly I had inferred its absence, which shows at once that the purity of bodies prepared with the greatest care is only relative. Absolute purity is a limit, driven back without cessation by the perfection of our methods of analysis.—*Comptes Rendus*.

The Fellows of the Chemical Society at the Woolwich Arsenal.—It is scarcely necessary to remind the Fellows of the Chemical Society of the President's invitation to visit the Royal Arsenal on the 14th inst. Under any circumstances an inspection of the various departments of the Arsenal is a treat of no mean order, but the advantage as well as pleasure to be derived from an inspection of forging operations on the largest possible scale, and experiments on detonation, &c., under the guidance of Professor Abel himself can hardly be overestimated. Perhaps the greatest novelty to the visitors will be the firing of the 80-ton gun.

NOTES ON ANIMAL CHARCOAL.

FREE LIME AND ORGANIC MATTER.

By ROBERT FRAZER SMITH.

MANY chemists are familiar with the phenomenon of "free lime" in animal charcoal. On calculating out the results of analysis, and portioning out to the acids their due equivalents of bases, there remains in all cases a residue of lime, which has provisionally to be marked free. Of course really caustic lime is never, or at least rarely, present in char, and in very minute quantity. Formerly, and for that matter probably is so still, it was the custom to mark the "phosphates" by difference, in reporting an analysis of char for refinery purposes, as the estimation of phosphoric acid, either in his new char or working stock, is to the sugar-boiler a matter of no moment. Of course, when selling the spent, the manure merchant takes care to inform him of the existence of phosphoric acid; but, as a rule, "phosphates" in sugar-boiling establishments include tricalcic phosphate, free lime, and loss.

On the Continent a very curious result is noticed in the treatment of the charcoal with hydrochloric acid of 2 per cent for the removal of excess of lime:—

1. The weak acid does not remove from the char as much calcic carbonate as is equivalent to the HCl used.
2. In the washings calcic chloride is found equivalent to the original HCl.
3. No phosphoric acid is dissolved out by the hydrochloric acid.

Wagner made an analysis of the dried substance (100° to 120°) obtained from the acid waters of the "fermentation" treatment of char for lime-excess removal, this so-called "fermentation" being simply digestion of the char for six or seven days in 1 per cent HCl. No particulars are given of the amount of sugar left behind previous to the treatment.

Lime	32.60
Chlorine	32.00
Magnesia	2.66
Alkalies	1.10
Organic insoluble	0.42
Inorganic	0.56
Sulphuric acid	traces
Phosphoric	absent.

If we calculate the chlorine out to crystallised calcic chloride there remains an excess of 7.36 per cent of lime, which, according to Wagner, is, as well as the magnesia, combined with acids of the acetic series.

One would expect, judging from theory, that, in presence of excess of calcic carbonate, any phosphate dissolved on the first attack would be re-precipitated as soon as the acid was neutralised; and in practice it is found to be the case.

The following are analyses I have made, among many others, for the purpose of ascertaining if the "free lime" was a constant constituent of all charcoals. The samples were dried at 100° , and the results calculated to dryness:—

	Dried at 200° .			
	I.	II.	III.	IV.
Carbon and organic matter	11.51	7.60	9.57	18.33
Phosphoric acid	35.85	36.87	35.76	34.30
Sulphuric acid	0.09	0.27	0.37	0.33
Carbonic acid	3.54	1.61	0.70	0.40
Sulphur	0.04	0.18	0.21	0.10
Sodium chloride	0.47	0.17	0.16	0.31
Iron	0.18	0.69	0.71	0.41
Magnesia	0.28	0.19	0.08	0.04
Lime	47.34	48.82	45.32	43.62
Insoluble and sand	0.50	3.03	6.25	1.60
	99.80	99.43	99.13	99.44

I. A new char. III. Impalpable dust, from dust-room of char-elevators. II. and IV. Old chars; IV. from a refinery where carbon goes up; II. from one where carbon as steadily decreases.

We may state these results as follows:—

	I.	II.	III.	IV.
Carbon and organic matter	11.51	7.60	9.57	18.33
Tricalcic phosphate	77.17	79.75	77.89	74.72
Magnesian phosphate	0.78	0.53	0.23	0.11
Calcic sulphate	0.15	0.46	0.63	0.56
Calcic carbonate	8.04	3.66	1.59	0.91
Sodic chloride	0.47	0.17	0.16	0.31
Ferrous sulphide	0.11	0.49	0.57	0.27
Ferric oxide	0.16	0.54	0.50	0.34
Lime	0.96	3.36	1.96	2.39
Insoluble and sand	0.50	3.03	6.25	1.40
	99.85	99.59	99.35	99.54

Stammer has promulgated the view, which has met with some approval, viz., that in char, revived in the ordinary manner and at normal temperatures, there are present organic salts of calcium, soluble in water, still more readily in ammonia. By subjecting char to the latter solvent, evaporating to dryness, calcining the residue, and testing for phosphoric acid, none will be found. The lime which remains is the measure of the organic salts of lime, soluble in ammonia water originally present. The lime found in this way does not equal by a long way the "free lime" shown by analysis, but it is assumed that the balance of lime is present as organic calcium salts, insoluble in water and ammonia. If this is so with normally burned char, it follows that the used char before burning must contain a still greater amount of lime in combination with organic acids. In Wagner's analysis, already given, the amount of organic acid present calculated as acetic is equal to the HCl originally present, which amounted to 1 per cent upon the weight of the char, or say, approximately, $1\frac{1}{2}$ per cent of calcium salt. Any organic acids at all likely to be found in sugar, as the result of lactic, butyrous, acetic, mucous, or vinous fermentations, if it were possible even that all should be going on simultaneously, would be found chiefly in the wash waters during the subsequent rigid washing with hot water to which char is always subjected. Supposing even a portion to be finally retained in the char, when sent to the kiln-head the calcic carbonate would be regenerated on re-burning. On the contrary, we do not find any very appreciable difference, as far as regards calcic carbonate, between kiln-head and burned char.

The following analyses represent—V. Char before burning, over which the proper quantity of faintly acid sugar passed in ordinary working. VI. The same char, in order to secure uniform sampling, burned in a pipe in the combustion furnace, at a low red heat.

	Kiln-head Char. V.	Same Char Burned. VI.
Carbon and volatile matter ..	8.95	8.21
Tricalcic phosphate	80.17	80.31
Magnesian phosphate	0.19	0.21
Calcic carbonate	3.46	3.25
Calcic sulphate	0.61	0.70
Sodic chloride	0.16	0.17
Ferrous sulphide	0.36	0.47
Ferric oxide	0.30	0.34
Lime	3.07	2.97
Insoluble and sand	2.51	2.94
	99.78	99.57

Both chars previous to analysis were dried at 200° C., and the results are calculated to dryness.

A portion of V. was boiled with pure caustic soda and filtered. The filtrate, on being acidified with sulphuric acid and distilled, gave nothing more than traces of vola-

tile acids in the distillate. Of course, after allowing the char to ferment, abundant evidence could be had of the presence of the acids in question.

Instead of the lime being in combination with organic matter, it appears more probable that we have present in many charcoals, if not in all, a proportion of oxygen apatite, $3\text{Ca}_3\text{P}_2\text{O}_8 + \text{CaO}$, larger or smaller, according to the working age of the char and the treatment it may have been submitted to.

The following table gives the composition of several charcoal phosphates present in chars from various sources, but all excepting XI., which is new, were contained in the working stocks of different sugar-boilerries, and their composition deduced from the full analysis of the coal:—

	Oxygen Apatite, $3\text{Ca}_3\text{P}_2\text{O}_8$ + CaO.	$\text{Ca}_3\text{P}_2\text{O}_8$	Wibel's Phosphate, $5\text{Ca}_3\text{P}_2\text{O}_8$ + CaO.	VII.	VIII.	IX.	X.	XI.
Phosphoric acid ..	43.21	45.81	44.7	42.99	43.70	43.92	44.08	45.2
Lime ..	56.79	54.19	55.3	57.01	56.30	56.98	55.92	54.76

XI. is unused char. VII. is known to be the most used of the five; but whether the increase in lime is directly proportioned to the age of the char in all cases, it would be rash to hazard an opinion. As far as we can venture to judge, it would appear likely that where the highest heat is carried on the kilns, other things being equal, and the greatest amount of washing is practised, there the basic lime would increase most rapidly. It is interesting to compare these phosphates with the precipitate obtained in the ancient method of phosphate estimation, viz., by adding NH_3 to an HCl solution of the char.

Phosphoric acid	47.01
Lime	51.12
Ferric oxide	1.71
	99.84

The phosphate $\text{Ca}_3\text{P}_6\text{O}_{23}$, octocalcic, yields—

Phosphoric acid ..	48.74	47.91	} = Precipitated phosphates.
Lime	51.26	52.09	

The material from which animal charcoal is manufactured, as far as has yet been proved, is, from a chemical standpoint, a *mechanical* mixture of $\text{Ca}_3\text{P}_2\text{O}_8$, CaCO_3 and gelatin-producing substances, and these, on subjection to a red-heat, yield tricalcic phosphate, with varying proportions of apatite, the undecomposed chalk, and nitrogenous carbon. Wibel (*Berichte der Deutsch. Chem. Gesellschaft*, vii., 220) has demonstrated that, on igniting pure artificial $\text{Ca}_3\text{P}_2\text{O}_8$, chalk, and casein, in varying proportions, the basic salt $5\text{Ca}_3\text{P}_2\text{O}_8 + \text{CaO}$ was obtained, which centesimally approaches the char phosphate X. I have given above. This salt is a chemical individual substance, and not a mixture. After ignition of the mixed phosphate, chalk, and casein, on moistening with ammonium carbonate, from 30 to 40 per cent of the original CO_2 was found to be unrestorable. This proves the futility of the proposals now and then made for determining free lime in char by the use of CO_2 solutions. One is justified in concluding that, had Wibel re-ignited his basic phosphate over and over again with organic matter and calcium carbonate, he would have reached oxygen apatite, $3\text{Ca}_3\text{P}_2\text{O}_8 + \text{CaO}$, and in the continual revivification of charcoal the result is brought about. Aeby (*Scheibler's Zeitschrift für Ruben Zucker*, &c., Jahrg. 1873, s. 57) has tried to demonstrate that the earthy matter of bones consists of this basic salt ($3\text{Ca}_3\text{P}_2\text{O}_8 + \text{CaO}$). If such is the fact we ought to find it in the fresh animal charcoal, and every time it passed through the kiln a certain amount of phosphate still more basic should result. But we find instead that the phosphate in coal from the retorts only varies 0.57 per cent from the normal, if $\text{Ca}_3\text{P}_2\text{O}_8$ is to be regarded as such,—i.e., 16 per cent of the original CO_2 present in the bones is expelled from the chalk, and the lime to which it belonged is linked on to the phosphates,

and after perhaps a hundred ignitions it has not reached further on the way to greater basicity than the apatite stage.

Warington (*Journ. Chem. Soc.*, 1873, xi., 983) has found that the continued action, for two or three hours, of boiling water on gelatinous tricalcic phosphate yields oxygen apatite. Cold water has the same effect, only the time required is immensely lengthened. Whether the basic lime of old char may in any degree arise in a similar way is at present unknown. If one boils a portion of new char in a flask with inverted condenser for twenty-four hours the cellular structure is partially disintegrated, and the pores choked with gelatinous matters. Four samples of new chars were boiled in water some hours, the filtrate collected, evaporated, and the phosphoric acid determined. They yielded 0.061, 0.043, 0.034, 0.043 per cent on the char, and the acid appeared chiefly combined with magnesium. Five grms. of char (VII.) were boiled twelve hours with 100 c.c. of water, and filtered. The filtrate contained 0.206 per cent of lime (on the char), and 0.04 per cent of phosphoric acid. On the basic phosphate the action appears reversed. Further experiments are in progress on the action of water on chars.

We now know that the charcoal in the act of production acquires so much basic lime, and the igneous action is quite capable of giving birth to all the subsequent additions to the initial amount without calling upon the aqueous method for assistance.

The presence of organic matter is apparently helpful to the apatite formation, and in "kiln-head char" which has been thoroughly washed the amount of such matter insoluble in acids is sufficiently large. The following carbon determinations of the same char before and after burning will show this. The samples were taken on different occasions:—

Before.	After.	Difference.
10.11	9.66	0.45
10.14	9.45	0.69
10.65	9.72	0.93
10.37	9.88	0.49
9.98	9.49	0.49
10.55	9.62	0.93
10.43	9.72	0.71

Of course this difference must vary according to the quality of the sugar manipulated and the general mode of working. From what is known in the meantime the hypothesis of Stammer has no solid foundation, but I propose to continue the investigation by making various organic analyses of animal charcoals. It would indeed be strange if any acid should be present capable of resisting (and increasing in spite of) frequent heatings to 800° F., and, though unestimated, showing no loss from deficiency equal to what would be necessary.

Divis (*Chem. Centralblatt*, 1873, 311; *Journ. Chem. Soc.*, xxvii., 709) gives a process for estimating "caustic lime" in char. A weighed amount, boiled with NH_4Cl solution and the evolved NH_3 , passed into standard acid. A char was taken containing no caustic lime, but 2.5 per cent basic lime, and tested carefully by Divis's process. The ammonia evolved was equivalent to 0.504 per cent of lime. As might be anticipated, the process is worthless. If anyone would discover a method by which uncombined or basic lime in char could be reliably and quickly estimated he would render a service, by solving a difficult chemical problem.

ENCROACHMENTS ON THE CHEMICAL PROFESSION.

A FRESH instance of the encroachments referred to in our last issue has occurred in connection with the proposed removal of the Mint to the Savoy. The Government has employed Mr. F. J. Bramwell, an engineer, to report on

the possibility of the Mint proving a nuisance to the neighbourhood. Now, as the nuisance, if any should arise, must depend on the escape of fumes and gases, the question is strictly chemical, and can no more be solved by an engineer than by a lawyer or a clergyman. We are glad to find that this opinion was expressed by one of the speakers at a meeting held in the vestry-room of St. Mary-le-Strand to protest against the New Mint. "Mr. Bramwell" it was remarked "was a civil engineer and not a chemist, and his opinions, therefore, on the fumes which would arise from the melting of the metals could not be worth much." An evening paper, discussing the question, makes the farcical suggestion that if Mr. Bramwell's report is not considered satisfactory "some other eminent engineer" ought to be consulted.

THE "EAGLEY EPIDEMIC."

THIS strange outbreak of fever has occasioned no small amount of controversy in the local papers. The analyses of Mr. E. Sergeant, Public Analyst to the Borough of Bolton, and of Mr. Pattison Muir, of Owens College, Manchester, both show that the suspected milk is abnormally poor, as if diluted with water. But Mr. Muir remarks, very justly, that it is impossible to say, from the results of chemical analysis, whether milk is contaminated with disease germs or not. The water from the farm of Mrs. Kershaw, at Eagley, which it is thought may have found its way into the milk, is reported by Mr. Sergeant to contain 0.26 part of albuminoid ammonia in a million. This is nearly as bad as unfiltered Thames water, taken above Hampton Court, in which 0.28 part of albuminoid ammonia per million were found by Messrs. Wanklyn and Chapman. Of course in water chemical analysis can show merely the total amount of foreign matter present, without indicating how much, if any, of it consists of the germs of disease. Mr. Sergeant's analysis is called in question by Mr. Watson, who doubts that 9 grs. of chlorine could be present in a water yielding only 13.5 grs. of total solids. It is extremely desirable that further light should be thrown upon this case. It is to very little purpose that we insist upon a pure water supply for our cities, if fever-poison may be swallowed in the shape of milk. The village of Eagley itself is said to be plentifully supplied with good water. A Government enquiry is promised, but unfortunately in cases of this kind so much time elapses before suspicion is aroused, that the most valuable evidence is no longer to be obtained.

ON CERTAIN CIRCUMSTANCES WHICH AFFECT THE PURITY OF WATER SUPPLIED FOR DOMESTIC PURPOSES.*

By M. M. PATTISON MUIR, F.R.S.E.,
Assistant Lecturer on Chemistry, Owens College.

WATER as supplied for domestic use may suffer contamination from various sources. Those which I propose to consider are—(1) The metallic pipes through which the water flows, and the metallic vessels in which it may be stored; (2) Certain of the metallic vessels through which the water may pass during various domestic processes; and (3) The existence of cisterns inside the house in which the water may be stored before it is used.

The metals which are most commonly employed in the formation of water pipes, or of vessels in which water is kept, are lead and copper; these metals exert, as is well known, a poisonous action upon the human organism.

It is known that water exerts a certain solvent action upon these metals, and that this action varies in accord-

* A Paper read before the Manchester Literary and Philosophical Society.

TABLE A.
Lead Dissolved by Water Containing Various Salts in Solution.

Salt.	Mgms. per litre.	Grains per gall.	Lead Dissolved.					
			In Mgms. per Litre.			In Grains per Gallon.		
			After 24 hrs.	48 hrs.	72 hrs.	24 hrs.	48 hrs.	72 hrs.
Ammonium nitrate	20	1.4	13.0	—	25.0	0.91	—	1.750
"	40	2.8	15.0	15.0	32.0	1.05	1.05	2.240
"	80	5.6	15.0	—	—	1.05	—	—
Potassium nitrate and Sodium sulphate	20 and 50	1.4 and 3.5	2.0	2.0	—	0.14	0.14	—
Potassium nitrate and Sodium sulphate	40 and 212	2.8 and 14.7	0.8	1.0	1.2	0.05	0.07	0.080
Potassium nitrate and Potass. carbonate	45 and 305	3.1 and 21.5	—	—	0.3	—	—	0.021
Potassium nitrate and Potass. sulphate	70 and 504	5.4 and 35.2	—	—	0.5	—	—	0.035
Calcium sulphate	252	17.5	0.4	—	0.8	0.02	—	0.050
"	408	28.5	0.4	—	1.0	0.02	—	0.070
Potass. Carbonate	310	21.7	—	—	0.2	—	—	0.014
"	516	36.1	—	—	0.2	—	—	0.014
Calcium chloride	250	17.5	0.5	0.5	0.5	0.04	0.04	0.040
"	510	35.7	0.3	—	0.4	0.02	—	0.028
Sodium sulphate	200	14.0	—	—	0.8	—	—	0.050
"	400	28.0	—	—	0.5	—	—	0.030
Ammonium nitrate and Calcium chloride	20 and 60	1.4 and 4.2	—	—	1.8	—	—	0.126
Ammonium nitrate, Potass. carbonate, and Sodium sulphate	20 and 100 and 200	1.4 and 7.0 and 14.0	—	—	0.4	—	—	0.028
Sodium sulphate, Potass. carbonate, and Calcium chloride	200 and 40 and 100	14.0 and 2.8 and 7.0	—	—	0.1	—	—	0.007
Loch Katrine water	—	—	1.0	1.0	1.5	0.07	0.07	0.105
Distilled water	—	—	2.0	2.0	3.0	0.15	0.15	0.210

ance with the quality and quantity of the salts held in solution by the water. I have endeavoured to obtain a few definite measurements of this action in regard to (a) the nature of the salts in solution, (b) the quantity of those salts, and (c) the length of time during which the action proceeds.

I. *Action on Lead.*—A number of solutions were made containing a known amount of various salts dissolved in distilled water; pieces of clean bright lead were suspended in these liquids for various lengths of time, and the amount of lead which was dissolved was estimated at certain intervals; the method employed being the colorimetric one described in the CHEMICAL NEWS, vol. xxxiii., p. 11. The salts employed, the amounts of each, and the amount of lead dissolved after 24, 48, and 72 hours' action are stated in the following table in m.grms. per litre and in grs. per gallon. The surface of lead exposed measured 5600 sq. m.m.

From the table it is evident that the salts enumerated have, when in solution, very different actions upon lead. Nitrates undoubtedly very largely increase the solvent action of water upon lead; the other salts generally diminish this action.

The general conclusion which I would draw from these results are:—

(1.) *Nitrates* if present alone even in small quantity cause water to exert a very marked solvent action upon lead.

(2.) The presence of other salts—*sulphates, carbonates, and chlorides*—along with nitrates, greatly decreases, or even stops, this solvent action; carbonates especially exercise a deterrent action.

(3.) *Carbonates, sulphates, and chlorides*, when added

to distilled water, greatly diminish the solvent action of that water upon lead.

(4.) A small quantity—about 15 grs. per gallon—of these last-mentioned salts exercises almost as great a deterrent action as a comparatively large quantity, about 35 grs. per gallon.

(5.) The amount of lead dissolved increases but slightly after the lapse of twenty-four hours in the presence of these salts which exercise a deterrent action upon the solvent power of water on lead. In the presence of salts which increase this action—notably of nitrates—the amount of lead dissolved increases with the length of time during which the water remains in contact with the lead. I cannot speak with certainty upon this point for a greater length of time than 72 hours.

In these experiments the lead was uniformly clean and bright. Inasmuch as natural waters, even if contaminated with nitrates, usually contain small quantities of soluble carbonates, sulphates, or chlorides, the solvent action of these waters upon leaden pipes and leaden cisterns may, I think, be said to be, under ordinary circumstances, exceedingly small. I would especially draw attention to the experiment made with water containing 1.4 grs. of ammonium nitrate and 42 grs. of calcium chloride per gallon; the amount of lead dissolved after 72 hours being only 0.126 grs. per gallon, whereas water containing the same amount of ammonium nitrate, but without the addition of any other salt, dissolved 1.75 grs. per gallon, or about fifteen times as much lead in the same time. As a water which contains nitrates very often also contains chlorides this reaction is one of some importance.

Under certain circumstances there can be no doubt that water will dissolve considerable quantities of lead, this is

especially the case with water charged with carbon dioxide, and with hot water.

Dr. Roscoe has described a case in which a leaden cistern was very quickly acted upon by hot water.*

(To be continued.)

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, March 2nd, 1876.

Professor ABEL, F.R.S., President, in the Chair.

AFTER the minutes of the previous meeting had been read and confirmed, Messrs. T. Bertram Udall and J. C. Gamble were nominated, and Messrs. Albert B. Prescott, M.D., and Nathaniel Bradley were elected Fellows of the Society after their names had been read the third time.

The PRESIDENT said he had now to invite the Fellows of the Society and the visitors to resume the discussion of Prof. Frankland's paper "*On some Points in the Analysis of Potable Waters.*" At the last meeting he had ventured to point out that there were three points especially to which it would be as well to confine the discussion. These were—First, the direct process for determining the carbon and nitrogen of Messrs. Frankland and Armstrong; secondly, the albuminoid ammonia process of Messrs. Wanklyn, Chapman, and Smith; and, thirdly, the evidence of pre-existing sewage contamination, and its value as affording indications of the wholesomeness of a potable water. Dr. Frankland had had a large experience, extending over many years, in the application of his method; and the diagrams which were exhibited afforded incontestable evidence of its value, showing as they did, not only the relative purity of waters from different sources, and the fluctuations caused by floods, but also the more minute details of the variation in water supplied by different companies from one and the same source, caused by differences in storage, the power of excluding flood-waters, &c. Since the process was first introduced various improvements had been made, which evidently much facilitated and expedited its working, especially in the method of evaporation; but there were one or two points upon which he would like to elicit information. The evaporation to dryness certainly appeared to be a weak point; for although no doubt the carbon and nitrogen in the residue obtained could be most accurately determined, did the residue really contain the whole of the organic matter present in the water? Were there any organic matters, however small in quantity, carried off mechanically or oxidised during the evaporation? The sulphurous acid used in the evaporation process, moreover, must become in part oxidised: did the small quantity of sulphuric acid thus formed cause the destruction or removal of any carbonaceous matter? Again, when nitrates were present, certain precautions were necessary: but had it been proved that the nitrates were entirely removed by the evaporation with sulphurous acid, even with the addition of ferric chloride? Dr. Frankland had published certain results, in which organic matter of known composition had been added to pure water, and the amount in the residue determined; but he did not know whether any nitrates were present in these cases, or if so what effect their presence and the consequent treatment of the water had upon the results obtained. With regard to the albuminoid ammonia method, which was very extensively used, and with which he himself had had some experience, although it gave concordant results if uniformly conducted, yet if he were asked, he could not say exactly what it indicated by itself; but

taking it in relation with the inorganic constituents and a knowledge of the source, the surroundings, and previous history of the water, &c., it was of value in forming an opinion regarding the wholesomeness of a potable water.

Mr. WANKLYN said that, besides the impracticable character of the "combustion" process of water analysis (to which character the discoverers of the process ascribe its almost universal rejection by chemists), another defect in the process has prevented chemists from resorting to it. This defect may be broadly said to be that whilst the process professes to measure the carbon and nitrogen existing in organic combination in drinking-waters, it fails to make these measurements. This failure follows as a necessary consequence from two of the conditions under which the process is placed. One of these conditions is that before resorting to "combustion," there is an evaporation of the water to dryness, and that destruction of the organic matter occurs during this evaporation. Some of the organic substances present in drinking-water are very perishable, and others doubtless are almost indestructible when exposed for several hours to the action of air and moisture at temperatures not exceeding 100° C. The few milligrms. of organic matter contained by a litre of drinking-waters are, probably in no single instance, preserved absolutely intact in the residue left on evaporating a litre of water down to dryness. In some cases, indeed, not the one-thousandth part of the organic matter originally present in the water will survive the evaporation. If, then, the actual combustion itself were perfectly devoid of experimental error, it follows, as a matter of necessity, that the combustion process of water analysis must sometimes fail to measure $\frac{999}{1000}$ of the carbon and nitrogen existing in organic combination in drinking-water. The second condition under which the "combustion" process of water analysis is placed, and which renders it a failure, is that, owing to the extreme minuteness of the proportion of organic matter in drinking-water, the combustion has to be made on too small a scale. Instead of burning the quantity of organic matter which chemists are accustomed to employ (viz., from 0.1 to 0.5 grms.), the operator burns only a few milligrms. when he practises the Frankland process. Unless, therefore, the absolute experimental error had been diminished by the modifications of the operation of combustion, it will be acknowledged that the error of experiment is so large as to render illusory the measurement of organic carbon and nitrogen in drinking-water. Has Dr. Frankland diminished the absolute error in combustion? The paper read by Dr. Frankland to this Society in the year 1868, shows that at that date he had not diminished it. On that occasion—viz., in the year 1868—having burnt centigrms. of organic substance, whilst chemists usually burn decigrms., he made an absolute error of about $\frac{1}{2}$ of the quantity of carbon operated upon; and (since the usual experimental error is $\frac{1}{200}$) it will be seen that the absolute error remained about the same. The Lecturer has alleged that the accuracy of the process has been increased since 1868, and in the appendix to the last Report of the Rivers' Commission, *vide* page 505, three experiments on sulphate of quinine are published in support of that assertion. A correspondence published in the CHEMICAL NEWS sheds light on the character of these experiments, and I presume that the majority of chemists will not place implicit confidence in them. To those chemists (if any such there be) who are convinced by the quinine experiments, and who believe that the absolute error, which was 0.30 milligrm. of nitrogen in the year 1868, had diminished to 0.01 milligrm. in the year 1873, I would suggest that the curve showing the amount of organic matter in Thames water during the last eight years must surely be the curve expressing the improvement in the Frankland process. And highly interesting it is that this should follow the flow of the River Thames. Passing next to the Lecturer's criticisms of the ammonia process of water analysis, invented by Chapman, Smith, and myself, I will be very brief. The

* Proceedings of the Manchester Literary and Philosophical Society, xiv., p. 23.

statements that "*the actual present organic matter in water can only be ascertained by the estimation of the carbon and nitrogen*," and that "*the albuminoid ammonia process affords no evidence of the absolute quantity either of the organic matter or of organic nitrogen present in potable waters*," are based on a long-exploded fallacy which I believe Liebig once encountered. The fallacy is that the only quantitative operation to which organic compounds can be submitted is the process of elementary analysis into carbonic acid, water, and nitrogen. I will answer it by a parallel. Acetic ether may be made to yield acetic acid quite quantitatively; and in whatever sense the carbon contained by acetic ether can be said to be a measure or index to the acetic ether, in that same sense the acetic acid is a measure or index to the acetic ether. An unknown mixture of acetates could not be absolutely measured by the carbon it contains, neither could the mixture be absolutely measured by the acetic acid which it gives. But in whatever sense the carbon can be said to be an approximate measure of the mixed acetates, in that same sense is the acetic acid a measure. For acetic acid read albuminoid ammonia, for acetic ether read albuminous substances, and for mixed acetates read the miscellaneous nitrogenous organic matters in drinking-water, and the parallel is complete. To the Lecturer's objection that there are nitrogenous organic substances which yield no albuminoid ammonia, I answer that urea is the only one which is likely to occur in drinking-water, and urea is fully provided for by the ammonia process. If, perchance, a trace of nitro-compound ever make its way into drinking-water it would not be detected by the ammonia process. But that is no objection to the ammonia process, which is designed to measure the albuminoid substances and miscellaneous nitrogenous *débris* in water, and which is all the more valuable for not confounding nitro-compounds with them. The Lecturer's assertion that my colleagues and I "declared the waters of Bala Lake, and that supplied to Manchester from the Derbyshire hills to be no purer than Thames water," is a very curious misrepresentation of what we really do maintain. The passage requires the insertion of the word "filtered," and then would be correct. That filtered water is clean, and that rain-water is sometimes foul, has been taught us by the ammonia process, and is, indeed, consonant with reason. On the subject of pre-existing sewage contamination I need say very little. The cleanest water that is found in nature is loaded with nitrates, the dirtiest is sometimes almost devoid of nitrates, and the same river-water is at one time charged with nitrates, and at another time devoid of nitrates, and suffers no corresponding fluctuation in the sewage poured into it. Knowing this I cannot understand how nitrates can indicate previous sewage contamination, and, as is well known, I systematically disregard them in the ordinary course of water analysis. I am even disposed to look on their presence as evidence that the water which contains them has undergone purification.

Dr. LETHEBY said he was very sorry that he was unable to be present at the lecture, and after some preliminary observations remarked that as very small quantities of substance were dealt with small errors became of great importance. He thought that during the evaporation of the water the atmospheric oxygen and the oxygen dissolved in the water must destroy a portion of the organic matter. Moreover, in order to get rid of the carbonic acid and nitrates from the residue, sulphurous acid had to be used, which became in part changed into sulphuric acid, so that if too much were produced organic matter would be destroyed, and if too little sulphurous acid were used we should have nitrates present in the residue. Again, in making the combustion, there were practical difficulties, such as the introduction of organic dust from the atmosphere, &c. In fact, so unsatisfactory were the results which his coadjutor, Dr. Tidy, had obtained with it, although he had been to Dr. Frankland's laboratory to learn the process, that they had rejected it. He con-

sidered the term pre-existing sewage contamination a very improper expression, a very inexact expression, and very likely to mislead, for the organic nitrogen in water does not necessarily indicate sewage contamination; for instance, the water of the Nile, which in Egypt is considered a very sweet water, contains organic nitrogen, in plenty and abundance of nitrates, and that 400 miles above Cairo. He believed the estimation in which chemical opinions on sanitary subjects was held would be lowered, and much harm done by the general adoption of such an expression.

Dr. TIDY thought it was a pity such a diversity of opinion should exist amongst chemists on the subject of water analysis. He did not think there was much force in the objection to Dr. Frankland's process that it takes much time and trouble; for if it gave accurate results such a question ought never to be raised. But did it give accurate results? He said he had made sixty-eight determinations by the process, and then proceeded to give an account of the results he had obtained with it, from which it appeared that in sixteen cases out of twenty-eight he had obtained nearly double the amount of carbon he ought, and the nitrogen determinations were equally unsatisfactory, although every care had been taken in the evaporation. He had also found great practical difficulty in detaching the residue from the capsule, and in keeping out dust, &c. With regard to the albuminoid process he must express his regret that Mr. Wanklyn had not published an extended list of carefully-collected details of experiments, as he had found not only that the whole of the nitrogen was not obtained, but that the presence of chlorides certainly affected the results. You were told to distil the permanganate solution until ammonia ceased to be given off, but if you then left it for a day, and again distilled, you would again get ammonia, and the next day more. Notwithstanding this, it certainly gave results which had some relation to the amount of deleterious impurity present. A potable water, however, should be judged rather from a broad view of the whole of the results of the analysis.

Dr. RUSSELL said that after the display of oratory they had just heard he felt some diffidence in speaking, but his excuse must be that he had some little experience both with Dr. Frankland's and with Mr. Wanklyn's methods. There certainly were difficulties of manipulation attached to the former process, amongst which he might mention the obtaining pure copper oxide, and the assaying of it; but if the operations were carefully performed, accurate results would be obtained, although inconveniently large errors of observation might occur. The necessity of setting up a complicated apparatus militated against its use in laboratories where water analysis was only occasionally performed. The albuminoid ammonia process had come into general use from its comparative simplicity; the results obtained by it, however, should never be taken alone, but in conjunction with the amount of the chlorides and the remainder of the analysis, and the same might also be said of Dr. Frankland's process, for the question put to chemists practically amounted to this—Is this water fit for use? He thought the ammonia process the most convenient for general use, but when any special points were to be brought out we should fall back on the "combustion method," which was a most admirable scientific process, and had given us some splendid and instructive information.

Dr. VOELCKER would have left no stone unturned to follow out Dr. Frankland's process if he could see what to do with the carbon and nitrogen when he had got it. But if he could do nothing with it, why should he go to so much trouble to ascertain its amount? Moreover, if the amount of carbon and nitrogen present was an indication of the impurity of the water, we should have to reject many waters of a peaty nature which were perfectly wholesome. He had adopted Wanklyn and Chapman's process, not because he considered the results obtained by it as strictly exact, but because it gave extremely useful indications of the purity of a water when taken in conjunction

with the other results of analysis. There was one point which had not been previously noticed, and which he might allude to, and that was the occurrence of notable quantities of phosphoric acid as phosphate of lime in waters which were decidedly unwholesome, generally associated with albumenoid ammonia, chlorides, and alkaline carbonates. If such a water were filtered through a soil the phosphates would be removed, but not if it had merely trickled through fissures in the ground; so that a well-water which contained phosphates might be considered unwholesome and exhibiting actual sewage contamination.

Dr. BISCHOFF remarked that although the albuminoid ammonia process had the advantage of simplicity of manipulation, it was not, strictly speaking, a quantitative method, and the results obtained by it were often fallacious. The speaker then showed that when examined by this process a pure water mixed with 0.01 per cent of urine would be considered wholesome, and also quoted numbers obtained by two Scotch chemists which were anything but concordant. He had also made experiments with the water of Loch Katrine by Frankland's process, which gave 0.162 carbon and 0.024 nitrogen. On adding variable quantities of nitrates to this, and evaporating, he had obtained the numbers 0.140 C and 0.020 N in one instance, and with a larger proportion of nitrates the numbers 0.170 C and 0.018 N.

Mr. W. THORP said that in connection with the Rivers' Commission he had had a large experience in the matter of water analysis, having collated the results of many thousands of determinations made by himself and three or four colleagues. He had been rather amused at the doleful description of the difficulties of the process given by some of the speakers; for instance, the removing the water residue from the capsule. This he could say had never been felt at the laboratory of the Rivers' Commission, except in a few instances where the water was contaminated with some factory products. The combustion process does what no other process does, and that is, gives the relative proportion of carbon and nitrogen. This ratio is of the utmost importance in forming an opinion on the nature of a water. With regard to the term previous sewage contamination, most of the mistakes arose from a misunderstanding of plain English, by confounding it with actual or *present* sewage contamination. In order to overcome this difficulty, however, he would propose to use instead "mineral combined nitrogen," or, more shortly, "mineral nitrogen," and would recommend that instead of reporting the nitrogen as ammonia it should be given as nitrogen present as ammonia.

Dr. ARMSTRONG said he would like to make a few remarks on the sources of error in the combustion process. The loss by evaporation had been much exaggerated. As now performed, under a glass shade, in a reducing atmosphere containing sulphurous acid, oxidation would be reduced to a minimum, and Pasteur has shown that, on exposure to the air of organic matters, but little action took place at the ordinary temperature, even in three years; the absorption of oxygen which usually takes place being due to the presence of living organisms. At the high temperature of the water during the evaporation this cause could not operate. Again, as far as our knowledge of the splitting up of albuminoids under the influence of dilute acids extends, they appear to yield bodies even more stable than the original substances, and therefore not likely to be affected by the evaporation.

Dr. DUPRÉ made some remarks on the indications given by the albuminoid process, pointing out that there was no difficulty in determining by it whether a water was bad, but whether a water which is nearly pure is wholesome or not. He considered that good results could be obtained by the ammonia process when fairly worked.

Mr. FRISWELL said as far as his experience went it was difficult to obtain good results when a very bad water was treated by the ammonia process, and illustrated his remarks by the abnormal results obtained with an effluent water he had examined, to which—

Mr. WANKLYN replied that the ammonia process was not properly worked unless the water was completely exhausted, and where time was an object it was advisable to operate on a comparatively small quantity when the water was of this character. One of the merits of the ammonia process was that it concentrated the ammonia, and thus rendered evident very minute quantities of albuminoid substances.

Dr. FRANKLAND, in replying to the various speakers, said he would make his remarks as brief as possible. With regard to Mr. Wanklyn's objection to the process on account of the difficulties of manipulation, he was afraid he still belonged to the number of those who had not tried it, and whose objections were purely theoretical, for he seemed to imagine that the errors incident to the determination were the same in amount as those of an ordinary combustion, but this was not the case. In the original paper, where the determinations were made with comparatively large quantities of organic matters, the error was found to be about 1-20th, and although even this was of no moment in forming an opinion about a water, yet subsequent experiments had shown that this error was not transferred when smaller quantities were operated on. With very small quantities results were now obtained within 1-44th of the whole organic matter present. The comparative error must be looked at in as far as it affects the chemist's opinion of the water. With regard to the possibility of a minute portion of the nitrates escaping destruction in the evaporation of the water, the original experiments of Dr. Armstrong and himself showed that this was not the case, as did also the analysis of certain waters which contained nitrates, but which gave no organic nitrogen on combustion. Moreover, Dr. Bischoff's experiments with the Loch Katrine water proved that the presence of nitrates did not interfere with a correct determination of the organic carbon and nitrogen, a result which may also be deduced from the results of the analyses of the Thames waters—for example, in summer, when the amount of nitrates varies most, the organic elements remain about the same. This the speaker illustrated by the numbers obtained with five of these waters in 1870. In order to obtain some evidence of the extent of the oxidation which might take place during evaporation, he had repeated the sawdust experiment referred to by Mr. Wanklyn at the last lecture, but after passing air over it for two whole days, at a temperature of about 70° C., he had only obtained mere traces of carbonic acid. All the recent experiments on putrefaction tended to show that the development of carbonic acid was due to the presence of germs, and at 70° or 80° C. these germs would be destroyed. Moreover, the organic matter during evaporation would be protected by the sulphurous acid. The amount of ammonia obtained from a water by the albuminoid process had comparatively little bearing on the subject, as it was found that waters, as those of a peaty nature, where you would expect little, gave the largest quantity. Mr. Wanklyn himself had found the Bala Lake to give 0.25, Loch Katrine 0.08, whilst the Thames only gave 0.06, making the Thames water appear the purest. The speaker then proceeded to very strongly criticise Dr. Letheby's remarks, especially alluding to his adherence to the method of estimating the amount of organic impurity present in a water by means of a solution of permanganate, and to his opinion that the evidence of pre-existing sewage contamination was of little or no moment. He would also reply to Dr. Voelcker's observations, that by means of the combustion method, in a very large number of cases at all events we could most positively detect the difference between contamination by vegetable or animal matters, and hoped shortly to bring this important matter before the notice of the Society. After referring to the formation of sulphuric acid from the sulphurous acid during evaporation, and the reasons why he had adopted the term "pre-existing sewage contamination," he concluded by saying he quite agreed with the President that the importance of judging of a water by all its antecedents could scarcely be over-rated.

The meeting was then adjourned until Thursday, March 16, when the following papers will be read:— (1) "Experiments on Crystallised Glycerin," by Dr. van Roos; (2) "Notes on the Fatty Acids, and on a Suggested Application of Photography," by Mr. W. H. Hatcher; (3) "On Stibine," by Mr. F. Jones; (4) "On the Use of Platinum in the Ultimate Analysis of Carbon Compounds," by Mr. F. Kopfer; (5) "Action of Sulphuric Acid on Naphthalene," by Dr. J. Stenhouse and Mr. C. E. Groves; (6) "On the Action of the Organic Acids and their Anhydrides on the Natural Alkaloids," Part V., by Mr. G. H. Beckett and Dr. C. R. A. Wright.

CORRESPONDENCE.

ELECTROLYTIC COLOURATION OF THE SALTS OF ANILINE.

To the Editor of the Chemical News.

SIR,—I have noticed in the CHEMICAL NEWS (vol. xxxiii., p. 78) a translation of a paper from the *Comptes Rendus*, by M. F. Goppelsröder on the production of green and blue pigmental matter at the positive pole when the salts of aniline are submitted to galvanic action. This fact was observed by me as far back as the year 1862, and it was the subject of a communication by me to the Chemical Society of London on the 20th March, 1862. The paper is published in the *Journal of the Chemical Society*, vol. xv., p. 161 (1862), and it is noticed in the CHEMICAL NEWS, vol. v., p. 178. I mention this for the purpose of showing that the production of this pigmental matter and the account of its leading chemical properties have been known for many years.—I am, &c.,

H. LETHEBY, M.R., M.S.

Laboratory, London Hospital College,
March 3, 1876.

ORGANISATION AMONG CHEMISTS.

To the Editor of the Chemical News.

SIR,—All chemists who appreciate aright matters affecting the interests of their profession must have welcomed the recent articles on the above subject that have appeared in your journal.

Chemistry is not only "a hard kind of badly remunerated work," but professional men are, under the existing circumstances, classed to-day with quacks who rejoice in the possession of bogus degrees, and to-morrow fees which should come to them are pocketed by men who ruin the title of "chemist" by borrowing it. And yet the remedy for these evils is in our own hands, and it is only necessary to following out some such plan as that so ably indicated by Dr. Wright, to effectually enhance the professional status of chemists.

In commenting on Mr. Allen's letter, you, Sir, have shown the undesirability to take the Society of Public Analysts as the nucleus of the proposed Chemical Guild or Association, and this view I venture to think, will be endorsed by most analytical chemists, for the reasons you have specified.

Further, as Dr. Wright has shown, the Chemical Society occupies very distinct ground from that which should be covered by the proposed guild; therefore, any attempt that should be made to establish such an institution must come from the body of chemists themselves.

While, however, nearly every chemist of any repute could furnish abundant evidence of the evils attending the present non-organised state of the profession from his own history, it is matter of wonder that no steps have been taken hitherto to cure these evils.

To successfully work out such a scheme as that

developed by Dr. Wright requires unity of strength and energy of character on the part of those who undertake it, and it becomes chemists to amiably join hands in the work, and to forget those differences which exist between many of them (which differences, be it remarked, are in no small measure inherent to the present nature of things), while endeavouring to secure for their profession that esteem in which it should be held.

The formation of a Chemical Guild would quickly engender a reformation among the disciples of the craft, who henceforth would not be regarded as fanatics by the outside world, and who would then be encouraged rather than tolerated as now. Such a guild too, would be especially powerful in overthrowing an evil which exists on all sides, viz., that exerted through the agency of cliques.

That a provisional committee may be formed which shall invite chemists to attend a meeting, with the view of establishing such a guild, is much to be desired.—I am, &c.,

CHARLES T. KINGZETT.

INFUSING TEA.

To the Editor of the Chemical News.

SIR,—It has often been stated that the boiling of hard water softens and renders it more valuable for cooking purposes, for the using of soap, and for the making of tea. It is so stated in evidence given before the Royal Commission appointed "To Enquire into the best Means of Preventing the Pollution of Rivers, and the Domestic Water Supply of Great Britain." The Commission sat in 1868. Now my experience in reference to the last named article, viz., tea, is the exact reverse of this. If the same quantity (about the weight of a sixpenny piece) of a given Congou tea, be put into two tasting pots, and the water used for one be exactly at the boiling-point, and that for the other be allowed to boil ten minutes, it will be found that the infusions (liquors) thus made (each having been infused five minutes) bear little or no resemblance to each other. The former made from water at boiling-point will have a liquor of a deep red colour resembling polished mahogany; the latter made with the same water which has boiled for ten minutes will be light in colour, resembling newly polished oak; thin, brackish, and flat to the taste, and worth 50 per cent less money.

I have proved this often with water from Derby, Leicester, Dover, and Plymouth, and many other towns, the water supplies of which differ enormously in character, but the effect is always the same, no matter if the water be soft or hard.

If any one could enlighten me why ten minutes boiling should create this great difference, they would confer a boon.—I am, &c.,

FRANK RANGER.

47, and 20, Botolph Lane, E.C.,
March 7, 1876.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Seances de l'Academie des Sciences. No. 7, February 14, 1876.

Ethers of the Hydracids.—M. Berthelot.

Formation of the Amides.—M. Berthelot.

Hyposulphite of Potassa.—M. Berthelot.—Three thermo-chemical papers, to which justice could not be done in an abstraction.

Laws of Gases.—M. D. Mendeleeff.—The author endeavours to find a closer approximation than the laws of Mariotte, of Gay-Lussac, and of Ampère and Avogadro.

Isomeric Rosanilins.—M. A. Rosenstiehl.—Reserved for insertion.

Optical Inactivity of a Reductive Sugar Present in Commercial Products.—MM. Aimé Girard and Laborde.—A reply to M. Maumené's complaint that the authors have ascribed an opinion, which he claims, to M. Dubrunfaut.

New Element in the Determination of Chemico-Calories.—M. Maumené.—The author shows that liquids of widely different characters experience a molecular alteration without any change in their nature. The effect of heat—a purely physical influence—gives them a species of *temper*, during which their chemical reactions show abnormal numbers of chemico-calories.

New Acid Pre-existing in the Recent Milk of Mares.—M. J. Duval.—The author has discovered in mare's milk a proximate principle not found in the milk of ruminants. Equinic acid crystallises in groups of small needles, not volatile without decomposition, of a fragrant odour and peculiar taste. Its reactions with nitrate of silver, perchloride of iron, chloride of gold, &c., distinguish it from hippuric acid.

Revue Universelle des Mines, de la Metallurgie, de Travaux Publics, des Sciences et des Arts Appliqués à l'Industrie, November and December, 1875.

This issue contains no chemical matter.

PATENTS.

ABRIDGMENTS OF PROVISIONAL AND COMPLETE SPECIFICATIONS.

Improvements in the preparation and treatment of soaps, and in combining therewith certain vegetable, farinaceous, or equivalent natural matters; also in apparatus or means therefor; parts of which improvements are applicable to other soaps. W. Green, St. Laurence, Thanet, Kent. January 13, 1875.—No. 129. This invention consists in the production of soaps possessing emollient and curative properties, by the admixture therewith of a larger percentage than heretofore employed of seaweed or extracts thereof, or similar or equivalent matters, or saponified wheaten flour or similar matters, together with soluble glass, liquid quartz, silicate of soda, or the aqueous solution of any of the chemical or commercial varieties and forms of silicates of soda or potassa, and other ingredients, as rosin, borax, steatite, &c., which, while assisting to give firmness to such soaps, will not neutralise the properties of the seaweed or equivalent matters employed; also the production for such purposes of a saponaceous fluid or paste, by the admixture with seaweed or equivalent matters of ingredients as before described, together with saponified or partially saponified farinaceous vegetable, animal, mineral, bituminous, or other cheap or refuse saponifiable matter. Several processes are described, showing special modes of applying the invention to ordinary household soaps, also to perfumed, coloured, medicated, farinaceous, glycerine, tar, saccharine, "milling," and other special or fancy soaps, and soap-powder. A mode is also described of mixing such matters with soap made by the cold process, and also by grinding, or equivalent means without melting. The inventor further claims the admixture of such matters with all kinds of soaps, however made, when from 50 to 100, or from 100 to 300 per cent and upwards of seaweed, or extracts, or equivalent matters are employed. The invention also comprises certain modes of treating seaweed or similar matters for obtaining mucilage, pulp, or powder; also the use of vacuum-pans or other apparatus for boiling or melting such soaps or fluids at a low temperature; also the use of stirring or agitating appliances driven by power in the manufacture or treatment of such soaps or fluids; also certain apparatus for ornamenting such soaps, consisting of appliances for interlacing, plugging, pouring, cutting, and figuring, which appliances are applicable to other soaps; certain terms as, "seaweed soap," and equivalent appellations are employed for distinguishing those soaps from others.

Improvements in the purification of coal-gas. Major-General Scott, C.B., Ealing, Middlesex. January 13, 1875.—No. 135. The objects of this invention are (1) the removal of the carbonic acid from the gas by means of magnesia; (2) the breaking up of the bisulphide of carbon; and (3) the abstraction of the carbonate of ammonia from the gas-liquor.

An improved method of and apparatus for preventing the formation and explosion of gas in the storage, transport, and delivery of gaseous or inflammable oil or spirit. G. R. F. Sinibaldi, New Cross, Kent.

(Partly a communication from P. Sinibaldi, Paris.) January 13, 1875.—No. 136. This invention relates to a novel method of and apparatus for preventing explosions of the gas generated from petroleum and other like gaseous and inflammable materials in the storage or transport, or measuring and delivering of the same. The said invention consists, first, in storing the oil or spirit in a vessel in such a manner that to be withdrawn therefrom it must be displaced by the introduction of another liquid of different specific gravity, so that there is never any empty space in the vessel above the gaseous liquid, and therefore no gas can be generated from the said liquid in the vessel. The said invention also consists in apparatus composed of a cistern or reservoir of iron or other suitable material in which the petroleum and other oil or spirit is stored, and from which the same is drawn off ready measured.

Improvements in the manufacture of sulphates, and in apparatus employed therefor. J. Hargreaves, Widnes, Lancaster. January 14, 1875.—No. 147. This relates to the well known direct action process for the manufacture of sulphates of soda and potassa, and consists—First. In making each joint of the sulphurous acid flues pass through a box or well containing fine sand or like material. Second. In forming the pyrites burner doors of a fire-brick tile or non-conducting material contained in a frame. Third. In placing the converting chambers in series back to back, and passing the sulphurous acid flue between them. Fourth. In superheating steam before admixture with the sulphurous acid by passing such steam through a chamber of finished sulphate, and in drawing air from a chamber of finished sulphate through a newly-charged chamber. Fifth. In using vessels of water to cool, when too hot, the gases issuing from the exhauster, and heat such gases when too cold. Sixth. In forming the enclosing walls of pyrites burners double, and passing steam through the space thus formed, such steam is afterwards mixed with the sulphurous acid; in making openings from the said space into the pyrites burners, and in passing steam through the pyrites burners. Seventh. In using an Archimedean screw, continuous scrapers, or travelling aprons to withdraw finished chloride from the chambers. Eighth. In passing the evolved gases through cooling syphons placed between the exhaust and the condensing tower. Ninth. In consolidating and breaking salt by passing such salt on an endless band between rollers through a heating chamber, and finally through breakers rollers.

MEETINGS FOR THE WEEK.

- SATURDAY, 11th.**—Physical, 3. "Influence of Light on the Electric Conductivity of Selenium, and on a Selenium Photometer," by Prof. W. G. Adams, F.R.S. "On some Problems connected with the Flow of Electricity in a Plane," by O. J. Lodge.
- MONDAY, 13th.**—Medical, 8.
— London Institution, 5.
— Royal Geographical, 8.30.
- TUESDAY, 14th.**—Civil Engineers, 8.
— Royal Institution, 3. "On the Classification of the Vertebrated Animals," by Prof. Garrod.
— Society of Arts, 8. (African Section). "The Diamond Fields of South Africa, and their Influence on the Native Races of the Neighbourhood," by J. B. Currey.
- WEDNESDAY, 15th.**—Society of Arts, 8. "A New Bridge for Providing for the Traffic across the Thames below London Bridge," by Frederick Barnett.
— Meteorological, 7.
— Society of Public Analysts, 6. "The Determination of Quinine in Organic Liquids," by A. H. Allen, F.C.S. "On an Abnormal Sample of New Milk," by J. H. Pattinson. "The Determination of the Melting-Point of Butter and Other Fats," by T. Redwood, Ph.D. "On the Detection of Alum in Bread," by J. A. Wanklyn. "On the Analysis of Butter," by J. Muter, Ph.D., M.A.
- THURSDAY, 16th.**—Royal, 8.30.
— Royal Institution, 3. "Polarised Light," by Mr. Spottiswoode.
— Royal Society Club, 6.30.
— London Institution, 7.
— Zoological, 4.
— Chemical, 8. "Experiments with Crystallised Glycerin," by Dr. Roos. "Notes on the Fatty Acids, and on a suggested Application of Photography," by W. H. Hatcher. "On Stibine," by F. Jones. "On the Use of Platinum in the Ultimate Analysis of Carbon Compounds," by Ferdinand Kopfer. "The Action of Sulphuric Acid on Naphthalen," by Dr. Stenhouse and Mr. Groves. "Action of Organic Acids and their Anhydrides on the Natural Alkaloids," by Mr. Beckett and Dr. Wright.
- FRIDAY, 17th.**—Royal Institution, 9. "Feudal Property on the Eve of French Revolution," by Sir H. Maine.
— Society of Arts, 8. Chemical Section. "The Preparation of Dextrin-Maltose (Malt-Sugar), and its Use for Brewing and other Purposes," by W. Valentin, F.C.S.
- SATURDAY, 18th.**—Royal Institution, 3. "Human Senses," by Prof. Robertson.

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DISCOVERERS AND ?

"Sic vos non vobis mellificatis apes."

It has more than once fallen to our lot to vindicate the claims of original discoverers in the pages of this journal. It sometimes happens that the same discovery is made by two men quite independently of each other. In such cases, although both may deserve equal credit, he who can prove priority of publication is acknowledged to be entitled to the honour attending the discovery.

It also sometimes happens that a discoverer, after his results have been not only published, but fully discussed in the scientific journals, finds them re-discovered, and laid before some illustrious society as an entire novelty, without the slightest reference to his labours. We have even known the "conveyer," as in the case of thallium, to accuse the original discoverer of appropriation.

It is not many years since we drew the attention of our readers to the fact of the well-known process of Parkes for the volumetric determination of copper being claimed by M. de la Folie as an improvement of his own upon the sulphide of sodium process of Pelouze, and, strange to relate, Pelouze himself actually described it as such in a memoir read before the Academy of Sciences. So far as we know—we shall be glad to be corrected if we are wrong—this claim has never been withdrawn, notwithstanding the indignant protests of German as well as English chemists. Now it is not to be expected that one man will surrender his claim until priority of discovery has been clearly established by another; but let this once be done, and he who desires to act honourably will at once concede to his rival the honour due to him. To act otherwise is dishonourable in the highest degree; nor is it, in our opinion, less dishonourable for one scientific man to endeavour to injure, or detract from, the reputation of another. We can understand feelings of national pride, and jealousy of national interests, leading men to leave no stone unturned in their endeavour to claim priority of discovery for a fellow-countryman, but we utterly fail to comprehend the motive which prompts a man to shift the credit of a discovery or an invention from a fellow-countryman to a foreigner. At the risk of being accused of obtrusiveness and egotism we feel bound to ask if this has not been the course pursued by Prof. Osborne Reynolds in his several notices of the Radiometer at the Manchester Literary and Philosophical Society. Prof. Osborne Reynolds we have always regarded as a man of singular ability, a physicist who, by earnest study and close application, has attained to an honourable position in the scientific world. His opinion on subjects connected with mechanical and physical science deservedly commands attention, and it is because we entertain these views of his scientific attainments that we think it necessary to point out what would seem to be a studied injustice unworthy of his present position.

The facts of the case are simple. In December, 1873, the first part of a paper "On Attraction and Repulsion Resulting from Radiation" was read before the Royal Society. The experiments therein described led to the construction of an instrument, which is now well known as Crookes's Radiometer, which was first exhibited at the *Soirée* of the Royal Society on the 7th April, 1875. In December, 1875, our attention was directed to an advertisement in *Nature*, in which Mr. Browning announced that he had been appointed sole agent for a new Radiometer made by Herr Geissler. We at once called on Mr. Browning, and he saw that the advertisement gave undue credit to Herr Geissler, and readily made the necessary alteration.

In January, 1876, it was announced by the Secretaries of the Manchester Literary and Philosophical Society (Prof. Osborne Reynolds and Mr. Baxendell) that at the next meeting Prof. Osborne Reynolds would exhibit and explain Crookes's Radiometer. In the report of the meeting on the 25th January, however, the instrument is described as "Geissler's light-mill." On the 17th February it is announced by circular that at a meeting of the Society on the 22nd inst. Prof. Reynolds will show some further illustrations with reference to the action of "Geissler's light-mills."

Any person unacquainted with the circumstances might infer from the above programme that the "light-mill" was a new and independent discovery of Herr Geissler; yet all the time it is simply the radiometer which has been exhibited before the Royal Society and the Royal Institution. Herr Geissler certainly makes radiometers so as to bear transportation, according to a suggestion given him by the inventor through Prof. Zöllner. The latter distinguished physicist, in an autograph letter, declares—"As regards the name of the instrument, 'Crookes's Radiometer' is the only suitable appellation, expressing at once the merit of the discoverer and the nature of the matter." It is no disparagement to the unimpeachable skill of Herr Geissler to say that the instrument should bear the name of its inventor rather than of its maker.

It is, to say the least, an unfortunate coincidence that the gentleman who has thought fit to employ this novel nomenclature has, with reference to the discovery in question, scarcely shown the candour and courtesy usual in physical and chemical discussions. After the reading of the first paper at the Royal Society Prof. Osborne Reynolds, in a letter to the editor of *Nature*, and in a paper which was read before the Royal Society on June 18, 1875, gave it as his opinion that the results we had obtained were in part due to evaporation and condensation, and that they afforded a direct proof of the truth of the kinetic theory of gases. Both in a letter to *Nature*, and in a lecture delivered before the Physical Society on the 20th of June, we brought forward what we conceived to be reasonable arguments against this supposed explanation. We stated that our impression was that the repulsion caused by radiation was directly due to impact upon the surface of the moving mass, and not secondarily through the intervention of air currents, electricity, or evaporation and condensation, but that we did not wish to insist upon any theory of our own, our object being to ascertain facts, varying the conditions of each experiment, so as to find out what are the necessary and what the accidental accompaniments of the phenomena. By working steadily in this manner, letting each group of experiments point out the direction for the next group, and following up as closely as possible, not only the main line of research, but also the little bye-lanes which often lead to the most valuable results, after a time the facts will group themselves together and tell their own tale. The conditions under which the phenomena invariably occur will give the laws, and the theory will follow without much difficulty.

At the meeting of the British Association in August last, Prof. Reynolds said* "he thought that some energy was given to the moving body which enabled it to propel itself. He did not desire to throw any slight upon the work of Mr. Crookes, but the latter should not produce facts to subvert preconceived notions of the nature of things, and deprecate explanations being given in accordance with principles previously well established."

Whether or not in what we have stated above we can fairly be charged with deprecating explanations given in accordance with principles previously well established, we leave to the judgment of our readers.

Since our earlier researches we have vastly improved the apparatus, and carried out numerous experiments, the results of which are embodied in three subsequent papers

* *Bristol Daily Post*, September 1, 1875.

read before the Royal Society. The radiometers first made are clumsy in the extreme compared with those we now use, and we have long discarded the use of instruments made of metal or talc, which substances, being used by Geissler, give rise to varying results. Is it not then a further injustice for Prof. Osborne Reynolds to attempt to explain away our results because they differ from those obtained by him in his few experiments with an instrument vastly inferior to that used by us? We are informed that at the meeting of the Literary and Philosophical Society, to which we have referred, he exhibited an apparatus to prove that the whole matter was a simple mistake of ours, and that what we thought to be an effect of light was merely an effect of heat, and that he had demonstrated by this apparatus that the heat generated by the impulsion of the light upon the vanes of Geissler's light-mill was sufficient as heat to produce all the effects we have obtained.

Our correspondent says the description was not so clear as it might have been, and he is not certain that he has correctly described Prof. Osborne Reynolds's present theory. We shall therefore refrain from making any remark upon it until we are in possession of an authentic report, but from what we can gather Professor Reynolds seems to have arrived at an opinion which is contained in a paper communicated by us to the Royal Society some time ago, in which we entered very fully and *experimentally* into the question of light or heat in connection with these phenomena, and gave reasons for supposing that the rays of light falling on the blackened surface were lowered in refrangibility and converted into thermometric heat, the motion being connected with the radiation of this heat back again.

We have repeatedly stated that the subject is surrounded with mystery, and nothing gives us greater satisfaction than to know that other scientific men are working at it and endeavouring to elicit new facts. The great incentive for scientific men to prosecute original research should be the discovery and development of new truths and the elaboration of old ones. The cause of science will be best promoted by its workers being eminent for their candour and courtesy, as well as for their scientific attainments.

PAPYRUS EBERS:

THE EARLIEST MEDICAL WORK EXTANT.

By H. CARRINGTON BOLTON, Ph.D., of New York.

(Concluded from p. 92.)

THE recipes and prescriptions contained in this treatise are evidently collected from various sources, some of them being quoted from still more ancient writings. It bears internal evidence of having been used in the healing art, for the word "good" occurs in the margin in several places, written in a different handwriting from the body of the work, and with lighter coloured ink.

Ebers thinks the compilation was made by the College of Priests at Thebes, basing his conjecture partly on the locality in which it was discovered. The other great Egyptian Universities were located at Memphis, Heliopolis, Sais, and Chennu.

Ebers gives a synopsis of the contents of the entire work, and a literal translation of the first two pages of the roll, reserving a commentary and fuller translation for a future publication. A hieroglyphic translation of a portion of the Hieratic manuscript also accompanies the plates; the latter, 107 in number, are faithful and beautiful reproductions of the original papyrus, in the same yellow-brown colour. The second volume contains a Hieroglyphic-Latin Glossary by Stern. Before proceeding to give details of its contents, one more peculiarity is worth mentioning. Though the pages are carefully numbered, the figures 28 and 29 are omitted, while the

text is continuous. Ebers conjectures that the writer either accidentally forgot his count, or abstained from using these numbers for superstitious reasons, the discussion of which we cannot here enter upon.

As already remarked, the work is divided into chapters or sections. We cannot give Ebers's synopsis in full, but a fair insight into the character of the treatise may be obtained from the selected headings of sections, and extracts here following:—

Contents of Papyrus Ebers.

Headings of chapters (selected).

The numbers refer to the pages of the papyrus.

1. Of the preparation of medicines.
25. Of salves for removing the *uhau*.
47. Catalogue of the various uses of the *Tequem* tree.
48. Medicines for curing the accumulation of urine and diseases of the abdomen.
55. The book of the eyes.
65. Medicaments for preventing the hair turning grey, and for the treatment of the hair.*
66. Medicines for forcing the growth of hair.
79. Salves for strengthening the nerves, and medicines for healing the nerves.
85. Medicines for curing diseases of the tongue.
89. Medicines for the removal of lice and fleas.
91. Medicines for ears hard of hearing.
99. The Secret Book of the Physician. The science of the beating of the heart, and the science of the heart as taught by the priestly physician Nebsecht.

Ebers encountered immense difficulties in the work of deciphering this papyrus; as an example of the obstacles met, he gives the following literal translation of a diagnosis beginning on Plate XXXVI., line 4:—

"Rules for the *re het*, that is, suffering in the pit of the stomach (pylorus or cardia.) When thou findest anybody with a hardening of his *re het*, and when eating he feels a pressure in his bowels (*chet*), his stomach (*het*) is swollen, and he feels ill while walking, like one who is suffering from heat in the back, *tau nu peht*, then look at him when he is lying outstretched,* and if thou findest his bowels hot and a hardening in his *re het*, then say to thyself, this is a liver complaint, *sepu pu n merest*. Then make thyself a remedy according to the secrets in botanical knowledge from the plant *pa chestet* and from scraps of dates. Mix it and put it in water. The patient may drink it on four mornings to purge his body. If after that thou findest both sides of his bowels (*chet*), namely, the right one hot, and the left one cool, then say of it: That is bile. Look at him again, and if you find his bowels entirely cold, then say to thyself: His liver (? *merest*) is cleansed and purified; he has taken the medicine, *sep nef sep*, the medicine has taken effect."

The following is the translation of the first four lines of Plate I.:—

"The Book begins with the Preparation of the Medicines for all portions of the body of a patient. I came from Heliopolis with the Great Ones from *Het aat* the Lords of Protection, the Masters of Eternity and Salvation. I came from Sais with the mother-goddesses who extended to me protection. The Lord of the Universe told me how to free the gods from all murderous diseases."

The work abounds in prescriptions, of which the following are samples:—

"Beginning of the Book of Medicines. To remove illness from the stomach.

"Rub up the seed of the Thehui plant with vinegar, and give the patient to drink.

* Verily, "there is nothing new under the sun" (Eccl. i. 9); hair invigorators, hair dyes, pain killers, and flea powders, were evidently fashionable 3400 years ago!

† It is curious to note here, that (according to Duglison) Diodorus states, the priestly physicians of Egypt formed their diagnosis principally on the position which the patient assumed in bed.

"The same for sick bowels.

Caraway seed $\frac{1}{4}$ drachm.
Goose fat $\frac{1}{8}$ "
Milk I tenat.
Boil, stir, and eat.

The same.

Pomegranate seed $\frac{1}{8}$ drachm.
Sycamore fruit (?) $\frac{1}{8}$ "
Beer I tenat.

"Treat as above."

In the original, the arrangement of the substances and quantities in two columns is the same as here given. The weights are written in red ink.

Other prescriptions contain reference to pills made by mixing certain substances with honey and rolling them into little balls.

The weights and measures in this unique work deserve a more lengthy notice than space will permit. A series of special signs indicate measures of volume, and figures with dots above them represent weights. The unit of weight employed is believed by Ebers to bear a close relation to the later Arabic Dirhem or Drachm, which is equivalent to about 48 English grains. But owing to the smallness of the quantities given in the recipes, the unit is probably double the drachm in value. This unit and its divisions are represented in hieroglyphics thus:—

One di-drachm



One-eighth di-drachm



One-sixteenth di-drachm



One thirty-second di-drachm



One sixty-fourth di-drachm



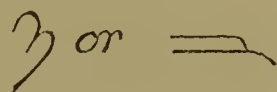
The fractions $\frac{1}{8}$, $\frac{1}{16}$, $\frac{1}{32}$, $\frac{1}{64}$, always recurring, and $\frac{1}{10}$ predominating, a quaternary arrangement which was superstitiously regarded as beneficial.

The unit of volume is thought to be the *tenat*, which is equivalent to six-tenths of a litre. This unit and its subdivisions are represented by arbitrary signs of which the following is an attempt at reproduction:—

Unit or tenat



$\frac{1}{2}$ "



$\frac{1}{3}$ "



$\frac{1}{4}$ "



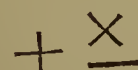
$\frac{1}{2} + \frac{1}{3}$ or $\frac{5}{6}$ "



$\frac{1}{3} + \frac{1}{3}$ or $\frac{2}{3}$ "



$\frac{1}{3} + \frac{1}{4}$ or $\frac{7}{12}$ "



$\frac{1}{4} + \frac{1}{3}$ or $\frac{3}{4}$ "



When equal parts of the components of a prescription are taken it is indicated by a light, short, vertical dash, placed opposite each substance thus, I.

The writer failed to detect any sign at the beginning of the several recipes equivalent to the \mathcal{R} now used by physicians, and which, though generally regarded as the initial letter of the Latin word Recipe = take, has also been referred to an Egyptian source. This \mathcal{R} is said to have been originally the same as the symbol of Jupiter \mathcal{J} , and to have been placed at the beginning of formulæ to propitiate the King of the Gods that the compound might act favourably.

The symbols in common use for scruples \mathfrak{S} , drachms \mathfrak{D} , and ounces \mathfrak{O} , are said to have been derived from inscriptions on the ancient monuments of Egypt, and the resemblance of our sign for drachm to that of the $\frac{1}{2}$ *tenat*, is certainly very striking.

Ebers states in his Preface, that notwithstanding there are to be found in this great work many incantations and conjurations, from which the priestly physicians could not abstain, still there is no hocus-pocus nor gibberish in it; on the contrary, it shows that it was possible to write in the 16th Century B.C., complex recipes, and that they understood how to administer with care the medicines prescribed. Moreover, sorcery was forbidden in ancient times in the strongest manner, and the alchemistic Magi were punished in the reign of Rameses III. with death. The art of the physician was lost in the post Christian era. Science became more and more tinged with magic, and was gradually obscured and degraded by it.

We cannot do better, in conclusion, than to quote the testimony of the learned Librarian of the Astor, with reference to the intrinsic value of this papyrus; he says.—

"It is hardly possible to exaggerate the literary, scientific, and historical importance of this remarkable document. It is the largest, best preserved, and most legible text in the language of Hieroglyphics, and does not speak vaguely of incomprehensible and fantastic ideas, but furnishes indubitable insight into different phases of the life of the ancient Egyptians."

DETERMINATION OF VERY SMALL AMOUNTS OF COPPER.

By J. M. MERRICK, B.Sc.

IN the researches of M. Bergeron and l'Hôte,* upon the occurrence of very minute quantities of copper in the human organism, these *savants* employed a colorimetric test based upon the blue tint which ammonia gives with salts of copper, and constructed a scale of colours, showing from 2 m.grms. down to 0.5 m.grm., and state that if the quantity of copper is less than 0.5 m.grm. the colour method fails to indicate it, but yellow prussiate of potash still gives the characteristic red colouration.

I would here add to this statement the outline of a gravimetric method which, in very numerous trials, has led, in my hands, to what I consider trustworthy results, even when the amount of copper present was very much less than 0.5 m.grm.

It consisted simply in concentrating to a very small bulk the solution suspected to contain copper, and then depositing the copper, if present, upon platinum, by the battery. I used for a depositing cell a very small test-tube, on a foot cut off, so as to give a vessel about $1\frac{1}{2}$ inches deep. Into this was introduced the solution acidified with sulphuric acid, and a platinum anode and cathode—each about 1 inch long, and $\frac{1}{8}$ th of an inch or less wide—were hung face to face, and very close together, and, the circuit being completed, very satisfactory depositions of copper were obtained with incredibly minute quantities of

* *Comptes Rendus*, lxxx., p. 268.

the metal. The amounts were determined by the increased weight of the cathode (which was provided with a platinum wire soldered on with gold, by which it was conveniently hooked on to the balance), and by the loss in weight of the same on washing it with nitric acid. The platinum was polished and heated red-hot before the first weighing, and then gently heated before hanging in the solution. The contrast in colour between deposited copper and bright platinum is, of course, striking and characteristic. In this way 0.1 m.grm. of copper may be, I believe, safely determined, while for mere qualitative analysis this method may be employed where the amount is even smaller.

Laboratory, 59, Broad Street, Boston, U.S.A.,
February 14, 1876.

REPORT

ON THE

DEVELOPMENT OF THE CHEMICAL ARTS DURING THE LAST TEN YEARS.*

By Dr. A. W. HOFMANN.

(Continued from p. 91.)

BEING cooled to the temperature of spring-water it passes into an expansion piston-machine, where it yields power in the same manner as steam. In consequence of the great expansion there is a considerable fall of temperature (see below), sometimes reaching -20° to -30° . The gas as it escapes from the machine can be used for cooling any article, for producing ice, and, finally, it may serve for the manufacture of soda-water. The machine shown at the Exhibition was two-horse power. The cost of the gas is said to be quite covered by the value of the copperas produced; 1 cwt. sulphuric acid (costing 5 florins Austrian) and 1 cwt. iron-spar (1 florin 50 kreutzers, together 6 florins 50 kreutzers) yielding 240 lbs. copperas (at 3 florins per cwt.), worth 7 florins 20 kreutzers. The idea of this combination is doubtless ingenious, and in individual cases it may prove remunerative in practice, but for general application it is not suitable. One cwt. iron-spar yields about 20 kilos. carbonic acid, which, at a pressure of 5 atmospheres, represent 2 cubic metres. On expansion to 1 atmosphere this quantity can theoretically produce at most a working-power of 170,000 kilogram-metres, equal 1 horse-power for about half-an-hour. By expansion the gas loses about 200 heat-units, and at the outside the same amount is available for the production of ice, yielding not more than 2 kilos. Hence it is plain that exceedingly large quantities of materials are requisite, and that it would be difficult to find a sufficient demand for the copperas. The escaping carbonic acid, also, can be but very partially utilised in the manufacture of soda-water, and would require for this purpose to be compressed anew, so that it would appear more advantageous to pass it at once into the water as it issues from the generator.

If the carbonic acid is used for making soda-water as it issues from the ice-receiver, the absorption would take place at about -5° , under a pressure of 33 atmospheres, or about six times more than is necessary. The power which the machine must exert to produce this superfluous pressure is totally wasted. The latent vapour-heat of carbonic acid is certainly not yet known, but even if it were high (which, from the great specific gravity, is improbable) enormous quantities of carbonic acid would be required to produce only a moderate yield of ice, far larger than there is any prospect of utilising in the manufacture of effervescing beverages. We can therefore prognosticate no success for the above-mentioned apparatus, and consider that the use of carbonic acid in a circulatory process would be more rational.

The Ammonia Machine.

At common temperatures ammonia is a gas; under pressure it may be condensed to a liquid. The temperature and pressure of liquid ammonia, according to Regnault, are respectively as follows:—

Temp.—	$+40^{\circ}$	$+20^{\circ}$	0°	-20°	-30°	-40°	C.
Pressure—	15.5	8.5	4.4	1.84	1.16	0.7	atmos.

Ammoniacal gas is readily soluble in water, which at 0° it saturates with 1050 volumes, or 0.875 in weight. At 20° water absorbs 654 vols., or 0.52 of weight, a little more than half its own weight. In this state it forms the liquid ammonia of commerce. The absorption is attended with a considerable rise of temperature, whence the latent heat of ammoniacal gas may be calculated as 500° , or close upon that of steam. The gas absorbed by water can be entirely expelled by the application of heat. A decrease of pressure has the same effect, in which case the temperature falls.

(To be continued.)

ON THE ACTION OF METALLIC MAGNESIUM ON CERTAIN METALLIC SALTS.

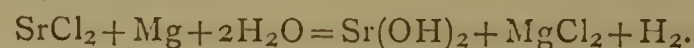
By SERGIUS KERN, St. Petersburg.

IN my paper inserted in the CHEMICAL NEWS (vol. xxxii., p. 309) I gave some preliminary remarks on the action of magnesium on some salts, viz., on cobalt chloride and silver nitrate. In this paper I will describe further experiments on the same subject.

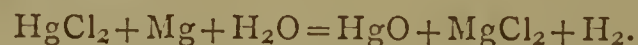
Concentrated solutions of salts were used (about 40 c.c. for every experiment): they were left in contact with magnesium ribbons, 0.05 metre long, for about thirty to forty hours.

1. In a solution of lead nitrate magnesium is quickly covered with metallic lead, in the form of a very fine powder, which is soon converted into lead hydrate $[\text{Pb}(\text{OH})_2]$.

2. Strontium salts require much more time to be decomposed. A concentrated solution of strontium chloride was in contact with magnesium for two days: a white precipitate was obtained, which was found to be strontium hydrate. The decomposition may be explained by the following formula:—



3. Mercuric chloride in contact with magnesium gives a greyish precipitate, which when ignited turns red. This precipitate is a third form of mercuric oxide, which has hitherto been known in two forms—as red and yellow precipitates. This third form is obtained during this reaction as follows:—



4. When magnesium is brought in contact with a solution of platinum chloride, hydrogen is evolved very rapidly. After about twenty to twenty-five hours a black precipitate was obtained, which proved to be a mixture of metallic platinum in finely divided state (platinum-black) and black platinum oxide (PtO_2). On leaving this precipitate for some hours in contact with water, the brown hydrate of platinum was obtained $[\text{Pt}(\text{OH})_4]$.

5. With a solution of ferric chloride magnesium is rapidly decomposed, and ferric hydrate is formed by the reaction:—



6. Magnesium with zinc salts evolves hydrogen very slowly. A solution of zinc chloride was used, and after forty-eight hours a small amount of zinc hydrate was obtained, owing to the decomposition of the zinc salt.

7. In a solution of sodium chloride magnesium also evolves hydrogen very slowly. This salt is converted

* "Berichte über die Entwicklung der Chemischen Industrie Während des Letzten Jahrzehends."

during this reaction into sodium hydroxide (HNaO). In such a solution red litmus-paper quickly turns blue.

These experiments show that most of the salts by the action of magnesium are transformed into their oxides or hydrates. It is very remarkable that such stable compounds as strontium chloride and sodium chloride are easily decomposed into their corresponding hydrates. The reaction of magnesium on mercuric chloride, yielding mercuric oxide in a new third allotropic form, is also very interesting.

If magnesium is ever prepared cheaply enough it will be a very good substitute for other metals in the construction of galvanic cells.

PROCEEDINGS OF SOCIETIES.

PHYSICAL SOCIETY.

March 11, 1876.

Professor G. C. FOSTER, F.R.S., President, in the Chair.

THE following candidates were elected Members of the Society:—W. H. Coffin, T. D. Humpidge, and the Rev. G. H. Hopkins.

Prof. W. G. ADAMS gave an account of some researches on which he has been engaged in connection with the influence of light and heat on the electric conductivity of selenium, and exhibited numerous experiments in illustration. The subject has also been studied by Lieut. Sale and Dr. W. Siemens, of Berlin, and as a general result it is found that, after it has been kept in the dark, the resistance of the metal is diminished by exposure to light. The effect, however, both of heat and light, is different in the several states through which the metal passes. Thus, when a piece of amorphous selenium is gradually heated to about 100° C., kept at this temperature, and slowly cooled, its resistance at first is so great that it cannot be measured by the ordinary arrangement; but as its temperature increases the resistance diminishes, and increases again more slowly when the metal is allowed to cool. The resistances of several pieces, which at the higher temperature were from 1 to 3 megohms, were found to be from 100 to 130 at the ordinary temperature. If this selenium be placed in a paraffin bath, and heated, its resistance diminishes, and when the temperature is kept constant above 140° C. for some hours, and the metal is then slowly cooled, it assumes a crystalline structure, and its resistance *diminishes* as it cools. The resistance of such selenium, at ordinary temperatures, *increases* with the temperature. The effect is more marked as the temperature of the paraffin bath is increased. In studying the effect of light, the metal—which had been heated to 140° C.—was exposed to a candle at distances of 1, $\frac{1}{2}$, and $\frac{1}{4}$ metre. The initial resistance being 115,500 ohms, the readings in these three cases were 112,000, 108,700, and 101,500. Deducting each from the initial number, we have 3500, 6800, and 14,000 ohms as the changes of resistance due to exposure at these distances. Hence the effect of light varies inversely as the distance, or, what amounts to the same thing, directly as the square root of the illuminating power. These considerations have led Prof. Adams to suggest the use of selenium for comparing the illuminating powers of different sources of light, and he exhibited the arrangement which he proposes to use for this purpose. The action of light of different degrees of refrangibility was then exhibited by allowing the light from several parts of a spectrum of the electric lamp produced by a bisulphide of carbon prism to fall on the metal, the remainder being cut off by means of a screen in which there was a narrow slit. The violet light gave a deflection of about two divisions on the screen, the greenish yellow four, the orange-red five and a half, and the deep red nine divisions.

The effects produced by the greenish yellow and the deep red are at times nearly equal. It may easily be shown by raising the temperature of the metal that the effect of light on its conductivity is essentially the same in kind at a low and moderately high temperature. The fact that light and not dark heat produces the observed effect has been shown by sending the beam through solutions of iodine in bisulphide of carbon, when a very small effect on the metal was always observed, but this may be assumed to have been due to light, as in all cases it was possible to see the form of the carbon points through the solution. This fact may also be strikingly shown by exposing selenium, through which a current is passing, to the flame of a Bunsen burner, first, when in its ordinary condition, and afterwards with the air openings at the base closed. It was shown that, whereas in the first case the effect produced was equivalent to three divisions of the scale, in the latter case one-tenth of the current produced by the exposure deflects the needle to the end of the scale.

Prof. STONEY pointed out that the action of light on selenium increases with the amount of heat accompanying that light. He suggested that the photographic effect of selenium might have some relation to the action under consideration.

Prof. BARRETT thought that a solution of iodine in bisulphide of carbon might give more satisfactory results, for this liquid is more opaque than a solution in the bisulphide.

Prof. GUTHRIE enquired whether the action on the metal is superficial or extends into the mass. If a definite answer could be given to this question it seems likely that we might determine whether electricity flows on the surface of a conductor or is uniformly distributed throughout the mass; for if we have an *opaque* substance, the conductivity of which is influenced by light, it would suggest that the former is the case. The question, however, would be very difficult of solution, but perhaps the variations in resistance in two bars of selenium, which only differ in diameter, might throw some light on the subject.

Prof. ADAMS, in replying to the various questions, mentioned that he has examined the effect of light deprived of the ultra-chemical rays, but the change seemed to have no effect. He has also made experiments on the influence of light after passing through glasses of different colours, but there is considerable difficulty in ascertaining how much of the effect is due to the degree of refrangibility, and how much to the change in the intensity of the light. The effect is most intense in the greenish yellow and the deep red, and it is worthy of remark that at these two points groups of bands are observed in the spectrum of the metal. He compared the effects observed with phosphorescent effects, in which case there is certainly some superficial action. The conductivity may be improved by some alteration of the surface, but Prof. Adams has suggested that light may modify the molecular condition of the metal so as to call into play something of the nature of electromotive force. He showed an experiment which proved that if the resistance of a bar of selenium is measured with a given current passing along it in one direction, then, when this current is reversed, the resistance is entirely different. This seems to bear some analogy to electrical polarisation, only the action is in an opposite direction, for it appears to be such as to help the current rather than oppose it.

Prof. JOHNSTONE STONEY then explained the theory which he has suggested in explanation of the phenomena observed in the radiometers of Mr. Crookes, which has been published in the *Philosophical Magazine* for the current month. The theory rests on the supposition that there is an excessively small trace of residual gas in the sphere in which the moving discs are enclosed. When the apparatus is exposed to heat the blackened side of the disc is slightly warmed, and this warms a layer of air in contact with it. At the ordinary atmospheric pressure, Prof. Stoney assumes the layer so warmed to have the thickness of a sheet of paper, when the temperature of the

disc is 20°C . above that of the surrounding air, and on such a supposition we may calculate it for any other pressure and temperature. If we diminish the pressure the thickness varies inversely as the pressure raised to the power $\frac{4}{3}$. Thus, if the disc be raised $1\text{--}10^{\circ}\text{C}$. above the surrounding air, and the exhaustion carried to the $1\text{--}10,000$ th of an atmosphere, the layer will have a thickness of more than a decimetre, and the effect of the air will then be peculiar. If the gas is of such a density that the glass envelope is beyond the range of this action, the gas beyond the limiting distance will be cold, but if the effect reach the glass conduction will take place to it. There will then be a procession of warm molecules towards the glass, where they will be cooled down, and form a number of cold slow-moving molecules, which will go back to the disc and beyond it, and these processions will be intermixed with molecules taking no part in the action. In consequence of this, very few members will travel far in their paths; a portion of the motion of heat, however, will be carried forward in the right direction. So long as these processions go on, the slow-moving molecules which reach the front of the disc are thrown off more vigorously than from the back. Prof. Stoney considers the pressure thus produced to be that measured by Mr. Crookes. With a pressure of the gas of $1\text{--}10,000$ th of an atmosphere, an elevation of temperature of $1\text{--}10^{\circ}\text{C}$. will produce the force actually observed; while, if the exhaustion be carried to $1\text{--}100,000$ th, the elevation of temperature necessary will be $8\text{--}10^{\circ}\text{C}$. Thus, with the greater pressure, a lower temperature will suffice, but other influences will then be brought into play, tending in an opposite direction. It was pointed out that on this theory the action may be considered as closely resembling electricity, and Mr. Crookes has shown that the glass envelope is often itself slightly electrified.

Mr. CROOKES said that Prof. Stoney assumed more than the facts would warrant. He considered that $1\text{--}10^{\circ}\text{C}$., the temperature assumed by Prof. Stoney, was far in excess of what actually is required to produce the observed effects, for the discs are strongly moved by light free from heat falling on them; for example, when placed in front of a window behind screens of water or alum. With discs of platinum there is attraction throughout all the stages in the exhaustion from 1 m.m. of mercury until a fairly good vacuum is attained. If, however, the exhaustion is continued a neutral point is reached, beyond which there is strong repulsion, and this is strongest in the very best vacuum which will not conduct an induced spark. Moreover, the assumed exhaustion of $1\text{--}10,000$ th of an atmosphere was much inferior to what he had often experimented with. Prof. Stoney would seem to show that the action should vary with the size of the apparatus, but this is not found to be the case. Mr. Crookes enquired whether the extremely rapid rotation of the disc would not interfere with the permanent difference of temperature between the front and back surface which was required to produce the motion on Prof. Stoney's hypothesis.

Prof. STONEY pointed out the enormous velocity of the molecular motions in proof that the rotation of the discs would be without influence. Thus, in such a gas as oxygen, the velocity of the molecules is about 500 metres per second. The fact that similar velocities of rotation are observed in radiometers of different diameters only shows that they are all within the limits of the thickness of the layer, which he considers to produce the effects observed.

NEWCASTLE-UPON-TYNE CHEMICAL SOCIETY.
General Meeting, January 27th, 1876.

JOHN PATTINSON, President, in the Chair.

THE minutes of previous meeting were read and confirmed.

Mr. W. F. Henderson was elected a member of the Society.

The name of Mr. R. W. P. Richardson, of Langley Park Colliery, was read for the first time. Professor ALDIS read a "*Note on the Theory of Balances.*"

The PRESIDENT read the following paper:—"On Gas-Burners. How best to burn coal-gas so as to obtain the greatest amount of light it is capable of producing is a subject which is perhaps as yet but imperfectly understood, even by those who know most about it. Great improvements have, however, been made in recent years in the construction of gas-burners, resulting from the researches of Frankland, Bowditch, Sugg, Silber, the London Gas Referees, and others; but from many causes, chiefly perhaps for want of information on the subject amongst the general public, these improved burners have come but very slowly into use. The gas-fitters, to whom the choice of burners is usually left, often know as little about the subject as any one, and thus a wretchedly bad kind of burner is in many cases used which burns the gas so as to destroy a great portion of its light-giving power. In the present paper no attempt is made to describe even a tithe of the multiplicity of burners which have been invented. The object is simply to describe the results of an examination I have recently made of a number of burners taken off fittings in various parts of Newcastle, and to show how unfit these burners are to develop the light-producing properties of gas as compared with good burners, which may be had quite as easily as the bad ones.

Gas burners may be divided into two great classes: first, argands, such as give a ring of light and require glass chimneys; and, second, flat-flame burners, which may be sub-divided into what are known as "batwings" and "fishtails," so called from the shape of the flames they produce.

In the experiments I have made, the gas used was of the same quality throughout the whole of the tests. It gave a light equal to 14.1 standard sperm candles when burnt at the rate of five cubic feet per hour in the standard argand burner, known as the Sugg-Letheby burner. Before about seven or eight years ago this was the best burner known, and as it was in use at the time when the Newcastle and Gateshead Gas Company obtained their Act of Parliament, it is still retained as the standard by which the gas is tested. It is but fair to state here that the quality of the gas was unusually low when the experiments were made, the gas usually being equal to about fifteen standard candles. It may be explained for the benefit of the uninitiated that the standard sperm candle with which the gas is compared is one defined in the Act of Parliament as a "sperm candle of six to the pound, burning at the rate of 120 grs. per hour."

I will first of all give the results of the testing of argand burners. The argand burner when properly constructed is the one best adapted for obtaining the greatest amount of light from common coal gas. The air supply, upon which so much depends, is most perfectly under control in such burners. Argand burners are certainly more expensive in their first cost, and perhaps are more troublesome to keep in order than flat flame burners, owing to the occasional breakage of chimneys and other causes, but it will be seen that the amount of light they can produce in comparison with the latter is very great, and would soon compensate for their extra cost and the extra trouble of keeping in order, especially in private houses. They require the gas to be supplied to them at a uniform pressure, otherwise they are liable to smoke when the pressure is increased, and more gas passes through than they can burn. They should, therefore, always be used with regulators for regulating the pressure of the gas. These may be conveniently placed near the supply meter.

In each of the following tables the first column shows the actual amount of gas consumed per hour in cubic feet during the experiments; the second gives the illuminating power of the gas consumed expressed in standard sperm candles; the third gives the illuminating power calculated per five cubic feet of gas:—

Argand Burners.

Kind of Burner.	Cubic feet of Gas used per hour.	Illuminating power in Candles.	Illuminating power per 5 Cubic feet of Gas per hour.
Sugg-Letheby Standard ..	5.0	14.10	14.10
Sugg's "London Argand"	5.0	15.90	15.90
Sugg's "Improved London Argand"	4.5	16.08	17.86
Silber's Argand	5.0	17.80	17.80
Common Argand	5.0	11.20	11.20
"	7.0	17.80	12.70

The common argand here mentioned was one similar to a great number used in shop windows in Newcastle. It will be seen that with the Sugg's Improved London Argand and Silber's Argand as much light is produced when burning five feet of gas as is produced when burning seven feet of gas in the common argand, thus showing a saving of 40 per cent over the last-named burner.

Flat flame burners do not consume the gas so as to give as much light from a given quantity of gas as argands. This is perhaps chiefly owing to the circumstance that it is necessary to make the gas issue from the burner under a certain amount of pressure, in order to produce the required shape of flame, and this pressure causes a portion of the gas to mix with too much air, so that it becomes over-burnt, as in the Bunsen burner. The best flat flame burners are those in which this pressure is reduced to the minimum amount found necessary, and such burners generally have some contrivance for checking the pressure of gas supplied at the mains. But although flat flame burners are not so economical as argands in their consumption of gas, yet they possess other advantages which probably will always cause them to be extensively used. The first cost of the burner is very trifling, no chimneys are required, they are not so liable to smoke under varying pressures of gas as argands, and they are easily lighted and attended to.

It is in this class of burner that the most extraordinary differences in quality for burning gas so as to produce light have been found. The following table shows the results of my tests of various new batwing and fishtail burners which can be had in Newcastle. Some of the burners were tried with different consumptions of gas.

New Batwing and Fishtail Burners.

Kind of Burner.	Cubic feet of Gas used per hour.	Illuminating power in Candles.	Illuminating power per 5 Cubic feet of Gas per hour.
Bray's No. 4 fishtail ..	4.0	5.02	6.28
"	5.0	5.80	5.80
Bray's No. 6 fishtail ..	5.0	9.00	9.00
Bray's No. 8 fishtail ..	5.0	11.80	11.80
"	7.0	14.21	10.15
Brönner's No. 4 batwing	4.0	10.10	12.62
"	5.0	11.60	11.60
Sugg's No. 4 batwing ..	4.0	8.40	10.50
"	5.0	10.90	10.90
Batwing with narrow slit	5.0	8.60	8.60
Batwing used in street lamps	5.0	11.60	11.60
Fishtail with small platinum disc in flame ..	5.0	10.20	10.20
No. 5 fishtail—metal top	5.0	7.50	7.50

It is difficult to get exact uniformity in the same kind of burner of each manufacturer, and the above burners may be inferior to others of the same kind. They were, however, selected at random. The results show that the best argand burners produce upwards of one-third more light from the gas than the best of the flat flame burners, so that, taking light-producing properties alone into consideration, there is an advantage to this extent in favour of argand burners. Bray's, Brönner's, and Sugg's are all good burners. It will be seen that Bray's No. 8 fishtail is much better adapted for burning the kind of gas experi-

mented with than the No. 4 and No. 6 fishtail of the same maker.

The next table gives the results of my trials of various burners recently taken from fittings in houses in several parts of Newcastle.

Burners in Use in Newcastle.

Kind of Burner.	Cubic feet of Gas used per hour.	Illuminating power in Candles.	Illuminating power per 5 Cubic feet of Gas per hour.
Batwing—metal top ..	5.0	9.26	9.26
" double }	5.0	8.90	8.90
slit }			
Fishtail No. 3—metal top	3.5	4.10	5.85
" steatite top	3.7	4.40	6.00
" "	4.0	3.00	3.75
" No. 4— ..	4.9	5.20	5.31
" No. 5— ..	5.0	7.80	7.80

It thus appears that burners are in use in Newcastle which give a light equal to only $3\frac{3}{4}$ candles, when burning the same quantity and quality of gas as, when burnt in a good argand, will give a light equal to $17\frac{3}{4}$ candles, or, if burnt in a good burner of its own class, will give a light equal to $12\frac{1}{2}$ candles, and such burners are very extensively used in Newcastle. Is it, then, a matter of wonder that we occasionally hear complaints of the bad quality of Newcastle gas? By the substitution of good burners for these wretched ones, from three to five times the amount of light will be produced from the same amount of gas; or, if the light of the present burners is sufficient, the same amount of light can be obtained from one-third or one-fifth the amount of gas. In the latter case, besides the saving in cost of gas, there is also the additional advantage of having the vitiation of the air in the room by sulphur compounds and carbonic acid from the burning gas reduced to the same extent.

It is in the hope that public attention will be called to this matter, and the wasteful misuse of gas prevented, that these results are now published.

Mr. FREIRE-MARRECO proposed a vote of thanks to the President.

Dr. LUNGE seconded the motion, which was carried, and the discussion was adjourned until the next meeting.

Mr. M. W. BULLEN exhibited and described the occurrence of specimens of Pectolite, and of supposed Prehnite and Rutile, from Teesdale, for which the thanks of the meeting were offered to him.

Mr. CROOKES's "Radiometer" was exhibited in action.

CORRESPONDENCE.

MR. STEWART'S ANALYSIS OF TELL-TALE LIQUOR FROM SUGAR VACUUM PANS.

To the Editor of the Chemical News.

SIR,—Noticing an article some time ago in the CHEMICAL NEWS (vol. xxxiii., p. 5), entitled as above, by G. C. Stewart, F.C.S., I would like very much, through your valuable columns, to say a few words on it *pro bono publico*.

In the first place, Mr. Stewart has analysed the two sugar-liquors (it being the same liquor, but boiled in two pans), and states that the mechanical construction of the pan causes the one liquor to have more cane-sugar destroyed than the other; therefore the long-necked pan, which produced the worst liquor, is almost, I may say, condemned (by Mr. Stewart).

Again, the "salt-water" used in condensing the vapours sometimes gets down more roads than the tube it is bound for—"accidentally," "peradventure," or "otherwise"—and getting mixed with the sugar-liquor in the tell-tale

safe, I should say this itself destroys at least four times its bulk of cane-sugar.

But Mr. Stewart is not satisfied with telling us all this and the above reasons. He further states that a chemist in charge of a sugar refinery can tell by a continual examination of these liquors whether a panman handles his pan in a proper manner: and in this quiet way he gives us his third (as it seems) reason for the deficiency of cane-sugar in the liquor produced from the long-necked pan. I think in this Mr. Stewart has drawn his line of imagination too far away from "good common sense," which gives his imaginative powers too large a space to work upon; consequently a muddle is the result. But, however, after the liquor has been reduced (in cane-sugar) by "mechanical construction" and "salt-water," I think it hardly fair for Mr. Stewart to say that he could tell whether a pan was handled in a proper manner or not. But what I wish to know is, does Mr. Stewart really blame the panmen for the deficiency in his results in cane-sugar? if so, then his best plan would be to take an average of his three reasons—

"Mechanism,"
"Salt-water," and
"Humanity."

I hope either to see the average, the individual reason, or to say whether the three combined cause the deficiency in cane-sugar of the liquor drawn from the "tell-tale safe" of the long-necked sugar vacuum pan.—I am, &c.,

THOMAS MCKEAN.

PRODUCTION OF ANILINE-BLACK BY MEANS OF VANADIUM SALTS.

To the Editor of the Chemical News.

SIR,—In reply to Mr. Higgin's remarks (CHEMICAL NEWS, vol. xxxiii., p. 86) on my patent for vanadium and aniline taken out five years ago, I have to state that I discovered the extraordinary action of salts of vanadium on aniline as early as 1868 and 1869, and made many *successful* experiments in producing aniline-black with these salts. But the very high price and scarcity of vanadium at *that time* prevented me from introducing it commercially. This difficulty is now remedied by the enterprise of Messrs. Johnson, Matthey, and Co., Hatton Garden, who are prepared to supply the various vanadium salts at a comparatively moderate price, for the use of printers and dyers. After taking out my patent I noticed, in Mr. Lightfoot's work on aniline-black, his very interesting laboratory experiment of simply placing pieces of the *rare metals* on cloth saturated with aniline salt and chlorate of potash, with the result that the *metal vanadium* had the powerful effect.—I am, &c.,

ROBT. PINKNEY.

ORGANISATION AMONG CHEMISTS.

To the Editor of the Chemical News.

SIR,—It seems to me that the benefits of the proposed organisation among chemists could be attained without the Guild which was suggested by Dr. Wright, and supported by those of your various correspondents who have interested themselves in this subject, by means of an Act of Parliament permitting an incorporation of recognised chemists, such as the professors of chemistry at the various hospitals, the Doctors of Science who have gone out in chemistry at the University of London, and the Examiners in Chemistry at the various Universities, the Science and Art Department, &c. (similar to that obtained by the medical circle), with powers to grant diplomas in chemistry, after due examination of candidates for them. This body, when incorporated, might be recognised as the "Royal College of Chemists." The institution of various degrees or grades similar to the various diplomas of physicians and surgeons, &c., could then be effected.

The degrees granted in chemistry by the London University would, by a clause in the Act, then confer a legal status precisely as they do now in divinity, medicine, &c., which status would, of course, be conferred by either of the diplomas of the "Royal College of Chemists," or of any other qualified examining body. It would be advisable that the various Universities now permitted to grant diplomas in medicine should be empowered to confer diplomas in chemistry. The possession of any such diploma would enable its holder to recover fees at law, which, without it, he should not be permitted to do; the giving of evidence as experts in courts of law should also be confined to the possessors of diplomas. Some revenue might accrue to the Government by a license, to be renewed annually, similar to that required by solicitors. The public would soon learn the difference between a "qualified" chemist and a "non-qualified" one, and the latter race would die out utterly in a brief space.

The series of diplomas for chemistry might thus stand when placed in juxtaposition to their equivalents in medicine:—

Chemistry.		Medicine.	
University of London.	{ Sc.D. Sc.B.	University of London.	{ M.D. M.B.
Royal College of Chemists.	{ F.R.C.C. M.R.C.C.	Royal College of Physicians or Surgeons.	{ F.R.C.P. or S. M.R.C.P. or S.

Possibly a greater variety of diplomas would hardly be necessary, but that is a mere matter of detail.

There still remains a question of some considerable interest and difficulty which would have to be solved, namely, what to do with those chemists who have justly earned a position and a reputation, but who have not either degree of the University of London. Should they be granted an honorary degree, say of M.R.C.C.? Should they all be made to pass a full examination? Should they be "admitted" on passing a modified examination? In the first or third case who should decide which of the persons calling themselves chemists were worthy of the honorary diploma, or of admission on passing a modified examination?

Can we not hold a provisional meeting, say at the Scientific Club, to discuss our projects and our prospects.—I am, &c.,

CHARLES H. PIESSE.

303, Strand, London, March 11, 1876.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Seances de l'Academie des Sciences. No. 8, February 21, 1876.

Rotatory Power of Styrolen.—M. Berthelot.—The author announces that in consequence of the paper by M. van't Hoff he has repeated his experiments with pure material, volatile at 147°, and agreeing on analysis with the formula $C_{16}H_8$. The rotatory power, with reference to the sodium light, was found—

First specimen $\alpha_D = -3.1^\circ$

Second specimen $\alpha_D = -3.4^\circ$

The slight difference he considers due to the formation of a little inactive styrolen during the rectification.

Coefficient of Dilatation of Air under Atmospheric Pressure.—M. Mendeleeff and Kaiander.—A mathematical paper, not adapted for abstraction.

Certain Remarkable Points in Magnets.—M. Jamin.—If a very short magnetic needle, supported at its centre of gravity, is moved near the surface of a magnet the

direction of this needle is seen to vary at the same time as the co-ordinates of its centre of rotation. Among these directions there are some deserving attention, those which are normal to the surface of the magnet. The author terms the points with which they correspond the orthogonal points, and finds that they possess the following properties:—If in an orthogonal point we place a small magnetic body, in order to remove it from its place to an infinite distance more mechanical power is required than if it had been placed on any other adjoining point of the surface of the magnet. Another curious property is as follows:—The positions of spontaneous equilibrium of a small magnetic body with relation to a magnet are exactly the orthogonal points.

Composition of the Black Matter obtained on Calcining the Ferrocyanide of Potassium.—M. A. Terreil.—This substance is not a carbide of iron of definite composition, but a mixture containing finely-divided iron in the state of cast-iron, magnetic oxide of iron, free carbon, and a small amount of cyanide of potassium, which cannot be removed by washing.

Formation of Anhydrous Acids of the Fatty and of the Aromatic Series by the Action of Phosphoric Acid upon their Hydrates.—MM. H. Gal and A. Etard.—If the acid is mixed in a retort with anhydrous phosphoric acid, the phosphoric acid turns slightly brownish, and the mixture heats. On rapid distillation the anhydrous acid passes over.

Products of the Action of Chloride of Lime upon the Amines.—M. J. Tschermak.—Not adapted for abstraction.

Bulletin de la Société d'Encouragement pour l'Industrie Nationale. No. 26, February, 1876.

Report made by M. Lamy, on behalf of the Committee of Chemical Arts, on the "Tank Apparatus" for Concentrating Sulphuric Acid to 66° B., Designed by MM. Faure and Kessler.—This invention has been already noticed (see CHEM. NEWS, vol. xxxiii., pp. 55, 95).

Note on Charcoals used for Decolourising, their Artificial Production, and the Regeneration of Blacks Employed in the Arts.—M. Melsens.—The author has made a series of comparative experiments on the absorption and condensation of dissolved bodies, including colouring matters from an industrial point of view. He considers that chemically pure charcoals have no decolourising action. Bone-black, or animal charcoal, is solely employed in the arts, as it has a well-marked power of absorbing either colouring matters, or salts of lime, and any excess of lime contained in saccharine liquids. The high price of bone-black has led to many attempts to find a substitute, but these experiments have not been successful. The author doubts whether the decolourising action of bone-black is purely physical, as commonly supposed, and suggests that chemical processes may also be concerned.

Moniteur Scientifique, du Dr. Quesneville,
February, 1876.

Analysis of Mixtures of Mono- and Bicarbonates.—A. Mebus.—Two equal portions of the mixture are weighed out, and in one of them the total alkali is determined by means of a standard acid. Into the solution of the other portion is poured a standard solution of caustic soda, perfectly free from carbonic acid, and corresponding in quantity with the alkali just found to be present,—i.e., as many equivalents of alkali are added as are already contained. Of the alkali thus added, one portion combines with half the carbonic acid of the bicarbonate, and the remainder—which is precisely equal in quantity, or equivalent (according as the base added is identical with or different from the base of the salts under examination) to the alkali of the monocarbonate—remains in a free

state in the liquid. This solution is then precipitated with an excess of chloride of barium; the carbonates are thus eliminated as carbonate of baryta, and after filtration there remains in the filtrate merely the excess of chloride of barium, chloride of sodium, and a quantity of free caustic soda equivalent to the alkali of the monocarbonate. All that remains is to take a known fraction of this liquid, and determine the alkali in the usual manner. We have thus the alkali of the monocarbonate, and by subtracting it from the total alkali we find the alkali of the bicarbonate.

New Method for the Volumetric Determination of Silver.—J. Volhard.

Determination of Chlorine, Bromine, and Iodine in Organic Compounds.—E. Kopp.—The author takes a tube, 60 c.m. long and from 5 to 6 m.m. in internal diameter, and closes one of its ends at the lamp. To regulate more easily the decomposition of the organic matter it is mixed with pure oxide of iron, prepared by calcining the crystalline sulphate. This mixture is introduced without letting it heap up in the tube, where it ought to occupy a portion of 12 to 18 c.m. in length. The tube is rinsed with pure oxide of iron, which is introduced after the mixture. A number of narrow coils of very fine iron wire are then introduced, occupying from 20 to 25 c.m., and the rest of the tube is filled up with porous crusts of anhydrous carbonate of soda. The part of the tube containing the wire coils is heated first, and the heat is then gradually extended towards the closed end, till that has been reached. At this temperature the organic matter is decomposed by the peroxide of iron and any portions which may be volatilised by the red-hot spirals. In whatever form the chlorine, bromine, or iodine is disengaged it is arrested by the red-hot iron, forming with it the sparingly volatile compound FeCl_2 , FeBr_2 , or FeI_2 . Traces of Fe_2Cl_6 , Fe_2Br_6 , &c., are arrested by the alkaline carbonate. After cooling, the tube is cleaned externally, broken over a sheet of paper, and the fragments placed in a flask containing a little distilled water, which is raised to a boil. The chlorides, bromides, and iodides are decomposed by the alkaline carbonate. It is filtered, washed, the filtrate acidified with nitric acid and precipitated with nitrate of silver. In most cases the total volume of the liquid does not exceed 40 c.c.

Detection of Oil of Turpentine or of Rosemary in Oils for Lubricating Machinery.—M. Burstynn.—The oil is shaken up with an equal volume of alcohol at 90 per cent. This dissolves a very small quantity of oil and any free fatty acids. If the essences of turpentine or rosemary are present, they also are chiefly taken up. After standing for some hours the oil and the alcoholic liquid are completely separated, and the latter, which forms the upper stratum, may be drawn off by means of a syphon. It contains the fatty acids and the essences. This mixture is distilled in the water-bath, the essences are carried along by the vapour of alcohol, and are found chiefly in the first portion of the distillate collected. The fatty acids and the oil dissolved remain in the retort. The presence of the essences in the distillate is easily shown. If the quantity of the essence in the alcohol is not less than 0.1 per cent in volume, the liquid becomes turbid on dilution with water. If the proportion is smaller, the liquid remains clear. In this case, if we add to the diluted solution a few drops of sulphuric acid, a very distinct rose-red colour is produced where the two liquids come in contact. If the essential oils are present in quantity, the sulphuric acid forms a rose-coloured stratum at the bottom of the test-tube.

Determination of the Quantity of Acid contained in Fatty Oils.—M. Burstynn.—Into a test-glass with a foot, and fitted with a ground stopper, pour equal measures of the oil and of alcohol, at 88 to 90 per cent. Stopper, shake well, and let settle. After two or three days the alcohol forms a perfectly clear supernatant stratum, containing the free fatty acids and a little oil: 25 c.c. of this

alcoholic solution are taken out with a pipette. A few drops of an alcoholic solution of turmeric are added, and the liquid is titrated with a normal solution of caustic potassa. The number of c.c. of the solution of potassa employed, multiplied by 4, shows how much of the alkaline liquid is required to neutralise the free acids in 100 c.c. of the oil. As the acidity of the oil is due not to one acid, but to a mixture, the result thus obtained cannot be calculated into weight. The number found, however, enables us to calculate what weight of any of the acids present is equivalent to the total acidity of the mixture.

Ferments and Fermentations.—C. Blondeau.—Not suitable for abstraction.

History of the Manufacture of Turkey-red.—Theodore Chateau.—A continuation of the memoir commenced in the January issue.

Note on Explosive Compounds.—P. Champion and H. Pellet.—Already noticed.

March, 1876.

Progress of the Manufacture of Artificial Colouring Matters.—Ad. Wurtz.—An extract from the fifth volume of the Report of the French Commission at the Vienna Exhibition.

New Explosive Agents.—Alfred Nobel.—A Lecture delivered before the Society of Arts.

History of the Manufacture of Turkey-Red (continued).—Theodore Chateau.—A lengthy historical survey, into which want of space forbids us to enter.

Reimann's Farber Zeitung,
No. 7, 1876.

A Forgotten Colour.—The simple decoction of onion-peel is said to produce upon glove-leather an orange-yellow superior in lustre to any other. It is also said to be suitable for mixing with light bark shades, especially willow bark, and as a yellow for modulating browns. The onion-dye is said to fix itself readily, even upon leathers which resist colours, and covers them well and even. Dr. Reimann seems, however, almost as sceptical of the tinctorial power of onions as of truffles.

Bulletin de la Societe Chimique de Paris,
No. 4, February 20, 1876.

Third Memoir on Albumenoid Bodies.—(Conclusion.) M. P. Schützenberger.—This voluminous essay is quite unfit for abstraction.

Products of the Action of Chloride of Lime upon the Amines.—M. J. Tschermak.—The author examines the action of chloride of lime upon hydrochlorate of ethylamin; describes the preparation, properties, and constitution of dichlor-ethylamin, and its behaviour with zinc-ethyl.

Composition of the Gases Derived from the Combustion of Pyrites.—M. A. Scheurer-Kestner.—A reply to M. Bode's critique (*Polytech. Journ.*, November, 1875, 322) on the author's former paper on this subject (*Bulletin de la Soc. Chimique*, xxiii., p. 437).

Volatile Oil of Storax.—M. J. H. van't Hoff.—The author finds that this oil is lævogyrous; that the optical activity is not due to styrolen, and that the identity of this body with cinnamon maintained by Kopp is not refuted by the researches of Berthelot. Volatile oil of storax contains a body of great optical activity (-30° at least), and of a formula approaching $C_{10}H_{18}O$. This body is only present in storax to the extent of one-twentieth per cent.

Swedish Correspondence.—M. Cleve.—This consists of notes on the action of chlor-oxy-carbonic ether upon amidophenol, by M. E. Groenvik; on the derivatives of

the biliary acids, by M. J. Lang; on the formation of fibrin, by M. Olof Hammarsten; on the spectra of the metalloids, by MM. Angström and Thalen; and on the mercaptides and certain sulphides of ethyl, by M. P. Clässon.

Les Mondes, Revue Hebdomadaire des Sciences,
No. 8, February 24, 1876.

This issue contains no chemical matter.

No. 9, March 2, 1876.

A paragraph on eosin describes it as a new orange colour.

MISCELLANEOUS.

Asbestos Powder.—Asbestos powder, made into a thick paste with liquid silicate of soda, is used with great advantage for making joints, fitting taps, and connecting pipes, filling cracks in retorts, &c. It will be found to be of great service in the manufacture of nitric acid, sulphuric acid, and other products, because it can be employed in a very easy way; it hardens very quickly, stands any heat, and prevents the escape of acid vapours. Mr. John Casthelaz informs us that he has used it for about twenty years, but he recently found that its use for the above purposes was unknown to several prominent manufacturing chemists. They have since adopted it, and are highly satisfied with the result.

The Visit of the Fellows of the Chemical Society to the Royal Arsenal.—A special train conveyed Prof. Abel's guests to and from the Woolwich Arsenal on Tuesday last, and the whole of the arrangements, from whatever point they may be viewed, were in the highest degree successful. About 450 Fellows of the Society were present. An hour and a half was spent in the Laboratory, Carriage Department, and Gun Factory. Then came the firing of the 80-ton gun. The first trial was made with a charge of 220 lbs., and the second with a charge of 230 lbs., the shot weighing 1265 lbs. The first experiment gave a muzzle-velocity, as observed by the chronoscope, of 1546 feet per second, the mean internal pressure on the gun being 23.3 tons per inch. In the second trial the velocity only reached 1536 feet, notwithstanding the additional 10 lbs. of powder employed, the slight diminution in speed being due to the additional length and looser packing of the powder cartridge in this instance. By this arrangement the internal pressure was reduced to 18.9 tons, as compared with the former result of 23.3 tons—showing that, at a comparatively small sacrifice of speed, considerably less strain was put upon the gun. The powder used consisted of 1.7-inch cubes, previous trials having established the superiority of this size over the 2-inch cubes recently employed. This was followed by three series of experiments on Detonation and its Applications, a subject which Prof. Abel may almost be said to have made his own. The first series illustrated the conditions which promote detonation of an explosive agent by a blow, or by the force exerted by an initial detonation. A fuze containing 30 grms. of gunpowder strongly confined and exploded in contact with a mass of compressed gun-cotton only inflamed and dispersed it, a similar effect only being produced by 3 grms. of unconfined mercuric fulminate—a powerfully explosive compound. But a fuze containing 0.6 of the latter material in a strongly confined state immediately produced detonation. It was thus made apparent that the explosion of the gun-cotton was dependent on the sharpness and quality of the initial detonation, as well as upon the resistance to mechanical dispersion offered by its own mass. Still more striking experiments on this head were the detonation of 10 grms.

of compressed gun-cotton in proximity to dynamite—producing immediate explosion of the latter—and the detonation of 80 grms. of dynamite in actual contact with compressed gun-cotton, which merely suffered dispersion. The second series of experiments bore upon the transmission of detonation through varying masses of compressed gun-cotton, and the means of scientifically computing its rate under certain circumstances, the third series being devoted to some illustrations of sundry practical applications of the same material. Thus a wrought railway-iron was shivered by the firing upon its surface of about 8 ozs. of this substance, a similar fate being shared by a large granite block. In the latter case, however, the interest lay in the fact of the destruction of the block by a wet mass of perfectly unflammable gun-cotton, detonated by the firing on its surface of a comparatively trifling charge of the same body in a dry state. The interesting experiments in the Royal Gun-Factories included the formation of the outer coil of a 38-ton gun, the welding under the 40-ton hammer of an exterior 80-gun coil, and the “shrinking” of the trunnion-coil on to a 38-ton gun. Some “rolling-mill” work was also shown before the building was quitted. The final display was the run of the “Whitehead,” or “Fish” torpedo (self-propelled by means of compressed air) in the canal of the Arsenal. Luncheon was provided for the visitors; after which Dr. Hooker, F.R.S., in appropriate terms, proposed the health of the host.

PATENTS.

ABRIDGMENTS OF PROVISIONAL AND COMPLETE SPECIFICATIONS.

Improvements in gas or hot-air engines, heated by heat generated by chemical action igniting hydrogen and oxygen gases to form water. P. Vera, Kensington Garden Square, Bayswater, Middlesex. January 16, 1875.—No. 175. My invention consists of a process and apparatus for generating heat and motion, by causing the explosion of a mixture of hydrogen and oxygen gases in the proportions for forming water, and utilising the heat generated, thereby to convert the water so formed into steam, also to heat air to a high temperature with the steam produced in order to serve as a motor like steam serves, and also utilising the electricity generated by the oxygenation of the hydrogen for decomposing water to obtain hydrogen and oxygen gases to add to the supply of gases for the same machine.

Improvements in the manufacture of phosphate of iron, and its application to the preparation of alkaline phosphorus, and to the extraction of phosphorus. J. Box, Great George Street, Westminster, E. Aubertin, L. Boblique, and H. Leplay, Paris. January 16, 1875.—No. 181. Phosphate of lime is dissolved in hydrochloric or other acid, and oxide of iron is also dissolved in similar acid: the two solutions thus prepared are mixed, the insoluble matter from each is removed, and the excess of acid saturated with carbonate of lime, a precipitate of phosphate of iron is formed, which is employed in the manufacture of ammoniacal salts; also the preparation of alkaline phosphate by a wet and dry process as described.

A certain new and useful process in the production of glossiness, and the application of mordants and dye on vegetable textiles. J. B. Kuenemann, Leicester Square, Middlesex. (A communication from P. Magnier and E. Kunemann, M.D., both of New Orleans, United States of America.) January 19, 1875.—No. 194. According to this Provisional Specification the textile material is converted into cellulose, and it is deoxidised preparatory to mordanting and dyeing.

Improvements in apparatus and means for utilising petroleum and other hydrocarbon liquids in lieu of coal or other fuel. O. C. D. Ross, C.E., Craven Street, Strand, Middlesex. January 19, 1875.—No. 197. This invention mainly relates to a shallow vessel termed a “carburetter,” constructed with several orifices, through one of which atmospheric air is to be drawn or driven, and through such one of the others as may be selected by the operator it will emerge more or less saturated with hydrocarbon vapour in a state in which it is inflammable. This carburetter should be so partitioned interiorly as to allow of the current of air passing over the surface of the liquid in a long and continuous current, and the channel to which the air is confined should be capable of regulation so as to admit more or less vapour. Various subsidiary details are given. In lieu of atmospheric air gases of an inferior quality are employed, such as poor coal-gas, wood-, or peat-gas, decomposed steam, carbonic acid or oxide, more or less diluted with nitrogen or other vapours.

Certain compounds for dissolving resins and gum-resins, and for other purposes for which alcohol or methylated spirit is now employed. Murdock Mackay, practical chemist, Mark Street, Finsbury, Middlesex. January 19, 1875.—No. 200. Adding to methylated spirit one or other of two certain combinations hereinafter stated, or a certain article hereinafter stated, and stirring till the mixture becomes

opaque or turbid, and then adding more methylated spirit till it becomes clear. The combinations are termed No. 1 and No. 2. No. 1 consists of 1 part of benzoline to 6 parts of mineral or coal naphtha, or 1 part of turpentine to 6 parts of mineral or coal naphtha; or half one part of benzoline and half one part of turpentine may be substituted for one whole part of either. No. 2 consists of 1 part of turpentine to 3 parts of benzoline. The said certain article is benzoline, which is employed in the proportion of 1 part of benzoline to 3 of methylated spirit, or any other hydrocarbon (employed in suitable proportions) may be substituted for benzoline. Instead of combination No. 1 mineral or coal-tar naphtha may sometimes be employed. The proportions stated may be varied.

An improved process for preserving vegetable and animal substances. W. R. Lake, Southampton Buildings, London. (A communication from J. R. McClintock, New Orleans, United States of America.) January 19, 1875.—No. 207. This invention relates, first, to the dry fumigation of the articles to be preserved by subjecting the same to the action of the gases or fumes generated from a compound of powdered sulphur and charcoal: after the process of dry fumigation has been completed, the said article or articles must be submerged in water thoroughly impregnated with the gas or vapour generated from the same composition.

NOTES AND QUERIES.

** Our Notes and Queries column was opened for the purpose of giving and obtaining information likely to be of use to our readers generally. We cannot undertake to let this column be the means of transmitting merely private information, or such trade notices as should legitimately come in the advertising columns.

Treatment of Woollen Waste.—Would Mr. Allen, of Sheffield, kindly inform me what are the chemical reagents used, and in what proportions in the treatment of woollen waste (shoddy) for the manufacture of manure?—GUILLERMO PROSSORIO.

MEETINGS FOR THE WEEK.

- MONDAY, 20th.—Medical, 8.
— London Institution, 5.
— Society of Arts, 8. Cantor Lectures. “Wool Dyeing,” by George Jarman.
- TUESDAY, 21st.—Civil Engineers, 8.
— Zoological, 8.30.
— Royal Institution, 3. “On the Classification of the Vertebrated Animals,” by Prof. Garrod.
- WEDNESDAY, 22nd.—Society of Arts, 8. “Railway Couplings,” by F. A. Brocklebank.
— Geological, 8.
- THURSDAY, 23rd.—Royal, 8.30.
— Royal Institution, 3. “Polarised Light,” by Mr. Spottiswoode.
- FRIDAY, 24th.—Royal Institution, 9. “Geological Measure of Time,” by Prof. McK. Hughes.
— Quekett Microscopical Club, 8.
— Society of Arts, 8. India Section. “The Land Revenues of India,” by Major-General Marriott C.S.I.
- SATURDAY, 25th.—Royal Institution, 3. “Human Senses,” by Prof. Robertson.
— Physical, 3.

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THE CHEMICAL NEWS.

VOL. XXXII. No. 852.

ON REPULSION RESULTING FROM RADIATION.*

By WILLIAM CROOKES, F.P.S., &c.

PART III.

THIS paper contains an account of experiments on the action of radiation on bodies the surfaces of which have their radiating and absorbing powers modified by various coatings. The difference between a white and a lamp-black surface in this respect was at first not very decided, and experiments have been instituted with the object of clearing up some anomalies in the actions observed. Two pith discs, one white and the other black, are suspended on a light arm in a glass bulb by means of a fine silk fibre; after perfect exhaustion the white and black discs are found to be equally repelled by heat of low intensity, such as from the fingers, warm water, &c. A copper ball is then tried at gradually increasing temperatures. Up to 250° C. it repels both equally, above that the black is more repelled than the white, and at a full red heat the repulsion of the black disc is very energetic. A lighted candle acts with more energy than the red-hot copper.

The presence of even a small quantity of aqueous vapour in the exhausted apparatus almost, if not quite, neutralises the more energetic action which luminous rays appear to exert on a blackened surface.

After describing several different modifications and some new forms of apparatus devised to facilitate experiment, the author gives a drawing of an instrument which enables him to get quantitative measurements of the amount of incident light falling on it. It consists of a flat bar of pith, half black and half white, suspended horizontally in a bulb by means of a long silk fibre. A small magnet and reflecting mirror are fastened to the pith, and a controlling magnet is fastened outside so that it can slide up and down the tube, and thus increase or diminish sensitiveness. The whole is completely exhausted and then inclosed in a box lined with black velvet, with apertures for the rays of light to pass in and out. A ray of light reflected from the mirror to a graduated scale, shows the movements of the pith bar. The degrees of deflection produced by the light of a candle at distances from 6 feet to 35 feet are given.

The experimental observations and the numbers which are required by the theoretical diminution of light with the square of the distance, are sufficiently close, as the following figures show:—

Candle 6 feet off gives a deflection of 218.0°

" 12	"	"	54.0°
" 18	"	"	24.5°
" 24	"	"	13.0°
" 10	"	"	77.0°
" 20	"	"	19.0°
" 30	"	"	8.5°

The effect of two candles side by side is practically double, and of three candles three times that of one candle.

The action of various solid and liquid screens is next given.

A candle three feet off, giving a deflection of 180° , has its action reduced to the following amounts by—

Yellow glass	161°
Blue	"	102°
Green	"	101°
Red	"	128°
Water	47°
Alum	27°

* Abstract of a paper read before the Royal Society, Feb. 10, 1876.

A candle on each side of the apparatus, and equidistant from it, keeps the index ray of light at zero; by shading off either one or the other the light flies off to either extremity of the scale. This gives a ready means of balancing two sources of light one against the other. Thus, retaining the standard candle 48 inches off, on the left of the bar, the index is brought to zero by placing on the right

2 candles	67 ins. off.
1 candle behind solution of sulphate of copper	6	"
" " alum plate	14 "
A small gas burner	113 "

These experiments show how conveniently and accurately this instrument can be used as a photometer. By balancing a standard candle on one side against any source of light on the other, the value of the latter in terms of a candle is readily shown; thus, in the last experiment the standard candle 48 inches off is balanced by a gas-flame 113 inches off. The lights are therefore in the proportion of 48^2 to 113^2 , or as 1 to $5\frac{1}{2}$. The gas-burner is therefore equal to $5\frac{1}{2}$ candles.

By interposing screens of water or plates of alum, and so cutting off the dark heat, the actual luminosity is measured. In addition to this, by interposing coloured glasses or solutions, any desired colours can be measured either against the total radiation from a candle, its luminous rays, or any desired colour. One coloured ray can be balanced against another coloured ray, by having differently coloured screens on either side.

The variations in the luminosity of a "standard" candle will cease to be of importance. Any candle may be taken; and if it be placed at such a distance from the apparatus that it will give a uniform deflection, say of 100 divisions, the standard can be reproduced at any subsequent time; and the burning of the candle may be tested during the photometric experiments by taking the deflection it causes from time to time, and altering its distance, if needed, to keep the deflection at 100 divisions.

If the pith bar in this instrument be blacked on alternate halves, an impetus given by a ray of light always acts in the same direction of movement. A candle causes it to spin round very rapidly until the suspending fibre is twisted up, and the rotation is stopped by the accumulated torsion.

By arranging the apparatus so that the black and white surfaces are suspended on a pivot instead of by a silk fibre, the interfering action of torsion is removed, and the instrument will rotate continuously under the influence of radiation. To this instrument the author has given the name of the "Radiometer." It consists of four arms of very fine glass, supported in the centre by a needle-point, and having at the extremities thin discs of pith lamp-black on one side, the black surfaces all facing the same way. The needle stands in a glass cup, and the arms and discs are delicately balanced so as to revolve with the slightest impetus.

In the *Proceedings of the Royal Society* last year, the author gave a brief account of some of the earlier experiments with these instruments. In the present paper he enters very fully into the various phenomena presented by them, and gives Tables showing the number of revolutions made by the radiometer when exposed to a constant source of light removed different distances from the instrument. The law is that the rapidity of revolution is inversely as the square of the difference between the light and the instrument.

When exposed to different numbers of candles at the same distance off, the number of revolutions in a given time are in proportion to the number of candles, two candles giving twice the rapidity of one candle, and three, three times, &c.

The position of the light in the horizontal plane of the instrument is of no consequence, provided the distance is not altered; thus two candles, one foot off, give the

same number of revolutions per second, whether they are side by side or opposite to each other. From this it follows that if the radiometer is brought into a uniformly lighted space it will continue to revolve. This is proved to be the case by experiment.

The speed with which a sensitive radiometer will revolve in full sunshine is almost incredible. Nothing is visible but an undefined nebulous ring, which becomes at times almost invisible. The number of revolutions per second cannot be counted, but it must be several hundreds, for one candle will make it spin round forty times a second.

The action of dark heat (*i.e.*, from boiling water) is to repel each surface equally, and the movement of the radiometer is therefore arrested if a flask of boiling water is brought near it. The same effect is produced by ice.

From some observations made by the author, it appears probable that heat of a still lower refrangibility repels the white more than it does the black surface. Many instances are given of the radiometer revolving the reverse way. Thus, breathing gently on the instrument will generally cause this effect to be produced.

An experiment is described with a radiometer, the moving parts of which are of aluminium, blacked on one side. When exposed to the radiation from a candle, the arms revolve the normal way. On removing the candle they revolve the reverse way. Heated with a Bunsen burner the arms revolve the normal way as they are getting hot, but as soon as the source of heat is removed and cooling commences, rotation sets up in the reverse way, and continues with great energy till the whole is cold. It appears as if the reverse movement during the cooling is equal in energy to the normal movement as it is being heated.

It is easy to get rotation in a radiometer without having the surfaces of the discs differently coloured. An experiment is described with one having the pith discs blacked on both sides. On bringing a candle near it, and shading the light from one side, rapid rotation is produced, which is at once altered in direction by moving the shade to the other side.

The author describes many forms of radiometer, by means of which the movements can be exhibited to a large audience, or can be made to record themselves telegraphically on a self-recording instrument.

A STUDY OF HYDROCARBONS.

II. ALIZARIN AND DIBASIC COMPOUNDS.

By S. E. PHILLIPS.

(Continued from page 38.)

THE skeleton or mere dictionary character of the Tables referred to would be incomplete without a generic appendix, whose object should be to succinctly epitomise the leading characters of generic groups.

What is a quinon, a pinacolin, a urethan, or urethylan? would here find answers in brief funeral notices; while acids, aldehyds, alcohols, &c., might be enlarged to embrace separate categories.

It might be well to give some account of the generic origin of the tribasic acids; but while very much is done with "hydroxyl" dispositions, and M. Richter exults in a molecular omniscience, which gives a marvellous insight and a delightful pleasure to the architect of that pleasantry, yet I must confess to an utter ignorance of the subject.

The generic production of the infinitude of ordinary acids is well provided for in the Tables referred to, but no place whatever is assigned for the succinic group of acids. I deeply regret this, but deem it well to make the confession.

While the fatty acids are universally considered *mono-*

basic, it is not a little curious that some chemists should—on the most slender grounds—invent for them fancied types of *ortho* or tribasic peculiarity, while the succinic groups are persistently regarded as dibasic.

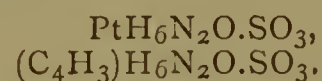
That view necessitates that the radical or acid group should contain an even number of H atoms, which I hold to be a violation of all analogy, and quite incapable of proof.

If ethylen ammonia is represented as $(C_4H_3)H_2N$,
No! says diatomic hypothesis, it is $(C_4H_4)HN$.

With a limited number of facts it might be difficult to decide between the two alternatives; but now that knowledge is extended it is easy to see that, in many cases, all the H atoms of ammonia are similarly replaceable, while with the diatomic or triatomic groups no such action is at all possible (see "Citric Acid," vol. xxvii., p. 109).

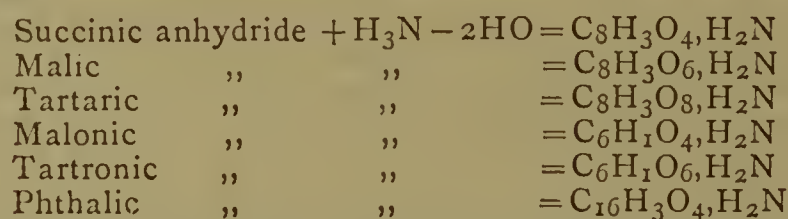
Notwithstanding the great reluctance evinced to fairly estimate the true character of the nitrile radicals, yet it is well that the propargyl hydride, C_6H_3H , has been well and definitely established in its corresponding relations with ethane; and now it is further seen that propargylen, C_6H_1H , has similar relations, and that its behaviour strictly corresponds with acetylen, C_4H_1H !

M. von Lang states that the sulphates of ethylen-diamine and platino-diamine are isomorphous: they contain the elements of—



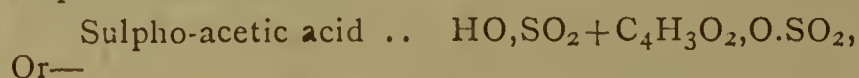
That the succinic group of acids are tribasic is well evinced in their ammonia types, in their sulpho-acid salts, and in their varied substitutional condensations.

The amides, anilides, or other ammonia forms, are very various, but all are consistent with the simplest amide forms.

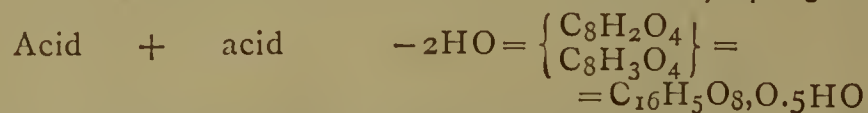
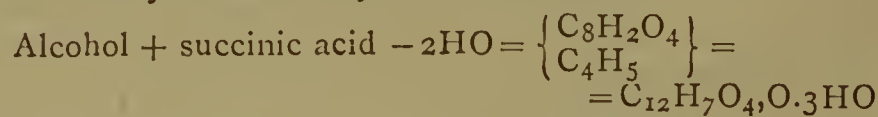


These can be similarly derived from the normal acids, $-4HO$, but for such reactions the anhydrides are mostly preferred.

If acetic and succinic acids are separately digested with sulphuric acid there results—



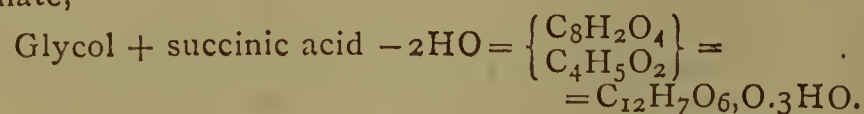
The laws of condensation testify to the same result and we may have an ethyl or a succin-succinic acid.



the only difference in the radicals being that in one case ethyl replaces one H, whereas in the other succinyl replaces one H!

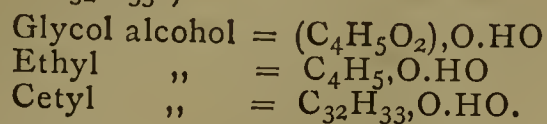
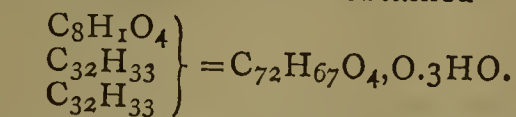
Ditartaric and benzo-tartaric acids are quite parallel, and these condensations may be traced in many ways with only one consistent result.

It is curious to notice the varied shades of confusion ever subsisting with the diatomic ethylen, and it is quite clear that "ethylen succinic acid" is truly a "glycol succinate,"—



It is produced by digesting glycol with succinic acid at $150^\circ C.$; but if the temperature be raised to $300^\circ C.$, then the anhydride results $C_{12}H_7O_6, O.HO$.

A dicetylo-succinate has been obtained—



We now pass on to a wider consideration of alleged diatomic behaviour.

As in mineral chemistry we have the alums and other double saltic types, so in organic chemistry the same play of forces between — and + salts subsists in greater variety, the double sulphates being called sulpho-salts,—the double carbonates carbo-salts, &c. As typical of the whole we may state that—

Sulpho-salts are considered monobasic,
Di- " " dibasic,
Tri- " " tribasic.

This may be really so, but I cannot accept it without fuller evidence and a better explanation.

There are many considerations which point in a contrary direction, and the limits of this paper only admit of a glance at some of these. At the onset it is a very suspicious circumstance that such bi-bases (?) often occur with salts of admitted mono-acids.

If we tabulate a long list of sulpho-hydro-cinnamates we find they consist of alleged acid and neutral salts.

All the acid salts conform to the type—



For a long time the others have been a standing enigma, as if they contained the elements of—



I knew that some acids were under these circumstances either di- or tri-basic, but this was not one of them; and that if acetic acid would give a certain type of salt, or amide, &c., that hydro-cinnamic acid would act similarly.

		Acids.
Acetic acid		$\text{C}_4\text{H}_3\text{O}_2, \text{O.HO}$
Hydro-cinnamic acid		$\text{C}_{18}\text{H}_9\text{O}_2, \text{O.HO}$

		Salts.	Amides.
		$\text{C}_4\text{H}_3\text{O}_2, \text{O.MO}$	$\text{C}_4\text{H}_3\text{O}_2\text{H}_2\text{N}$
		$\text{C}_{18}\text{H}_9\text{O}_2, \text{O.MO}$	$\text{C}_{18}\text{H}_9\text{O}_2\text{H}_2\text{N}, \text{ \&c.}$

Many chemists have none of these misgivings: they can as easily notate such an acid with $(\text{C}_{18}\text{H}_8)$ as with $(\text{C}_{18}\text{H}_9)$, and may even prefer it, as more like the favourite ethylen.

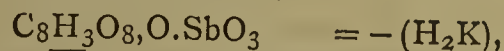
The secret of all this now stands revealed in the tendency of some reactions, and especially with some hydrocarbons, to a metallic substitution in the radical. This tendency is most strongly manifested in certain nitrile or aldehydic hydrocarbons, with a diminished proportion of H,—with acetylen, propargylen, and bodies of that character, copper or some other metal seems almost as normal as the H itself!

This subject has engaged much attention in a prolonged study of urea and ureide condensations, to the following result:—"We therefore sweep away all the tribasic cyanurates, glycollamates, and an infinity of others. They have long been a perplexity and a stumbling-block; but the positivity of the enunciations, conjoined with a paucity of the means of research, have kept a humble amateur in restless suspense. We now take confidence in the perception so long desired, that hosts of di- and tri-basics can be explained and cleared away, in the recognition of a simple mono-law of saltic combination."

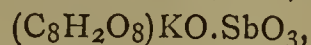
The truth is that such bodies are no salts at all, but metal amides; and a great want of discrimination has tended to needlessly confuse a very complex subject. As well might we say that zinc amide, ZnH_2N , is a monobasic salt, and Zn_2HN a diabasic salt, &c. Such considerations may enable us to penetrate a mystery in regard to tartar emetic, which has puzzled many.

That acid contains the elements of $\text{C}_8\text{H}_3\text{O}_8, \text{O.3HO}$
The ordinary saltic forms $\text{C}_8\text{H}_3\text{O}_8, \text{O.2MO}, \text{HO}$
Tartar emetic $\text{C}_8\text{H}_3\text{O}_8, \text{O.SbO}_3, \text{KO}, \text{HO}$

We now regard tartar emetic as—

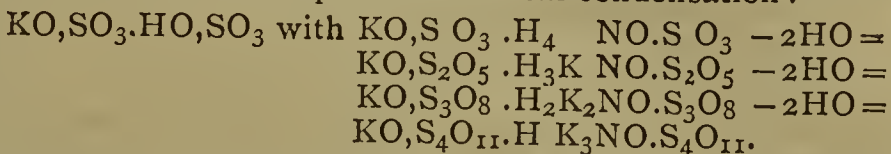


because it so happens that the water of crystallisation is expelled at 212°F. , while at 400° chemists have been puzzled by the anomalous result, viz.,—



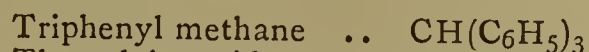
which is tartar emetic when dissolved in water. In such case we have the elements of a tartryl radical, minus one H, and minus the necessary O! Let K replace the missing H, the O is then provided, and we have a normal tribasic tartrate!

As a remarkable instance, and a feasible illustration that the type need not necessarily alter by metallic substitution, we cite a sulpho-ammoniacal condensation:—

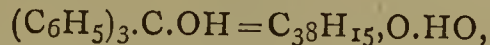


Similar condensations occur with the mono-, di-, and tri-sulpho salts of methyl, phenyl, &c., but they have a special interpretation of di- or tri-atomic hypothesis.

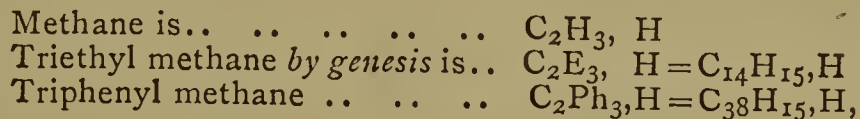
When Kekulé and Franchimont synthesised "triphenyl methane," it took shape and character from the special point of view. Some interpreted these hydrocarbons as benzol derivatives, but they preferred a genesis from the fatty acid bodies—



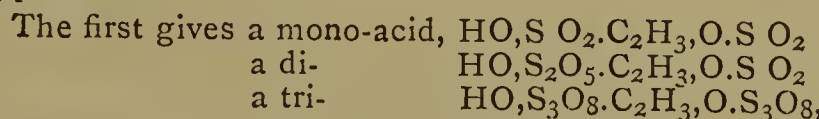
but subsequently I find it yields "a true carbinol,"—



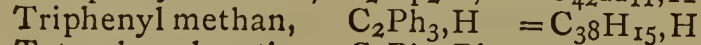
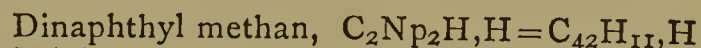
and the triatomic peculiarities vanish in the inevitable future result.



and all three are, by fact and behaviour, hydrocarbons in common with all others, and equally defiant of special hypothesis.



and similarly with the two others and all alike affecting metallic substitution in the radical, in proportion as the saltic condensation is pushed. As methane is not a tri-atomic body, so neither is either of its more complex derivatives or analogues. The association of triphenyl methane with its trisulpho acid, and their analogy of constitution, is either real or true or fancied and false! A proper survey of collateral facts very strongly suggests the latter alternative. We have—



and while it may be well to take note of these genetic origins, as some clue to the explanation of isomeric peculiarities, yet the presumption is that these type representations are, in fact, a falsity, when used in any other sense, and that the latter substance is truly a hydride, like the two others.

Let it be shown that the dinaphthyl methane is diatomic, the triphenyl triatomic, and the tetramethan tetratomic, and we must submit to evidence. In the meantime we accept these hydrocarbons in the sense referred to in the Tables, and look for their oxide, sulphide, chloride, hydrate, acid, or ammonia derivatives, in common with all other hydrocarbons, quite irrespective of their individual specialities or of the isomeric genesis of their radicals.

Nonan, $C_{18}H_{19}H$, is a hydrocarbon possessing all these characteristics. It may be true that some of these hydrides are 1 vol., others 2 vols., and that their boiling-points differ in proportion as they may be derived from a di-, tri-, or tetra-substitution or condensation; but will any one have the hardihood to affirm that *the* nonan tetra-ethyl methan, $C_2E_3E = C_{18}H_{19}H$, is in any way tetratomic?

Heptan (œnanthyl), $C_{14}H_{15}H$, boils at $99^\circ C$.

The isomer "triethyl methan," 1 vol., C_2E_3H , boils at 96° ,

The tetra-isomer, C_2E_2Me, Me , boils at 87° ,

but in a very wide chemical sense they are all one heptan, in full possession of all the generic characteristics referred to.

We have two sulpho-anthracenic acids,—

The mono- $HO, SO_2.C_{28}H_9, O.SO_2$

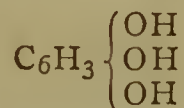
The di- $HO, S_2O_5.C_{28}H_9, O.S_2O_5$.

but by the terms of the above hypothesis the second would not be anthracenic, and the anthracen-oxide must become $C_{28}H_8.SO_2$!

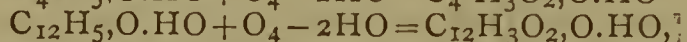
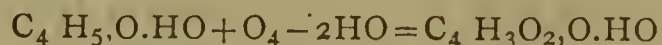
In sight of all this it is easy to understand how Dewar may have been misled in estimating the diatomic phenols, and then the transition is easy through a host of similars, and by the naphtha- and anthraquinons to alizarin—the special object of this enquiry.

Let us take hydroquinon as an illustration, and the allegation is that a monobasic sulpho-phenate, by oxidation with potash hydrate, becomes an oxy- or diatomic and dibasic phenol. $C_6H_5.OH$ becomes $C_6H_4 \begin{Bmatrix} OH \\ OH \end{Bmatrix}$.

Of course all this is ingeniously ethylenic, but of such radicals as $(C_{12}H_4)$, or of such hydroxyls, I have no evidence whatever, much less of the three hydroxyls of pyrogallol—



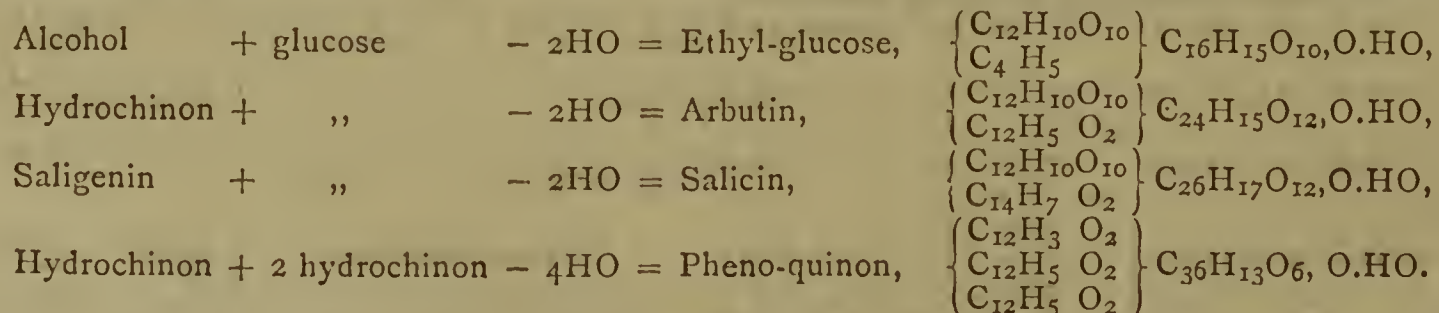
We hold that the same character and formula of oxidation which gives acetic acid from alcohol also gives quinon from phenol—



and both acid results are really and strictly monobasic.

Whether the second be the actual quinon or one of its isomers does not at all concern us in this connection: in either case 2 of nascent H are assimilated to the production of a hydroquinon type, $C_{12}H_5O_2.O.HO$.

That glucose is an alcohol or hydrate has been proved by ammonia substitution, and other evidence of the completest character. Hence—



Can anything be plainer than the similarity, or, in other words, the generic character of these reactions? Glycerin or pyrogallol has 3 hydroxyls, erythrite 4, and glucose 6 hydroxyls; yet they all behave in the same generic manner.

As alcohol + ammonia - 2HO = the amide C_4H_5, H_2N ,

So arbutin „ „ „ $C_{24}H_{15}O_{12}, H_2N$

Or glucose „ „ „ $C_{12}H_{11}O_{10}, H_2N$,

and similarly with glycol, erythrite, pyrogallol, &c.

These radicals are absolute facts, admitting of the clearest evidence, even though they may or may not have a separate free existence; while the hydroxyl hypothesis

is nothing beyond an ingeniously devised numerical pleasantry.

The artificial synthesis of the alizarin type has been realised in several ways, but as the ratio of C to H is unusually large most formulæ require certain modifications to get the required result; and even then, as the valuable "Turkey red" is only one of many isomers, the practical difficulty is further enhanced. And why some of these bodies affect metallic substitution in the radical, while others do not, yet remains for further explication; but the great point—the all-essential consideration—is that the real constitution can be determined by an appeal to amide or sulpho-acid derivatives. Let an acid contain the elements of $C_{28}H_8O_8$, then, if diatomic (so-called), the amide would be $C_{28}H_5O_4, H_2N$; if monatomic, then it would be $C_{28}H_7O_6, H_2N$.

At the onset of this paper I had observed a great many anthraquinon and other derivatives, all leading inevitably to the conclusion that alizarin would evince the mono-alternative; and while writing this the whole truth has been made manifest by chemical authorities of the highest standing.

Liebermann gives alizaramide $C_{14}H_9NO_3$, or amido-oxy-anthraquinon—



The barium compound .. $C_{28}H_7O_6.H_2N$ — = (H_6Ba)

Purpuramide $C_{28}H_7O_8.H_2N$.

I would not accept these as standing by themselves, because such elements may admit of another typical distribution; but as they are exactly what sound theory had pre-indicated, and as that high authority regards them as amides, it only remains to have the definite sulpho-acids of alizaric and purpuric acids for a final decision—the more so as Ladenburg had previously obtained the amine or hydrate form $C_{28}H_7O_6, H_3NO.HO$.

REPORT

ON THE

DEVELOPMENT OF THE CHEMICAL ARTS DURING THE LAST TEN YEARS.*

By Dr. A. W. HOFMANN.

(Continued from p. 112.)

If aqueous ammonia is heated in a closed boiler the expulsion of the gas can be carried on even under a strong pressure. If the gas liberated is conducted into a cooler at a certain temperature it passes the point of

saturation and condenses to form liquid ammonia. This liquid anhydrous ammonia if brought in connection with a receiver containing water rushes into it with violence and is absorbed. In proportion as the solution is heated, the temperature of the evaporating liquid is lowered, and may fall to -50° . On these principles depends the ingenious apparatus constructed by Ferd. Carré. It has two modifications, adapted respectively for the intermittent and for the continuous production of ice, the former on a small scale for quantities of 1 and 2 kilos., the latter for manufacturing purposes, and arranged so as to turn out 25 to

* "Berichte über die Entwicklung der Chemischen Industrie Während des Letzten Jahrzehends."

200 kilos. hourly. The apparatus is manufactured by Mignon and Rouart, of Paris. The intermittent machine consists simply of two vessels free from air and connected firmly by means of a tube. The weight of the whole is such that it can be conveniently lifted and turned. One of the receivers contains ordinary liquid ammonia, whilst the other is empty. The operation begins by placing the receiver containing ammonia over a charcoal fire, whilst the empty one is set in a tub of cold water. The gas is expelled by the heat and condensed in the cold receiver, forming a liquid. When all the ammonia has passed over the apparatus is taken up, the receiver which had previously been heated is placed in the tub of cold water, and the substance to be frozen is put in a tin cylinder, fitting into a concavity of the receiver containing the liquefied gas. The latter evaporates, producing a great reduction of temperature, and is again absorbed by the water which has remained in the other receiver.

The continuous apparatus is more complicated. It consists principally of a vertical cylindrical boiler supported by masonry in which the heating and volatilisation of the ammonia for the preparation of liquefied ammonia goes on without interruption. The cylinder consists of two compartments, the lower containing a very dilute solution chiefly deprived of its ammonia, whilst in the upper are a number of bowls, into the uppermost of which flows fresh liquor ammoniac; the liquid as it overflows enters the next bowl, and so onward. The lower compartment only is exposed to the fire. The very aqueous vapours given off convey as they ascend more and more ammonia into the bowls, which at last evaporates almost, though not absolutely, free from water. The gas now arrives in the cooling apparatus, in which it is condensed to a liquid. The pressure at which this takes place depends on the temperature of the condenser, and varies from 4.4 to 8.5 atmospheres, when the temperature of the water used for cooling ranges from 0° to 20°. The temperature of distillation is about 130°. From here the liquefied ammonia, at the pressure of the boiler, arrives in a regular current in the evaporator (ice-generator), the influx being checked by a regulator. The arrangement of the ice-generator presents nothing worth of remark. A solution of chloride of calcium takes up the cold and transfers it to the water to be frozen. If a liquid, *e.g.*, beer-wort, requires merely to be cooled, no intermediate body is requisite. The ammonia evaporating in the cooler has to be absorbed by water. As an absorbent is employed, the liquid in the

lower half of the cylinder, which is not quite exhausted and which issues continuously in a thin stream, arrives cooled in the absorption vessel. The latter, again, must now lie in a cooler in order that the high temperature produced by absorption may not hinder the further reception of gas. Thus, therefore, liquor ammoniac is formed as at first, and is thrown back into the cylinder by means of a pump, exchanging on the way its heat for that of the liquid drawn off from the lower compartment of the cylinder. As the ammonia evaporating in the cylinder is not quite anhydrous, a certain amount of water reaches the ice-generator, where it gradually accumulates and retards evaporation. From time to time, therefore, the contents of the cooler must be drawn off, and pumped direct into the cylinder.

(To be continued.)

ON CERTAIN CIRCUMSTANCES WHICH AFFECT THE PURITY OF WATER SUPPLIED FOR DOMESTIC PURPOSES.

By M.²M. PATTISON MUIR, F.R.S.E.,
Assistant Lecturer on Chemistry, Owens College.
(Continued from p. 104.)

For the following details regarding the amounts of lead found in various samples of aerated beverages in the manufacture of which leaden apparatus had been employed, I am indebted to my friend Dr. Milne, of Glasgow.

TABLE B.

Lead Found in Various Samples of Aerated Beverages.

Description of Liquid.	Quantity of Lead in grs. per gallon.				
Lemonade	0.20
Do.	0.40
Do.	0.05
Gingerade	0.10
Soda-water	0.60
Do.	0.05

In addition to these numbers I have determined the following, which show the amounts of lead dissolved by distilled water charged with carbon dioxide at the ordinary atmospheric pressure for varying lengths of time, and also

TABLE C.

Lead Dissolved by Water Charged with Carbon Dioxide at Ordinary Pressure.

	Mgms. per litre.	Grains per gall.	Lead Dissolved.					
			In Mgms. per Litre.			In Grains per Gallon.		
			After 24 hrs.	48 hrs.	72 hrs.	24 hrs.	48 hrs.	72 hrs.
Distilled water charged with carbon dioxide.. .. .	—	—	3	3	3	0.21	0.21	0.21
The water poured off, more added, and again poured off, and finally fresh water containing carbon dioxide added	—	—	none	none	none	none	none	none
Potassium carbonate and ammonium nitrate	100 and 20	7.0 and 1.4	merest trace			merest trace		

TABLE D.

Lead Dissolved by Water Charged with Carbon Dioxide at a Pressure of about 6 Atmospheres.

Salt.	Mgms. per litre.	Grains per gall.	Lead Dissolved.			
			In Mgms. per Litre.		In Grains per Gallon.	
			24 hours.	48 hours.	24 hours.	48 hours.
Potassium carbonate	80	5.60	13.2	32.0	0.924	2.24
Calcium chloride	160	11.20	—	6.0	—	0.42
Ammonium nitrate	16	1.12	32.0	44.0	2.240	3.08
Distilled Water	40	2.80	5.0	—	0.350	—
	—	—	10.0	35.0	0.700	2.45
	—	—	14.8	24.0	1.036	1.68

the amounts dissolved by the same water on the addition of various salts. The surface of lead exposed measured 2100 sq. m.m.

As the results indicated that water charged with carbon dioxide at the ordinary atmospheric pressure exercises no considerable solvent action upon lead, and moreover that this action ceases on the addition of carbonates; the large amounts of lead found in some of the samples of soda-water examined by Dr. Milne are probably due to the increased solvent action of water containing large quantities of carbon dioxide forced into it under pressure. In order to test the truth of this supposition I have made a few determinations of the amounts of lead dissolved by distilled water, and by the same water containing known quantities of various salts when charged with carbon dioxide at a pressure of several atmospheres.

The apparatus consisted of an ordinary *gasogene* used for making so-called "soda-water." From the known capacity of the globe and the weight of sodium carbonate and tartaric acid employed the pressure exerted in the interior of the vessel by the carbon dioxide was calculated as approximately equal to 6 atmospheres.

The surface of lead exposed measured 750 sq. m.m. (See Table D).

It appears from these numbers that distilled water charged with carbon dioxide under a pressure of (approximately) 6 atmospheres dissolves five times as much lead as the same water charged with the gas at the ordinary atmospheric pressure; that the presence of a small quantity of ammonium nitrate does not increase the solvent action until after a lapse of 48 hours or so; and that potassium carbonate, when present in somewhat large quantities, exerts a marked deterrent action. The amount of lead dissolved, however, even in the presence of potassium carbonate, is far too large to allow of such a water being drunk with safety.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, March 16th, 1876.

Professor ABEL, F.R.S., President, in the Chair.

BEFORE the ordinary business of the Society commenced,

Dr. GLADSTONE rose and said it was a pleasant duty he had to perform. The Fellows had met last Tuesday at the Royal Arsenal at the invitation of their President, where they had not only had a rare treat of a most intellectual character in visiting the works of the various departments, and in seeing experiments illustrating his discovery in explosives, but had also experienced his generous hospitality. He felt sure the Fellows would agree with him that it was fitting they should express to him their heartiest thanks for his entertainment.

Dr. GILBERT, in a few words, seconded the motion, which was carried by acclamation.

Prof. McLEOD remarked that the very interesting experiments on detonation which Prof. Abel had exhibited to them at Woolwich should be regarded as demonstrations, and that he should be asked to furnish a short account of them, which might be entered in the minutes of the Society, and thus form a permanent record of the visit to the Royal Arsenal, which the speaker thought ought to be regarded as a special meeting of the Society.

The PRESIDENT, in replying, said the pleasure he had felt at the manner in which his invitation had been responded to was enhanced by their expression of satisfaction at the arrangements which had been made to enable them to see that vast national establishment, and for which he was deeply indebted to the cordial assistance he had

received from his colleagues, the heads of the various departments there.

The minutes of the previous meeting were then read and confirmed, and the presents recently made to the Society announced. Messrs. C. O'Sullivan, W. E. R. Martin, R. Messel, and E. W. Napper were proposed as Fellows, and Messrs. Jonathan Sparke Walton, Jeremiah Head, John Henry Starling, Percy James Winsor, Philip Holland, and Alfred Anthony Nesbit were ballotted for and duly elected after their names had been read for the third time.

Dr. P. F. VAN HAMEL ROOS then gave a short notice "*On Crystallised Glycerin*," of which he exhibited a large specimen, 56 lbs. in weight. He had found that perfectly pure and dry glycerin will crystallise spontaneously when cooled to 24° F. if the liquid is vigorously agitated, or a ready-formed crystal of glycerin added to it. Moreover, a crystal of glycerin introduced into well cooled ordinary good commercial glycerin rapidly increases in size, most of the impurities remaining in the mother-liquors. He had repeated Redtenbacher's experiments on the fermentation of glycerin, which was said to produce propionic acid; but with pure glycerin he could get no signs of fermentation, and concluded, therefore, that the propionic acid must be derived from some impurity. The solidification of pure glycerin was shown during the reading of the paper.

Dr. ARMSTRONG said with regard to the question of the fermentation of glycerin giving rise to propionic acid, that some time ago, wishing to prepare a quantity of that acid, he had induced his friend Mr. Brown, of Burton-on-Trent, to examine the subject, and he had found that at 20° to 25° C. the addition of yeast to commercial glycerin at first set up a brisk fermentation, and the liquid became turbid from production of bacteria: this action soon ceased, however, and the fermentation could not be again started. It was the experimenter's opinion that the fermentation which took place in this instance was merely due to the presence of some impurity in the glycerin.

The PRESIDENT having thanked Dr. Roos in the name of the Society,

Mr. W. H. HATCHER read a paper entitled "*Notes on the Fatty Acids, and on a Suggested Application of Photography*." The author, in experimenting on the setting-points of mixtures of the fatty acids with one another and with various other fatty substances, had found that these generally differed to a considerable extent from those obtained by calculation. It would be observed, on reference to the diagrams, that in some cases the curve of setting-points was always above that obtained by calculation—in some always below, and in others, again, partly above and partly below. For instance, in mixtures of palmitic and stearic acids, the curve representing the actual setting-points was abnormally low, whilst with mixtures of palmitic and oleic acids it was abnormally high compared with the line of calculations. Similarly mixtures of the fatty acids with the harder paraffins have their setting-points below their lines of calculations, whilst with the softer paraffins they are mostly above. A series of one hundred small cast blocks of mixtures of palmitic and stearic acid, in proportions varying each from the next by 1 per cent, were exhibited, from an inspection of which it could readily be observed that the mixtures formed three groups of highly crystalline composition, separated by two groups of close fine-grained material. A series of one hundred and forty beautiful photographs of the crystalline structure of these mixtures, and some others as seen by polarised light, was also shown.

In reply to a question of the President, the author said the setting-points were determined by stirring a portion of the melted material in a test-tube with a thermometer, and noting the fall of the mercury until this became stationary. The points of solidification, as far as his experience went, were the same in all vessels. They had been compelled to reject all methods of determining melting-points as giving fallacious results.

Mr. FIELD said he had listened with great interest to Mr. Hatcher's excellent paper, and had himself always used the method just described for ascertaining the solidifying-points of fats. There were some curious points about the solidification of mixtures, which it was at present somewhat difficult to understand. For instance, a mixture of stearic acid of solidifying-point of 130° F., and spermaceti of 112° F., gave a mixture below either, namely, 108° F. Similarly, a mixture of stearic acid of 130° F., and paraffin of 130° F., gave one of 112° F.; and, still more curious, a mixture of high melting-point stearic acid from cotton seed with animal stearic acid of 130° fused at a point below that of the animal stearic acid.

In reply to a question of Dr. Müller as to the method of purification employed, the author replied that fractional crystallisation from alcohol and from ether had been used. By these means it was easy to obtain the palmitic acid in a pure state; for although a mixture of 90 parts of palmitic acid with 10 of stearic crystallised from alcohol unchanged, the two could be readily separated by crystallisation from ether, or, still better, from benzene, in which stearic acid is more soluble than palmitic. The palmitic acid had almost the same melting-point as that given by Heintz (60.8° C.), and the stearic acid from cocoa butter melted at 68.6° C.

Dr. THUDICHUM objected to the use of alcohol in the purification of the fatty acids, as he had found that fatty ethers were formed in such cases with great rapidity, especially when absolute alcohol was employed. Even in the cold, if the mixture was allowed to stand, ethers were gradually formed.

The PRESIDENT having thanked Mr. Hatcher for his valuable paper, which had given rise to such an interesting discussion,

Mr. F. JONES proceeded to read a communication "*On Stibine*." The author had examined the gases obtained by the action of acids on an alloy of zinc and antimony, by the addition of an antimony solution to a mixture of zinc with a dilute acid, and by the action of sodium amalgam on a concentrated solution of antimony chloride. In all cases, however, the stibine was mixed with a very large amount of free hydrogen, so that the attempts to analyse it eudiometrically by observing the increase of volume it underwent when submitted to the action of the electric spark, led to unsatisfactory results. The author also examined the so-called silver antimonide, produced by passing the mixed gases through a solution of silver nitrate, and ascertained that the precipitate invariably contains a considerable quantity of uncombined silver in fine filaments. The action of stibine on sulphur is very marked, the gas being decomposed, with formation of antimony sulphide and sulphuretted hydrogen according to the equation, $2\text{SbH}_3 + 6\text{S} = \text{Sb}_2\text{S}_3 + 3\text{H}_2\text{S}$. This reaction takes place slowly at 100° C., rapidly in sunlight, and accompanied as it is by the production of the red antimony sulphide, affords an extremely delicate qualitative test for antimony, the author having in one case obtained a distinct colouration with 0.00007 grm. of antimony. He has also utilised this reaction for obtaining photographic copies of fern fronds, &c., by placing them on paper coated with sulphur in an atmosphere containing stibine, and exposing them to light: the unprotected parts rapidly assume a deep orange colour, whilst the protected portions remain yellow. It was attempted to make this property the basis of a photometric process, and with fair success, the chief objection being the difficulty of obtaining and preserving the stibine of uniform strength.

Mr. SPILLER said he had listened to the paper with great interest, especially as it seemed to afford a new method for photometric observations. He must also be allowed to congratulate Mr. Jones on his photographic results.

The SECRETARY then gave an abstract of Mr. F. KÖPFER's paper "*On the Use of Platinum in the Ultimate Analysis of Carbon Compounds*." The author burns the substance in a platinum boat in a current of oxygen, the anterior

portion of the tube containing spongy platinum and coils of fine platinum wire. The results he has obtained with sugar and with aurin are very satisfactory. For the analysis of liquids the apparatus is specially arranged, and the author purposes to extend the method to carbon compounds containing chlorine, nitrogen, sulphur, and other elements.

Mr. W. H. PERKIN said he had some time ago made experiments on the combustion of carbon compounds in oxygen by means of platinum, with the object of simultaneously determining, not only the carbon and hydrogen, but also the chlorine. He then described the method, but had abandoned it on account of the practical difficulties, not only in obtaining glass which would resist the action of chlorine, and especially bromine, but also from the liability to explosions.

The last paper was by Mr. G. BECKETT and Dr. C. R. A. WRIGHT, "*On the Action of Organic Acids and their Anhydrides on the Natural Alkaloids* (Part V.). As far as regards the formation of acetylated derivatives, whilst codeine and morphine easily yield them, only negative results have been obtained by the action of acetic anhydride on narcotine (and its derivatives, hydrocotarnine and cotarnine), narceine, papaverine, and thebaine. Strychnine forms an amorphous diacetyl-strychnine, and amorphous acetyl derivatives were obtained on treating quinine and quinidine with acetic anhydride, which on saponification gave a base, apparently quinicine. A non-crystalline monacetyl-cinchonicine was also formed on submitting cinchonine and cinchonidine to similar treatment.

The PRESIDENT, after thanking the authors, adjourned the meeting until Thursday, March 30, the Anniversary.

NOTICES OF BOOKS.

A Dictionary of Hygiene and Public Health. By A. WYNTER BLYTH, M.R.C.S., &c. London: C. Griffin and Co.

WE have here a kind of encyclopædia of sanitary science in the widest sense of the term, including the detection of adulterations, the dietetic value of foods, sanitary chemistry, toxicology, and laws relating to public health. In a book of this extent, compiled from very various sources, the matter cannot, of course, be uniform in value. Still, upon the whole, we consider the work as likely to be useful.

From the notice of absinthe we learn, with regret, that the quantity of this fearful poison consumed in London has been enormously on the increase. How is it that none of our professed philanthropists can take up this question? Its importation, manufacture, and sale should be totally prohibited. Arsenic has its legitimate uses; absinthe has none.

On the propagation of cholera we find a passage, the more important as it must evidently apply in a like manner to plague, typhoid, yellow fever, &c.:—"The discharges being liquid, the great bulk of them find their way to the ground, from which the poison may be propagated in three ways: (a), by rising into the air as a product of evaporation; (b), by percolating into the drinking-water; (c), by atmospheric dispersion in the form of impalpable dust after it has passed into the dried state." If such is the case, what more perfect arrangement could be devised for the dissemination of epidemics than a sewage irrigation farm where the first and third of these methods of propagation must be alternately at work?

In the article on the disposal of the dead the author justly condemns the proposed wicker-work coffins, from the facility they would afford for the escape of noxious emanations. The Turks do not use coffins, and the horrible smell arising from their cemeteries is well known.

The author recommends for graves a minimum depth of 8 feet, and in the selection of land for cemeteries would make it a condition that neither rock nor water should come within 10 or 12 feet of the surface. If the soil is damp it should be deeply drained. We ask—Into what? Cemeteries are unfortunately often placed on eminences in the outskirts of cities, into which their drainage can and does descend. We could give instances of this in the suburbs of London, but to tell the truth is often libellous. Dr. Blyth also proposes that not more than one body should be placed in each grave, and that the length and breadth of each burial allotment should be 8 feet by 4. These recommendations, from a sanitary point of view, are very judicious, but they would necessitate the devotion of a very large area of land to the purpose of burial. This evil would be partially met by his proposal that after eighty years from the last burial the land should revert to secular—we hope he means agricultural—uses.

In speaking of smoke, the author declares that black smoke contains a much larger proportion of carbonic acid than ordinary smoke. Now, as carbonic acid is the product of the perfect combustion of carbon, we should expect that the blacker the smoke—i.e., the less perfect the combustion—the smaller would be the proportion of carbonic acid.

Sometimes the author seems to contradict himself. Thus, in the article on "plague," it appears to be considered as identical with the "black death" of the Middle Ages. But when we turn to "black death" it is there stated—"We are of Englada's opinion, that it was a distinct species, and that now it no longer exists."

On the subject of sewage treatment Mr. Blyth might, we think, have easily procured more complete and accurate information. Hille's (not Hill's) process, as carried on at Wimbledon and elsewhere, consists in the precipitation of sewage by lime, chloride of magnesium, and tar. The effluent from Whitthread's (not Whitbread's) process, being admitted to contain phosphoric acid and ammonia, can scarcely be termed inoffensive. Phosphate of ammonia has a very marked action in setting up putrefaction when brought in contact with organic matter, and its removal from sewage is hence of the highest importance. The account of the A B C process is at once imperfect and incorrect; the materials now used are basic sulphate of alumina, carbon, and clay, and the effluent water has been found to come within the standard of the Rivers Pollution Commissioners. Much of the evidence advanced in favour of irrigation can scarcely be pronounced worth the paper upon which it is printed.

On the subject of disinfection the author very judiciously remarks that there "never will be any one disinfectant which will be useful, like a quack pill, for every kind of contagion."

Under the head "Microscope" Mr. Blyth recommends the "Hartnack" as indisputably the best for the Medical Officer of Health and the Public Analyst.

In speaking of hospitals the author declares—"If the political, moral, and religious state of the ancients be examined, it is easy to be convinced that they had not, nor could they have, hospitals; for to found and maintain them a virtue was necessary which paganism was destitute of, namely, Charity; and it is to this virtue, wholly Christian, that we owe these buildings, which—begun in the first days of Christianity—have continued to our time." Now if Mr. Blyth will refer to Tennant's "Ceylon" he will find that in that island hospitals for men and for animals existed as early as 400 B.C.

To Medical Officers of Health it will doubtless prove very convenient to be thus furnished with what may be called an encyclopædia of professional knowledge and official obligations, and in spite of the shortcomings which we have indicated, and of others which want of space has compelled us to pass over, we think that the author has supplied a desideratum in our sanitary literature. The work is enriched with 140 illustrations, and with a map of the geographical distribution of disease.

CORRESPONDENCE.

DETERMINATION OF GLYCERIN AND SUCCINIC ACID IN WINES.

To the Editor of the Chemical News.

SIR,—I find in the CHEMICAL NEWS (vol. xxxi., p. 186) a new method of "Determination of Glycerin and Succinic Acid in Wines," by E. Maumené, taken from the French journal *Les Mondes*. I beg to inform you that this new analytical method was not found by Maumené, but such a process for determining glycerin and succinic acid is mine. Erroneously it was so stated in *Les Mondes* (No. 9, March 4, 1875), but in a subsequent number of the same journal the fault is corrected (See *Les Mondes*, No. 2, May 13, 1875, pp. 73 and 74).—I am, &c.,

I. MACAGNO.

Asti (Italy), March 18, 1875.

WATER ANALYSIS.

To the Editor of the Chemical News.

SIR,—The recent discussion upon this subject before the Chemical Society, though voluminous, is in some respects unsatisfactory. One especial feature in connection with it is the want of independent and impartial testimony in favour of the combustion process of Frankland and Armstrong. Of all the speakers four only (as reported in the CHEMICAL NEWS) can be said to have spoken in favour of this process, and of these four one (Dr. Armstrong) was one of the original discoverers of the method, and another (Mr. Thorp) is or was the chief chemist in the laboratory of the Rivers Commission, where Dr. Frankland and his method reign supreme. This want of testimony is no doubt to be accounted for by the fact that the process has not yet won its way to the appreciation of chemists; but this circumstance is in itself a powerful argument against the process. It has had the same chance as its rival, it is older than the ammonia process, and it possesses a powerful and eloquent advocate in its discoverer. What, then, is the cause of its non-success? The object of a water analysis usually is to answer the question—"Is this water fit for drinking?" And it is because the Wanklyn process enables us to answer this question far more readily than the Frankland process that it is more frequently employed. Until Dr. Frankland has proved not only that this method does not give a constant fraction of the nitrogen as ammonia, but also that it affords no indication of the purity of a water, he may rest assured that his more elaborate, if more exact, process will only be resorted to in rare cases where great accuracy is required.

And here, Sir, I have not questioned in any way the accuracy of the Frankland process. The experiments published by Dr. Frankland seem to show that under certain circumstances (in the absence of nitrates) it gives very accurate results; but it has still to be shown that in the hands of other chemists, and under different conditions, the same results can be obtained. The sulphuric acid formed by the oxidation of the sulphurous would certainly be imagined to affect the destruction of some of the organic matter present, but Dr. Armstrong and Dr. Frankland say that it does not,—so that this question is taken out of the region of theory into that of fact.

The existence of nitrates in a water is taken by Dr. Frankland as a proof of the previous contamination of that water by sewage matter, and their amount is considered proportional to the amount of such contamination. This position seems untenable, for nitrates are found in some of the most wholesome of waters, and they vary in quantity perhaps more than any other constituent. Also when we consider that their presence in a water may arise in two ways,—first, by the solution of previously-formed

nitrates, and second, by their formation in the water itself, —I fail to see how the amount can be a measure of the sewage from which they are formed in the second only of the above cases. But even allowing that they measure accurately the amount of previous sewage contamination, of what value is this knowledge? The sewage has now become oxidised and the organic nitrogen converted into inorganic, or that which was injurious previously is injurious no longer. Thus if the presence of nitrates is a proof of anything, it is that the water has become purified to a certain extent at least.—I am, &c.,

ERNEST H. COOK.

Bristol, March 17, 1876.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Seances de l'Academie des Sciences. No. 9, February 28, 1876.

Explosion of Gunpowder.—M. Berthelot.—The explosion of gunpowder yields at first all possible bodies, that is to say, all bodies which are stable under the conditions of the experiment; these are principally potassic sulphide, sulphate, and carbonate, carbonic acid, carbonic oxide, nitrogen, and watery vapour. These bodies originate in proportions, which vary with the local circumstances of mixture and of ignition. If they remain in contact for a sufficient time they undergo reciprocal actions capable of bringing them to a state corresponding with the maximum heat evolved, that is to say, the state of sulphate of potash and of carbonic acid.

Researches on a Sulphate which appears to contain a New Oxide of Manganese.—M. E. Fremy.—All chemists know the vinous-red liquid formed in the preparation of oxygen by the action of concentrated sulphuric acid upon the peroxide of manganese. This body is very unstable, being decomposed by heat and by water, and is produced in very small quantity, and only from certain samples of manganese. The author has succeeded in preparing it in larger quantity, and considers that it contains the sulphate of a new oxide, but reserves the details for a future communication.

Influence of Vegetable Mould upon the Nitrification of Azotised Substances of Organic Origin used as Manure.—M. Boussingault.—The author maintains that nitrification is due to vegetable mould, since in sand and chalk blood furnishes merely traces of nitrates.

Oil of Elæococca, and on its Solid Modification as Produced by the Action of Light.—M. Cloëz.—In the liquid oil the author finds elæolic and margarolic acids, apparently derived from two neutral proximate principles, elæolin and margarolin, the mixture of which forms the largest part, if not the whole, of the Elæococca. The solidified oil contains, on the contrary, stearolic acid, fusible at 72°, and containing—

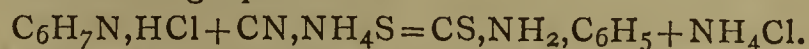
Carbon	74.30
Hydrogen	11.16
Oxygen	14.54

100.00

Certain Compounds of Titanium.—MM. C. Friedel and J. Guerin.—The authors describe the oxychloride, $Ti_2O_2Cl_2$; the sesquioxide, Ti_2O_3 , and the tetrachloride.

On Sulpho-phenylurea.—M. Ph. de Clermont.—The author obtains this compound by causing the hydrochlorate of phenylamin to react upon the sulphocyanide of ammonium in equivalent proportions at the heat of the

water-bath. Double decomposition takes place according to the following equation:—



Antiseptic Properties of Borax.—M. Schnetzler.—The author refers to the vast deposits of borax discovered by Mr. Robottom in Southern California, and recommends it as preferable to salicylic acid for the preservation of articles of food.

Reducing-Sugar Contained in Crude Sugars.—M. A. Müntz.—A brief controversial note.

Manufacture of Superphosphates.—M. A. Millot.—The author declares that superphosphates never go back if neither iron nor alumina is present, and if a sufficiency of acid is used. With phosphatic minerals containing superphosphates this is no longer the case. With the coprolites of the green-sand, and a quantity of acid sufficient to attack the carbonate and phosphate, the reversion is very considerable, and is complete in about two years. If the sulphuric acid is deficient the reverted phosphate is a mixture of phosphate of iron and of bicalcic phosphate. The acid phosphate of lime is decomposed during desiccation, and splits up into bicalcic phosphate and phosphoric acid, which combines with the oxide of iron. Alumina is never attacked by phosphoric acid in these products.

Moniteur Scientifique, du Dr. Quesneville,
March, 1876.

Industrial Preparation of Sulphuric Acid and Chloride of Lime in the South of France.—M. A. Favre.—The sulphuric acid is entirely obtained from pyrites, roasted in furnaces capable of revolving on their axle. The depth of the kiln is 1.80 m., and its surface 1.20 m. The pyrites are burnt down to 2 or 3 per cent. In many works the pyrites are burnt in too shallow layers, when the combustion becomes too brisk, and is necessarily incomplete. The kilns are not always well proportioned in number and power to the size of the lead chambers. The author approves of the following proportions:—180 cubic metres of chamber room to 1 square metre of surface of kiln grating, upon which are burnt every twenty-four hours 270 kilos. of pyrites at 40 per cent of sulphur, corresponding to 150 kilos. pyrites to 100 cubic metres of chamber room. He adds that the capacity of the chambers may be better utilised by cooling the gases before their entrance into the chambers. The chambers are generally erected in sets of three—a large one and two smaller ones following. The size most approved for the large chambers is 45 metres in length by 8 in width and 6 in height. The total cubic bulk of a set of three chambers is 4000 cubic metres. The sides are generally exposed to the weather, which makes it impossible to maintain a uniform temperature, and in summer the heat rises to 65°. The cost of 1000 kilos. of acid at 50° (Baumé?) is stated at 36.9 francs. In the manufacture of chloride of lime great care must be taken in burning the lime to be employed. If the heat is too great the lime is not easily slaked, and there results a dense mass, ill adapted to absorb the chlorine. If there are unburnt portions the evils are the same. If the lime is properly and uniformly burnt it is light and porous, and if the chlorine is evolved slowly, so as to prevent a great rise of temperature, a chloride of lime is obtained which marks 110° and which is permanent. The casks, he thinks, should not hold more than 250 kilos. each. The cost of 1000 kilos. chloride of lime at 110° he gives at 244.50 francs.

Reimann's Farber Zeitung,
No. 8, 1876.

According to Schwamborn, lyes from washing indigo-blue cloths are not easily precipitated with milk of lime. The precipitation is rendered easy if the glue present in solution is previously removed by the addition of a decoction of sumach. As is well known, alumina and iron are

not precipitated by alkalies if fixed organic matter is present in solution. Are the cases analogous?

According to G. Witz, of Rouen, each of the eighty printing-machines in France consumes daily 36 kilos. of egg albumen, equivalent to 12,000 eggs. In 1860 the print works of Alsace, with 100 machines, consumed 125,000 kilos. = 37,500,000 eggs, the yield of 250,000 hens. An ox yields 400 grms. of dry blood albumen; 1 kilo. of albumen represents $2\frac{1}{2}$ oxen, 10 sheep, or 17 calves.

MISCELLANEOUS.

Refrigeratory Processes.—In our translation of Dr. Hofmann's "Report on the Development of the Chemical Arts" we have laid before our readers an account of several processes for the manufacture of ice. It may therefore be interesting if we briefly notice the Reece's ice-machine, in which the combustion of 1 ton of coal is made to yield 20 tons of ice, at a total expense not amounting to five shillings per ton. Like Carré's machine, which we have already described, the apparatus in question is worked with ammonia. But there is a very important difference between the two processes. The Carré machine distils over weak ammoniacal gas, whereas the Reece machine makes it anhydrous. The gas produced by the former contains 25 per cent of water, which permanently dissolves and renders useless an equal weight of ammonia. One-third of the whole ammonia distilled is thus rendered useless—the fuel to distil it, the cold to refrigerate it, and the power to move it all being wasted. The consequences are, that ten times as much liquor is required to be kept in movement and pumped over and over again into the boiler against a very high pressure, four times as much water is required to cool the distilled vapour, and ten times as much power is required to drive the machines. They have, consequently, to be made much larger or more numerous, and are thus dearer in first cost, as well as infinitely more expensive to work than Reece's. This system, therefore, seems to surpass that of Carré as decidedly as that in turn excels the ether machine, and for the present, at all events, Reece's process must be considered the most practical and efficient known. The machine can be managed by an unskilled workman, and the escape of ammonia is imperceptible—an important point, both as regards economy and comfort. The importance of having at command a means of producing ice in quantity, or of reducing at will the temperature of liquids and solids is indisputable. In our chilly summers, indeed, ice can never play the part which it does in sunnier regions, but in the importation and preservation of fresh meat, milk, and various other articles of food, it has before it a wide future. In various chemical manufactures, also, a ready and economical means of lowering the temperature of liquids has long been desired. Several of the machines are now in operation in London, and the quality of the ice we have seen seemed to be equal to the finest specimens of foreign ice.

PATENTS.

ABRIDGMENTS OF PROVISIONAL AND COMPLETE SPECIFICATIONS.

Improvements in the means for producing gas from an admixture of atmospheric air with paraffin, petroleum, naphtha oil, or spirit containing a large amount of carbon, the said gas being adapted for lighting or heating purposes. J. Mather, Gateshead-on-Tyne, Durham. January 20, 1875.—No. 211. Atmospheric air rushing through a jet produces a vacuum in a small chamber or generator, into which also opens a jet from a vertical tube descending into a cistern containing the carboniferous fluid. This vacuum causes the fluid to ascend the tube and to issue from the jet in a spray, which combines with the air rushing by. This combination is carried forward along a tube that passes through a furnace or oven, and finally conveyed in a gaseous state to the hydraulic main, and thence to store tanks.

Improvements in the disinfection of solid and liquid matters, such as night-soil sewerage, either together or separately, gas, and other foul waters. J. Box, Great George Street, Westminster, E. Aubertin, L. Boblique, and H. Leplay, all three of Paris. January 20, 1875.—No. 214. We employ a soluble alkaline phosphate and a soluble salt of magnesia and oxide or other form of iron. We employ these agents in different combinations. We prefer phosphate of potash or ferruginous soda prepared by calcination in a reverberatory furnace, the phosphate of iron with the sulphate of soda and a soluble salt of magnesia, such as the chloride of magnesium. The liquid to be disinfected is allowed to run through the raw products as they leave the reverberatory furnace; the sulphuretted hydrogen is deposited in the state of sulphate or iron. To the liquid thus deprived of the sulphuretted hydrogen soluble salt of magnesia is added; magnesium phosphate of ammonia is formed, and precipitates itself with the azoted organic matters. To disinfect solid matters the same products are used dry in powder. We also employ phosphate of lime dissolved in hydrochloric acid, and oxide of iron also dissolved in chlorhydric acid.

Improvements applicable in the treatment and manufacture of metals and their alloys. W. H. Smith, Edgbaston, Birmingham, Warwick. January 20, 1875.—No. 217. This invention consists mainly in the improvement and purifying of iron, steel, copper, lead, zinc, nickel, and their alloys, and treating and manufacturing the same in prescribed manner in converters, cupolas, receivers of metal, or any other suitable vessel or vessels; and consists of introducing into or upon iron, steel, copper, lead, zinc, nickel, and their alloys, when in the above-named vessels and in a molten or liquid state, alumina, lime, soda, potass, and baryta, either mixed or separate, with oxides of iron, manganese, titanium, vanadium, tungsten, and chromium, either manufactured or in the natural metallic form; and I use them either singly or together, and in such proportion or proportions as may be required, with or without solid or gaseous carbons. It also consists in an improved arrangement of tuyeres as applied to such converters or vessels.

Improvements in the manufacture of steel. A. M. Clark, Chancery Lane, Middlesex. (A communication from J. Eyquem, Paris.) January 22, 1875.—No. 256. The invention consists in the use for cementing iron on a large scale of ammoniacal and carburetted hydrogen gases acting simultaneously, and produced by the decomposition of a mixture of sal ammoniac and a substance such as peat, tan, and other substances, or liquids furnishing proto or bicarburetted hydrogen. It also consists in the manufacture by the above means of cast-steel from scrap-iron, ores, refining-slag, and other products.

NOTES AND QUERIES.

Calcium Phosphates.—On page 365, under the heading "Calcium Phosphates," of Fownes's "Manual of Chemistry," it is mentioned that there are two tri-basic phosphates, $\text{Ca}_3\text{P}_2\text{O}_8$ and CaHPO_4 , and that when the first phosphate is digested with ammonia, that it is converted into the second. Is it therefore meant that by the addition of ammonia to an acid solution of $\text{Ca}_3\text{P}_2\text{O}_8$, that CaHPO_4 is precipitated, and not $\text{Ca}_3\text{P}_2\text{O}_8$? I have always understood that tri-calcic phosphate was precipitated, and that it could be filtered off, dried, ignited, and weighed as such.—AMATEUR.

MEETINGS FOR THE WEEK.

- SATURDAY, 26th.**—Physical, 3. "On Salt Solutions and Attached Water," by Prof. F. Guthrie, F.R.S. "On some Problems connected with the Flow of Electricity in a Plane," by O. J. Lodge.
- MONDAY, 27th.**—Medical, 8.
— London Institution, 5.
— Royal Geographical, 8.30.
— Society of Arts, 8. Cantor Lectures. "Wool Dyeing," by George Jarman.
- TUESDAY, 28th.**—Civil Engineers, 8.
— Anthropological Institute, 8.
— Royal Institution, 3. "On the Classification of the Vertebrated Animals," by Prof. Garrod.
— Society of Arts, 8. (African Section). "The Industries of South Africa," by T. B. Glanville.
- WEDNESDAY, 29th.**—Society of Arts, 8. "Model Dwellings for the Rich," by T. Roger Smith and W. H. White.
- THURSDAY, 30th.**—Royal, 8.30.
— Royal Institution, 3. "Polarised Light," by Mr. Spottiswoode.
— Chemical, 8. (Anniversary).
— Philosophical Club, 6.
— London Institution, 7.
- FRIDAY, 31st.**—Royal Institution, 9. "Physiological Action of Light," by Prof. Dewar.
— Society of Arts, 8. (Chemical Section). "The Methods of Estimating the Illuminating Power and Purity of Coal Gas," by A. Vernon Harcourt, M.A., F.R.S.
- SATURDAY, April 1st.**—Royal Institution, 3. "Wagner," by Mr. Dannreuther.

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EXERCISES in ELECTRICAL and MAGNETIC MEASUREMENT, with Answers. By R. E. DAY, M.A., Author of "Numerical Examples in Heat."

London: LONGMANS and CO.

THE CHEMICAL NEWS.

VOL. XXXII. No. 853.

ON REPULSION RESULTING FROM RADIATION.*

By WILLIAM CROOKES, F.R.S., &c.

PART IV.

IN this paper the author describes experiments on the repulsion produced by the different rays of the solar spectrum. The apparatus employed is the horizontal beam, suspended by a glass fibre and having square pieces of pith at each end coated with lampblack. The whole is fitted up and hermetically sealed in glass, and connected with an improved mercury pump. In front of the square of pith at one end a quartz window is cemented on to the apparatus, and the movements of the beam, when radiation falls on the pith, are observed by a reflected ray of light on a millimetre scale. The apparatus was fitted up in a room specially devoted to it, and was protected on all sides, except where the rays of light had to pass, with cotton-wool and large bottles of water. A heliostat reflected a beam of sunlight in a constant direction, and it was received on an appropriate arrangement of slit, lenses, prisms, &c., for projecting a pure spectrum. Results were obtained in the months of July, August, and September; and they are given in the paper graphically as a curve, the maximum being in the ultra-red, and the minimum in the ultra-violet. Taking the maximum at 100, the following are the mechanical values of the different colours of the spectrum:—

Ultra-red	100
Extreme red	85
Red	73
Orange	66
Yellow	57
Green	41
Blue	22
Indigo	8½
Violet	6
Ultra-violet	5

A comparison of these figures with those usually given in text-books to represent the distribution of heat in the spectrum, is a sufficient proof that the mechanical action of radiation is as much a function of the luminous rays as it is of the dark heat-rays.

The author discusses the question "Is the effect due to heat or to light?" There is no real difference between heat and light; all we can take account of is difference of wave-length, and a ray of a definite refrangibility cannot be split up into two rays, one being heat and one light. Take, for instance, a ray of definite refrangibility in the red. Falling on a thermometer it shows the action of heat, on a thermo-pile it produces an electric current, to the eye it appears as light and colour, on a photographic plate it causes chemical action, and on the suspended pith it causes motion. But all these actions are inseparable attributes of the ray of that particular wave-length, and are not evidences of separate identities.

The author enters into some theoretical explanations of the action of the different parts of the spectrum, but these cannot well be given in abstract.

An experiment is given by which sunlight was filtered through alum, glass, and water screens, so as to cut off the whole of the ultra-red or dark heat-rays. The ray of light which was thus freed from dark heat was allowed to

fall on the pith surface of the torsion apparatus, when it produced a deflection of 105°. When a solution of iodine in disulphide of carbon was now interposed the deflection fell to 2°, showing that the previous action was almost entirely due to *light*. With a candle tried under the same circumstances, the light filtered from dark heat produced a deflection of 37°, which was reduced to 5° by interposing the opaque solution of iodine.

In order to obtain comparative results between disks of pith coated with lampblack and with other substances, a torsion apparatus was constructed, in which two or more disks could be exposed one after the other to a standard light. One disk always being lampblackened pith, the other disks could be changed so as to get comparisons of action. Calling the action of radiation from a candle on the lamp-blackened disk 100, the following are the proportions obtained:—

On Lampblackened pith	100°
Iodide of palladium	87·3
Precipitated silver	56
Amorphous phosphorus	40
Sulphate of baryta	37
Milk of sulphur	31
Red oxide of iron	28
Scarlet iodide of mercury and copper ..	22
Lampblackened silver	18
White pith	18
Carbonate of lead	13
Rock-salt	6·5
Glass	6·5

In consequence of some experiments tried by Profs. Tait and Dewar, and published in *Nature*, July 15, 1875, the author fitted up a very sensitive apparatus for the purpose of carefully examining the action of radiation on alum, rock-salt, and glass. The source of radiation was a candle. Perfectly transparent and highly polished plates of the same size were used, and the deflection was made evident by an index ray of light. Taking the action on the alum at 100, that on the rock-salt in five successive experiments was 81, 77·3, 71, 62·5, 60·4. This increasing action on the alum was found to be caused by efflorescence, which took place rapidly in the vacuum, and rendered the crystal partially opaque. A fresh alum plate being taken, this and the rock-salt were coated with lampblack and replaced in the apparatus, the black side away from the source of radiation, so that the radiation would pass through the crystal before reaching the lamp-black. The action of radiation was in the proportion of blacked alum 100, to blacked rock-salt 73.

EARLY USE OF CHLORINE FOR BLEACHING PURPOSES.

By G. A. KEYWORTH, F.C.S.

AFTER the discovery of chlorine by Scheele in 1774, its power of destroying vegetable colours seems to have remained without practical application until 1785, when Berthollet drew attention to this property. The merit of first employing chlorine industrially at bleach works belongs to Mr. Robert Hall, who resided at Basford Hall, near Nottingham. He had long been an able and ingenious experimenter, and was a man whose character was singularly beautiful. Well read in the chemical literature of that period, *i.e.*, the *Annales de Chimie*, *Nicholson's Journal*, and the writings of Black, Scheele, Lavoisier, and Berthollet, he corresponded with Henry of Manchester, and with Priestley of Birmingham. The former had, it is said, received from Germany a phial of chlorine water, then called oxygenated muriatic acid, and to have sent it to Mr. Hall, pointing out the bleaching which the cork had undergone. Mr. Hall lost no time in trying the

* Abstract of a paper read before the Royal Society, Feb. 10, 1876.

effect of the new agent in a neighbouring bleach yard belonging to his brother-in-law, Mr. White. Here, in a place still called from the circumstance "Bedlam," the first failures and successes occurred. Much too strong a solution turned the fabric to tatters, but by degrees the new agent became manageable and successful. My father, whose early days were spent with these relatives, often spoke of the experiments, and during his life took occasion to mention them to chemical lecturers and others. The use of lime by Tennant, of Glasgow, as an absorbent of chlorine, in 1798, seems to have overshadowed these interesting early results.

The facts, however, are stated in "Self Help," by Mr. Smiles. The author of a paper on "Bleaching" in the useful Stanford Series of "British Manufacturing Industries," does not allude to them.

Assisted by Mr. Fox, whose family still carry on the bleach works, Mr. Hall made starch there, and chlorine was employed to bleach it by his son, Samuel, afterwards the inventor of the well-known Hall's Condenser for Marine Engines.

Marshall Hall, the distinguished physiologist and physician, was another son of Mr. Robert Hall.

REPORT

ON THE

DEVELOPMENT OF THE CHEMICAL ARTS DURING THE LAST TEN YEARS.*

By Dr. A. W. HOFMANN.

(Continued from p. 125.)

THE first intelligence concerning the apparatus here described is contained in a communication by Carré to the Paris Academy, December, 1860.† The inventor's English patent bears the date October 15, 1860. Not long after, in January, 1861,‡ MM. Tellier, Budin, and Hausmann, sen, claimed priority in the invention, which they had patented in July, 1860.¶ For machines on the small scale they recommended at the same time sulphurous acid instead of ammonia, since, though less soluble in water, it requires only half the pressure. The courts of law appear, however, not to have decided the question of priority in favour of these gentlemen, as their name does not occur in connection with the further development of the machine. We find, however, that Tellier, in the year 1862,§ issued a report in which he recommended ethylamin and methylamin for use on the large scale instead of ammonia. The vapour of the latter is absorbed by water in double the volume of ammonia and possesses a very slight tension, so that the internal pressure in the apparatus scarcely exceeds one atmosphere. Hitherto, however, we do not learn that machines for the application of these substances have come into use. Since Tellier has recently—as we have mentioned above—constructed an air-pump machine for methylic ether, it may be suspected that peculiar difficulties have been found in the utilisation of these amines. A detailed description of Carré's continuous machine with illustrations has been published by Pouillet.¶ An illustrated account of the small intermittent apparatus may be found in the *Württemberg Gewerbeblatt* for 1861, No. 40, and has been copied into other journals.** In

* "Berichte über die Entwicklung der Chemischen Industrie Während des Letzten Jahrzehends."

† Carré, *Comptes Rendus*, li., 1023.

‡ Tellier, Budin, &c., *Comptes Rendus*, lii., 142.

¶ *Dingler, Polyt. Journ.*, clx., 23 and 120.

§ Tellier, *Comptes Rendus*, liv., 1188. *Dingl. Polyt. Journ.*, clxv., 450.

¶ Pouillet, *Bull. Soc. d'Encouragement*, 1863, 32. *Dingl. Polyt. Journ.*, clxviii., 171.

** *Dingl. Polyt. Journ.*, clxiii., 182.

1868, the author drew up for the *Badener Gewerbezeitung*, a paper on ice machines based upon experiments undertaking with the small machines then known.

In this essay Carré's small apparatus is described and illustrated by diagrams. The larger, or 2 kilos. size, yielded 2½ kilos. ice with a consumption of ¾ kilo. of wood charcoal, the time of heating being 80 minutes, and that of freezing two hours. Ingenious and effective as is this apparatus, it cannot be recommended for household use, as its manipulation requires too much technical skill. In the *Badener Gewerbezeitung* for 1869, followed an account of the machines for manufacturing purposes, in which Carré's large machine is described and figured. It was then and there announced that two German firms, Kropff and Co., of Nordhausen (since 1867), and Vaas and Littmann, of Halle on the Saale (since 1868), have taken up the manufacture of ammonia ice machines on Carré's principle. The former of these firms has now become a joint stock Ice Machine Company. According to the most recent quotations, both these establishments furnish the larger apparatus in five sizes, yielding from 25 to 500 kilos. of ice per hour at the price of 4800 to 30,000 reichsmarks (say from £240 to £1500). The Nordhausen Company manufacture also a small apparatus, for 7½ kilos. per hour, for 2250 marks (£112 10s.). According to their statements 1 kilo. of coal, according to the size of the machine produces from 6 to 16 kilos. of ice.

(To be continued.)

ON A NEW DECOMPOSING FURNACE PATENTED BY MESSRS. JONES AND WALSH.*

By R. C. CLAPHAM, F.C.S.

THE best methods of decomposing salt with sulphuric acid have long attracted the attention of manufacturers, for it is admitted on all sides that those adopted in practice have been unsatisfactory—leading to expenses from the partial stoppage of work by the breakage of pans, and also to a waste of sulphuric acid.

When the present system of making soda was commenced on the Tyne in 1820 the chemical trade was in its infancy, and small lead pans lined with brickwork were used to decompose salt, a charge of 2 cwts. of salt was taken, and sulphuric acid was slowly poured upon it from a carboy through a hole in the roof of the furnace, and it took three hours to complete the operation. But as these lead pans were so liable to be injured, a fire-brick furnace was substituted in 1828, which was then looked upon as an improvement, and was well known locally as "The Dandy Furnace." At that time no attempt was made at the condensation of the hydrochloric acid fumes, and they passed directly into the atmosphere.

A great improvement upon the above was introduced in 1840 by the late Mr. John Lee, who applied a metal pan about the same size as we now use, and its adoption by the trade has led, amongst other advantages, to a large saving of sulphuric acid. This pan has, however, been always liable to frequent breakages from negligence of workmen and other causes, and to get over this difficulty various substitutes have at times been tried. In 1860 I had some fire-clay pans made at the Scotswood Brick Works, which were used at the Walker Alkali Works. They were found to work pretty well, but were difficult to heat, and were at last given up.

If we grant that the life of an ordinary decomposing pan is long enough to turn out 2000 tons of salt (and many of those present know that in many cases 1000 tons is a fair quantity), I calculate that, on the Tyne alone, not less than £5000 per annum is spent in the renewal of decomposing pans; and when we take into account the loss of

* Read at the Newcastle Chemical Society, March 23, 1876.

time caused by the breakage, the extra labour required, and a waste of acid, the money loss under these heads will be very considerable also.

In the face of these facts there would appear to be ample room for improvement. Messrs. Wm. Jones and John Walsh, of Middlesbro', have contrived and erected a new kind of decomposing furnace which completes the operation in one furnace. It has now been at work several months, and from an examination of the plates forming the dish of a furnace which was laid off a few days ago, I found the plates as fresh and good as when erected.

The furnace now in actual work, and from which fixed and regular results are obtained, consists of a metal dish divided into six segments, all tightly fixed together. The metal is $2\frac{1}{2}$ inches thick, and the whole rests on solid brickwork. The diameter is 15 feet, and the charge of salt is about 12 tons each twenty-four hours; so that a furnace of this capacity will turn out about 80 tons sulphate of soda in six days. The batch is kept in constant motion by means of two cross metal arms worked from a centre shaft, to which are fixed paddles and rakes, and the whole mass is much more completely mixed than in a hand-worked furnace, and the sulphate produced is of a very fine and uniform character. Mr. Walsh, who is present, has kindly undertaken to explain the furnace more fully from the working model on the table and from the plans exhibited.

The motion supplied to the crown wheel is got from a donkey-engine, and a small engine with a 6-inch cylinder is found to be sufficient to work the furnace. At present the batch is both charged into the furnace and discharged by hand, but Mr. Goodman expects to perfect a plan to do this by machinery.

The advantages to be derived from this new furnace may be described as a more regular daily means of working; free from sudden stoppages by breakage of the present pans; a direct saving in wages, as only one workman is required to each shift for charging and working the furnace; a saving of fuel, as the heat required is less than in the old furnace; and 3 cwts. of coke per ton of sulphates is used in the place of 9 cwts. coal ($=5\frac{1}{2}$ cwts. coke) by the old furnace; a saving, also, of sulphuric acid, calculated by Mr. Walsh to be equal to about $1\frac{1}{2}$ to 2 per cent. Then, again, owing to the complete mixing and better working of the batch by machinery, the great inconvenience felt by the public living in the neighbourhood of chemical works by the batches being frequently drawn by the workmen in a partly finished state, and containing free hydrochloric acid, is entirely avoided. The fumes being emitted in a gradual and uniform manner throughout the working of the batch the condensation is more perfect, and as the whole of the gas is passed through one coke tower it is condensed into hydrochloric acid of 26° to 28° T., hot. It therefore appears that both in an economical and sanitary point of view this furnace is an improvement upon its predecessors.

In the description I have given I have purposely confined myself to a furnace in actual daily work, and from which known results have been obtained; but there is a fair probability that experience will show that not less than 100 tons weekly will be turned out of each furnace.

ACTION OF PEROXIDE OF HYDROGEN UPON FATTY OILS.

By S. COHNÉ.

By the action of HO_2 upon fatty oils they become separated into the two distinct classes known as drying and non-drying oils. Though HO_2 does not exhibit any action upon the latter description it acts powerfully upon the first kind. When a few drops of a weak solution of HO_2

(if containing only half a volume) are mixed and shaken with a drying oil—such, for instance, as linseed, nut, cotton-seed, poppy, &c.—linolic or palmatine acid are immediately separated from it, which, if put into a basin to settle, the linolic acid subsides to the bottom in the form of a greasy mass, while the palmatine acid sets in fine sheets upon the top of the oil. The remaining fluid oil loses its property of a drying oil, and becomes a non-drying oil.

Castor oil, after treatment with HO_2 , does not then so readily dissolve in alcohol, and when dissolved in sufficient quantity of alcohol it will be found, if thrown on paper, that it will not dry up: consequently HO_2 is an easy test. If olive oil is adulterated with cotton-seed oil, this being a cheaper article, it may easily be detected, even if the adulteration is less than a quarter per cent, as the oil immediately becomes thick and dull.

The HO_2 appears to act upon the oil somewhat as sulphuric acid does upon alcohol, that is, the HO_2 is not decomposed, and when the solution of HO_2 is allowed to settle, and is afterwards drawn from the oil, it can be used again and again, and will continue to act upon a fresh quantity of oil with a like result.

The weak solution of HO_2 may remain for months under oil without being decomposed, even though heated up to 100° F.; similarly, as Saussure has found, that a layer of nut oil, if enclosed with oxygen gas, absorbs in eight weeks in the shade only three times its bulk of that gas. As drying oils are usually much cheaper than non-drying oils, advantage may be taken of the foregoing facts to convert the drying into non-drying oils for lubricating purposes.

NOTE ON THE ANALYSIS OF MINERAL PHOSPHATE OF CALCIUM.

By DR. B. W. GERLAND.

THE "Oxalic acid method" is pointed out in the "Report on the Methods Employed in the Estimation of Potassa and Phosphoric Acid in Commercial Products, &c." (CHEM. NEWS, vol. xxxii., p. 172) as one of the most recommended. Its advantages are, however, considerably reduced when, as is mostly the case, iron and aluminium are present, and the choice is left between precipitating these as phosphates with an alkaline acetate before adding the oxalate, and the use of the citric acid for keeping them in solution. If the former recommendation is adopted, the difficulty is to obtain the iron and aluminium phosphates completely separated, and at the same time free from calcium phosphate; and if citric acid has been used the precipitation of calcium oxalate is incomplete, the magnesium ammonium phosphate is liable to be accompanied by basic magnesium citrate, and the estimation of iron and aluminium becomes inconvenient.

The precipitation of the phosphate of iron and aluminium from their acid solution by alkaline acetate is prevented by the presence of oxalic acid. On the strength of this fact the "oxalic acid method" can be modified, so that it becomes expeditious and accurate. The operations can be conveniently carried out in the following manner:—The properly-prepared solution of the weighed sample in hydrochloric or nitric acid is neutralised as much as possible without forming a permanent precipitate, heated to boiling, and oxalate added in small excess. If the dilution is already sufficient, oxalate of ammonium may be added in crystals. Acetate of sodium (or ammonium) is added in sufficient quantity to take up the free mineral acid, and the liquor is removed from the fire. The calcium oxalate settles rapidly as a heavy granular powder. The liquor, which appears clear whilst hot, becomes turbid on cooling, but after two hours' rest is again clear. The filtration of the calcium oxalate can now be proceeded with; it requires very little time. The precipitate is free

from iron and aluminium phosphates, which is readily proved by dissolving the calcined residuum in hydrochloric acid, and adding ammonia, when no precipitate will be formed.

From the filtrate and wash-waters of the calcium oxalate iron and aluminium are to be separated. Ammonia alone does not effect it completely, boiling assists it, and the addition of bromine-water still more; but, to make the separation complete, it is advisable to add ammonium sulphide, and allow the sample to stand in a warm place until the liquor has cleared itself, and assumed a bright yellow colour. It is then filtered with the known precautions. The precipitate is generally free from magnesia, particularly if ammonia was not added in too great excess, but contains, besides aluminium phosphate and iron sulphide, a not insignificant quantity of silica, even if the solution has been previously evaporated to dryness. Phosphoric acid retains silica with a tenacity similar to the vanadic acid. For the analysis of the iron and aluminium precipitate the molybdanic acid method is the most convenient. Instead of using ammonium sulphide, the liquor may be treated with chlorine, or evaporated with sodium carbonate, and the residuum calcined for the destruction of the oxalic acid. The filtrate from the ammonium sulphide precipitate is to be concentrated, and the magnesia precipitated with part of the phosphoric acid by ammonia. Lastly, the remaining phosphoric acid is separated by magnesia mixture.

With these modifications the "oxalic method" compares favourably in point of convenience with Sonnenschein's, and yields results no less accurate.

Macclesfield, March, 1876.

ON A REMARKABLE CASE OF MILK ADULTERATION.

By SYDNEY GIBBONS, F.C.S., Melbourne, Victoria.

I VENTURE to apply the term "remarkable" to the case in question, because it is almost unique, being not only the first in my experience, but in addition only the second on record.

Almost all writers on adulteration mention many substances which they state to be used, but which each in turn admits that he has never detected. If this is found in the writings of professional investigators, can we be surprised if thoughtless and ill-informed paragraphers indulge, from time to time, in such intimations as that brandy is made with vitriol, champagne from petroleum, and fresh butter from river mud. Conspicuous among the instances of this blind faith in the unseen is the notion, constantly repeated, that milk is doctored or manufactured with sheep's brains. It is uniformly mentioned in "the books," but all of any repute admit the absence of foundation. The only evidence existing is that given by the late Prof. Quekett to a Committee of the House of Commons, in 1856. He stated that in one case, occurring five or six years before, he "found a sediment composed of nervous matter," but that since that time he had not been able to detect anything of the kind. In illustration, he submitted drawings of cow's milk, with and without the admixture of calves' brains. These drawings have passed into history, and are quoted by Dr. Hassall, who, though admitting that he has never found such an adulteration himself, gives an engraving of the microscopic appearance of the mixture, without, however, stating whether it is drawn from his own synthetic experiment or is a copy of Quekett's sketch.

My sample had been delivered the same day by a milkman serving a suburban hotel, at which complaints of the quality of the milk delivered had for some time been frequent.

At last it was bottled at once, much to the chagrin of

the milkman, who, when informed that it would be forwarded to me for analysis, was eager to recover possession this, of course, was not allowed.

As received, the sample had separated into three layers, of which the upper was thick and of a brownish dirty grey colour; the bottom resembled it, but was less in volume; and the intervening portion was white and thin. On being shaken it resumed, without frothing, its thick and pulpy consistency, but soon separated again. Its density in this state was 1022, but in five hours fell by dissociation to 1016.

On standing it yielded, as before, an upper layer of thick grey pulp, of which most was at the top. It differed from cream, and showed but few large cream-cells. In amount it was, after standing the usual time, one-fifth of the entire column; but on allowing this to remain, I found the measurement to alter materially, and indeed the proportions subsisting between the several layers were by no means constant. The lower portion was darker, and included much dirt, with sand and vegetal *débris*; on its surface torulæ abounded. Throughout, and chiefly in the grey pulp, was much granular and clotted matter, which "took" magenta, and was thereby distinguished from the normal fat globules. There was no added starch, and nothing that depolarised light except the sand, some cuticles of grasses, and a few granules of their starches.

Many singular masses of the same type, but of varying size and form, somewhat resembling bean pods, having a length of from two to six times their width, behaving optically like fat corpuscles. Others were larger, irregular and branched, clear, structureless, not polarising, comparable in appearance to the vacuoles that form when a viscid medium dries up between glasses. Also many resembling caudate brain-cells (though not of the acute-angled and stellate forms), without apparent nuclei, not definitely tubular, and without contents. Some large transparent round cells or oil-globules, not natural. These bodies, and a large part of the mixture which had not previously shown structure, took magenta freely, and then revealed fragments of what appeared to be nerve tubules. A "large" piece, longer than the diameter of the field of view (0.015"), consisted of a ramification of tubules proceeding in the same direction, as having been normally parallel, but now partly spread radially, and connected by a fine transparent structureless tissue. Of this two sketches were made at different times. A notable feature was a sharp straight line, obviously a clean cut, which bounded it.

All these indications seemed to point to nerve structure, as of cerebral matter, and to nothing else. The next thing was actual comparison under both conditions. I procured some brains, and, after preparing an emulsion, got a series of similar and accordant observations. There was, however, this marked difference:—I did not succeed in producing a pulp so smooth as the milkman's, and always had a conspicuous amount of unbroken and unmistakable structures; whereas in the milk they had to be sought, but, saving the particular forms mentioned, they were to be found. It is probable that special precaution had been taken to break down as much as possible of the tissues by long digestion, and to strain off what proved intractable.

Besides these experiments, I brought into comparison published figures of the structures without number, and with singular agreement. For example, Hassall's drawing of the mixed milk might almost have done for one of my fields, except that I found more than he figures; and I find in the "English Cyclopædia," article "Nervous System," fig. 3, a fascicle of nerve fibres, which, if cut in half transversely, will give a very good rough idea of my fascicle described above. But my sketched example of this fascicle of tubules with their sheaths, sharply cut off, and the observations with magenta, are pretty conclusive evidence, and throw strong light on the other appearances.

There is, then, no room for regarding this otherwise

than as a veritable case of the adulteration of milk with brains, probably sheep's (which I long considered mythic), and the solitary successor of Prof. Quekett's memorable case in 1850.

It may be observed that I have refrained from using any of the expletives with which sensational accounts of such matters are sometimes garnished. I know that some accomplished analysts have, while declaring their opinion that this particular kind of fraud was not practised, stigmatised it as "disgustingly horrible." But I see nothing more necessarily disgusting in this use of brains than in its service with calves' head. And it certainly bears a closer dietetic relation to milk than some of the recognised substitutes; albeit, containing as it does substances more nearly resembling flesh food than those of milk alone, it might be too strong a food for infants.

But a preparation stealthily made for fraudulent purposes is not compounded with all the nicety of a well-appointed kitchen, and dirt is by no means the worst evil likely to be associated with such an adulteration. It is hardly likely that the proceeding will now be repeated; still I regret that it is not in my power to publish the name of the present offender.

ON SOME RESEARCHES ON THE PURIFICATION OF CAST-IRON.

By SERGIUS KERN, St. Petersburg.

THE sample of cast-iron which was used during these experiments gave on analysis the following average composition:—

	Per Cent.
Silicon	2.76
Graphite	4.58
Carbon	1.20
Iron	90.40
Manganese	0.90
Sulphur	0.16
	100.00

This sample was melted in a small cupola, and the ingots obtained were analysed. The analysis showed that the amount of silicon and sulphur diminished and that a part of graphite was transformed into carbon. The results of this analysis are as follows:—

	Per Cent.
Silicon	1.50
Graphite	3.23
Carbon	1.98
Sulphur	0.05
Iron, &c.	93.24
	100.00

A second experiment was made; the sample of cast-iron was melted with a mixture of crude soda (1 per cent) and manganese dioxide (0.5 per cent). It was found that the resulting metal contained the following amount of silicon and sulphur:—

	Per Cent.
Silicon	0.58
Sulphur	0.02
Iron, &c.	99.40
	100.00

It was found well to add to the melted mixture a small quantity, about 3 per cent, of fluor spar. Some metallurgists have proposed the use of soda for refining cast-iron; my experiments showed me that a modification of all these processes may be proposed, consisting of melting and refining the metal at the same time. The

metal is melted in puddling or refining furnaces, and when the metal is fluid, into the furnace is thrown a mixture of 3 per cent of fluor spar, 1 per cent of crude soda, and 0.5 per cent of manganese dioxide for every 100 parts of the melted cast-iron. This process gave very good results.

MODIFICATION OF WANKLYN'S METHOD OF WATER ANALYSIS.

By H. B. CORNWALL, E.M.

To shorten the Nesslerising I keep on hand five bottles containing solutions in the proportion of 20, 15, 10, 5, and 2.5 c.c. of dilute standard ammonia solution (Wanklyn's "Water Analysis," 3rd ed., p. 25), filled up to 50 c.c. each with pure water, and then mixed with 2 c.c. of the Nessler reagent. I keep about 100 c.c. of the respective solutions in each bottle; they absorb ammonia, and become darker on use, but very slowly indeed.

Suppose it is required to determine the amount of ammonia in 50 c.c. of a distillate obtained by Wanklyn's ordinary method. It is collected in a test cylinder, and the usual 2 c.c. of the Nessler reagent run into it. After a minute or two, by pouring 52 c.c. from one or another of the comparison bottles into a second test cylinder, the amount of ammonia in the distillate can be roughly estimated by comparing the depth of color of the two liquids. If it is found that the distillate contains more ammonia than the solution made with 5 c.c. of ammonia solution, and less than that with 10 c.c. the comparison solution is returned to its bottle, and 10 c.c. of the above dilute ammonia solution, referred to on p. 25 of Wanklyn's book, is run into the second test cylinder, which is then filled to the 50 c.c. mark with pure water, 2 c.c. of Nessler reagent added, the whole stirred, allowed to stand five minutes, and then transferred to a burette in which tenths of a cubic centimetre can be accurately read. Now by running enough of this solution into the second cylinder to produce a colour as deep as that in the first and measuring the amount required, it is easy to ascertain by a proportion how many cubic centimetres of dilute ammonia solution have been required to produce this effect; in other words, how much ammonia is contained in the 50 c.c. of distillate. Suppose that 44.2 c.c. had been run out of the burette, then $52 : 44.2 :: 10 : 8.5$.

The standard solution referred to in Wanklyn's work contains 0.01 milligramme of ammonia in one cub. centimetre, and the supposed 50 c.c. of distillate would therefore contain 0.085 milligramme.

The following are the results of the first six experiments made to test this method, the numbers representing milligrammes of ammonia per 50 c.c. of solution, as usual:—

0.0939	instead of	0.094
0.1827	" "	0.179
0.1802	" "	0.179
0.0183	" "	0.017
0.0513	" "	0.050
0.0501	" "	0.050

By this means in ten or twelve minutes, at one operation and with a consumption of only 50 c.c. or less of water free from ammonia, the Nesslerising may be accomplished with an accuracy which, unless by lucky accident, one could not equal in half a dozen of the usual trials, each requiring eight or ten minutes and consuming 50 c.c., or a little less, of pure water. The test cylinders employed are 2.7 centimetres in diameter, of very thin glass, with perfectly flat bottoms, and certainly give more accurate results than I can obtain with the wider ones recommended by Wanklyn; per-

haps more accurate than are necessary for water analyses.

I have obtained quite good results by using the solution from my comparison bottles at once in the burette, but they grow darker with use. There is no need of cleaning the burette more than once in a morning, even if solutions of varying strengths are used, and unless the air of the laboratory is too bad for any such work, the solutions made up for use at any particular time may be used for all determinations that day, for which they are suitable, if kept meanwhile in tight hottles.

I have tried and am still trying to replace the comparison solutions by imitations of the yellow colour produced by mixing solutions of ferric chloride or platinic chloride with cobaltic nitrate, or by solution of burnt sugar, but with only partial success as yet. It is easy to imitate the depth of colour produced by a certain amount of ammonia, but on diluting the imitation liquids they do not give proportional results. In judging of the colours in the test cylinders, I prefer to hold them so that the bottoms of the cylinders are half over a white and half over a black surface; the dark part seems to heighten the contrast between the lighter portions.—*American Chemist.*

PROCEEDINGS OF SOCIETIES.

PHYSICAL SOCIETY.

March 25, 1876.

Professor G. C. FOSTER, F.R.S., President, in the Chair.

THE following candidates were elected members of the Society:—Prof. Liversidge, The Marquis of Salisbury, W. Ackroyd, Tolver Preston, W. Merritt.

Mr. O. G. LODGE, B.Sc., made a communication on "*The Flow of Electricity in a Plate*," in continuation of a paper which he read before the Society, on the 26th of February. In order to apply the principle of images already described to the flow of electricity in plates bounded by straight lines, it is necessary that the angles of the plate should be aliquot parts of 180° ; and, since this condition excludes obtuse angles, the number of rectilinear figures which can be treated is very limited. They are rectangles, equilateral triangles, two cases of right-angled triangles, the two limiting cases of isosceles triangle, for which the equal angles are 0° and 90° respectively and many cases of the general two-sided polygon or "wedge," including the regular two-sided polygon or "strip." Since the images of a pole in a wedge lie on a circle as in a Kaleidoscope, Cotes's property of the circle may be applied to obtain expressions for the potential of any point, and for the electrical resistance of the plate to the flow from any number of point-poles situated anywhere in it. The expressions are rather long, but they become simpler in certain special cases which were pointed out. Making the angle of the wedge vanish, the expressions modify into corresponding expressions for the strip, the resistance expressions of which always contain hyperbolic trigonometrical functions of the positions of the poles. The potential function for a circular sector also follow from the general case of the wedge. The general resistance formula applied to the case of the isosceles right-angled triangle leads to some continued products, all of which are generalisations of

Wallis's expression for $\frac{\pi}{2}$. The product of these products, which is itself of the same form, has been reduced by Mr. J. W. L. Glaisher to the complete elliptic integral usually denoted by K , its modulus being $\sin 45^\circ$. This quantity appears in all the resistance expressions for right-angled

triangles and squares which the author has yet examined. The case of an equilateral triangle leads to more complex and interesting products, which were reduced by Mr. Glaisher to the product of two theta-functions with $\sin 75^\circ$ as a modulus. When the conditions of flow are known in one rectilinear figure, they may be extended to a large number of others by alternate processes of reflecting the plate in one of its own boundaries and of cutting it along one of its straight flow or equipotential lines. Diagrams of such transition figures were shown. In order to obtain the resistance of a compound conductor by means of the known resistance of its components, it is necessary that the flow conditions in each component shall remain entirely unaffected by their being connected together. Thus, if the resistance of a circuit consisting of two wires side by side is to be deduced from the resistance of the wires separately, by the ordinary method of adding their conductivities, it is necessary either that the wires shall not touch each other, or that, if they do, no flow shall pass across the junction. This rule is often overlooked, and the oversight has given rise in certain cases to a notion of electrical "interference." The concluding part of the paper has to do with the flow conditions when line poles are combined with point poles in a sheet, especially when point electrodes are introduced into a sheet in which a uniform current or "river" is flowing across the sheet.

Dr. GUTHRIE referred to experiments which he had made on the subject with the aid of a magnetic needle, but the results were of course interfered with by the lines of flow. He suggested as another subject deserving of investigation, the flow of electricity through a network of wires, which might perhaps be considered as a case of one dimension.

Mr. LODGE had not investigated such a case, but thought it would rather be an extremely complicated case in two dimensions, somewhat resembling a plate with many holes in it.

Prof. FOSTER pointed out the extreme simplicity of the ideas employed to obtain the results which had been given. The subject has often been treated by mathematicians, but too often without regard to the physical point of view.

Dr. GUTHRIE then communicated a fourth paper on "*Salt Solutions and Attached Water*." It consists mainly of an account of an examination of the behaviour of a salt solution when cooled below the freezing-point of water. Having shown in previous communications that every salt solution, when of a certain strength, solidifies as a whole at a certain temperature as a cryohydrate, the present research was directed to the determination of the temperatures at which (1) ice separates from solutions of strengths weaker than the cryohydrate, and (2) the anhydrous salt, or some hydrate richer than the cryohydrate, separates from solutions stronger than the cryohydrate. About twenty typical salts have been examined in this manner, and curves were exhibited, in which the abscissæ represent strengths and the ordinates solidification temperatures. All the curves have a similar character, and exhibit a point of contrary flexure between the origin, representing pure water at 0° C., and the cryohydrate. Between the cryohydrate and the 0° C. degree of saturation, they are nearly straight lines, and are continuous with the curves of solubility above 0° C. The joint effect of two salts in depressing the temperature of ice-formation was also examined. From previous experiments the general law that the temperature of a freezing-mixture is identical with that of the solidification of the cryohydrate of the corresponding salt appeared not to be the case with iodide of sodium. It now appears that this salt offers no exception to the general law and that the what was previously mistaken for the cryohydrate is really a sub-cryohydrate solidifying at a higher temperature. Certain remarkable cases of supersaturation were discussed which show that a solution may be supersaturated in a three-fold manner—(1) With regard to ice;

(2) with regard to a salt; (3) with regard to the cryohydrate of the salt. The parallelism between a boiling saturated salt solution and a glaciating one was pointed out.

Prof. FOSTER explained that when we have discontinuity in a curve we always must be dealing with two separate phenomena. He mentioned also that the portion outside the part of the curve between zero and the cryohydrate must be taken as representing supersaturation of ice, and that outside the upward curve from this point refers to supersaturation of salt.

NOTICES OF BOOKS.

Remarks on Coal and Other Matters. By LEWIS THOMPSON, M.R.C.S. Newcastle-upon-Tyne: "Daily Journal" Office.

A COLLECTION of essays in which a certain amount of truth—chemical and otherwise—is conveyed in a very original, not to say eccentric manner. We quote the author's instructions for finding the comparative value of different samples of coal. The apparatus required is a small cast-iron crucible, about the size of a common tea-cup, and furnished with a lid. This crucible should be as light and thin as possible. The one the author now uses weighs 6 ozs., holds 4 ozs. of water, and cost one shilling. Having obtained a fair sample of the coal—which is rarely practicable except 1 cwt. has been crushed under edge-stones—we reduce an ounce of this to an impalpable powder, and carefully mix 10 grs. of this with $\frac{1}{4}$ oz. troy of common salt, previously dried, and then add and thoroughly mix with this 1 oz. troy of bichromate of potash; place the mixture in the crucible, put on the lid, counterpoise the whole with great care in the scales; then, allowing the counterpoise to remain, place the crucible in a common fire where it may become dull red-hot, and allow it to continue so for a quarter of an hour; after which take the crucible from the fire, and when cold place it again in the scales, and notice how many grains it has lost; then, deducting from this the weight of the coal (10 grs.), the remainder represents the quantity of oxygen carried off by the really useful combustible constituents of the coal. This he has found to vary in the coals supplied to the London market from 19 to 28.

The essay on "Pure Water and Sanitary Reform" will, we fear, be considered in certain influential quarters as little short of blasphemous. The Society of Medical Health Officers, he remarks, tell us that the drinking waters supplied to London have been remarkably free from organic pollution. "Unfortunately there is another Aquarius, a kind of Government official and eminent chemist, who, during part of the very same period, declares that the five companies which take their supply from the Thames sent out water quite unfit for domestic use, and containing large quantities of suspended impurities, fungoid fibres, moving organisms, and soluble brown matter." The author not only asks the old plain question, "Who is to disagree when doctors differ?" but points out that the inhabitants who have swallowed all these pollutions have enjoyed just as good health as those who have had the benefit of the Kent Company's water, "pure, brilliant, and palatable." "As a nation," says Mr. Thompson, "we are getting into the ruinous practice of keeping more cats than we have mice to be caught, the result of which is to induce our sanitary cats to make work for themselves by creating sensational mice." "Up to the present time sanitary reform has not only shown itself to be totally useless and frightfully expensive, and it is in fact a greater nuisance than the nuisances which it pretends to remove." This view, if exaggerated, is not without a certain element of truth. It may serve as an

instance of the reaction which sanitary reformers of the sensational school have succeeded in creating not merely in the minds of the interested and the ignorant, but in those of the scientific and thoughtful. Mr. Thompson's pamphlet contains much that will be called in question, but few can deny that it is highly suggestive, and may be read with much profit.

The Geological Record for 1874. An Account of Works on Geology, Mineralogy, and Palæontology Published During the Year. Edited by WILLIAM WHITAKER, F.G.S. (of the Geological Survey of England). London: Taylor and Francis.

THIS Annual supplies a want which must have been felt by all engaged in geological research, and deserves general encouragement. The books and memoirs referred to are classified under the heads of stratigraphical and descriptive geology, physical geology, including cosmogony, applied and economic geology, petrology, mineralogy, and palæontology. Under the title of each is found a brief abstract of its nature and contents. There are appended a list of the guarantors of the undertaking, among whom we find many names illustrious in science, a table of the works, periodicals, and memoirs referred to, and a copious index. We wish the editor and his colleagues the fullest measure of success.

Artizans' and Labourers' Dwellings Improvement Act, 1875; Report to the Honourable the Commissioners of Sewers of the City of London. By W. S. SAUNDERS, M.D., F.S.A. London: Skipper and East.

THE author gives an account of the areas which he finds to be in a bad sanitary condition, and which he recommends to be dealt with as provided in the Act. As might be expected, some interesting facts are brought to light. Thus, in Holborn Buildings, 48 rooms are inhabited by 160 persons; in Crown Court, Golden Lane, 60 rooms are tenanted by a population of 360, or six persons to a room! In the same district 1231 persons live in an area of 60,000 square feet, so that very little more than 6 feet by 8 falls to the share of each. In Holborn Buildings there is only a space of 5 feet by 5 per person. Altogether some 10 acres contain 5546 persons. But it is evident that the vital conditions are very much worse than might be inferred from the mere ratio of space to population. Dr. Saunders, without going into details, remarks that "whatever ventilation these houses once possessed has been so seriously curtailed that the air surrounding them remains stagnant and vitiated and teems with myriads of animal and vegetable germs, the woodwork and other absorbing surfaces of the houses have become supersaturated with the living and decaying organic matter emanating from the persons, excreta, refuse, and diseases of the people who occupy them." Would it not be very desirable that whenever similar "property" is swept away all the woodwork should be consumed by fire, as suggested, if we mistake not, by Mr. Wanklyn? It is to be hoped that the recommendations of Dr. Saunders will be fully carried out. The pamphlet is illustrated with plans of the condemned districts.

CORRESPONDENCE.

WHITE CAUSTIC SODA.

To the Editor of the Chemical News.

SIR,—I shall be glad if you will allow me to make a few remarks upon the paper by Mr. G. E. Davis, "On the Manufacture of White Caustic Soda." It appears that the name of the original inventor of the process is not

generally known, and I think it right to state what I know of this subject. When, in July, 1860, I was appointed chemist to Messrs. Evans and McBryde, Union Alkali Works, St. Helens, Mr. McBryde informed me that a year before he had heated caustic soda in the pots to redness, and that when this temperature was reached oxide of iron separated out and coloured the whole mass red. It was then run into a second pot to cool, and on breaking the solidified mass, Mr. McBryde found, to his astonishment, that the oxide of iron had completely deposited, leaving on the top a thick layer of perfectly white caustic soda. At Mr. McBryde's request I continued these experiments, and the process was soon so far perfected that for the first time white caustic soda was sent into the market. Mr. McBryde did not foresee the great commercial importance of this manufacture, and with his consent I published the details of this process as interesting from a scientific point of view, showing at the same time that not only the oxide of iron, but also all the alumina separates out, while on the other hand caustic lime may be dissolved in a large quantity in red-hot soda.

At the meeting of the British Association, in Manchester, 1862, I exhibited a large sample of this white caustic soda, and it was then generally acknowledged to be a perfectly new commercial product.—I am, &c.,

PH. PAULI, Ph.D.

Chemische Fabrik, Rheinau,
Near Mannheim, March, 1876.

TREATMENT OF ANTHRACEN OIL.

To the Editor of the Chemical News.

SIR,—I was pleased to see in the CHEMICAL NEWS (vol. xxxiii., p. 99) an article on the above subject, by A. McDonald Graham, F.C.S., and hope that the suggestions offered will be the means of eliciting valuable information from the numerous chemists who must now be working at the subject, it being evident, from the high price which tar commands, that the treatment of the various oils produced in the distillation of tar needs to be carried on in a more economic and practical method than hitherto.

The method of obtaining the anthracen from the oil, by what we may call the residual method, has been known to myself, and I have no doubt to other chemists, for a number of years, but, as far as I can learn, has only been employed in the laboratory, all attempts to work the method practically having resulted in the production of a black greasy mass, often pitchy, and very difficult to filter. In the laboratory I have always found the residual method to give a cake testing 5 to 10 per cent more by the anthraquinon test than the cake obtained in the usual way by direct distillation, and in the majority of cases a larger yield of cake.

If some method could be devised by which a clean residue, easy to filter, could be obtained in the works, there is no doubt that this method would be generally adopted.

There are one or two points in Mr. Graham's article in which I fail to comprehend his meaning. Thus he says—"The quantity of real anthracen contained in the distillate varies according to the nature of the oil operated on; but it is usually very small, amounting on an average to about 12 per cent." Here I presume he means the quality of the anthracen cake produced therefrom; still one would be led to infer from his statement that the distillate yields 12 per cent of real anthracen,—but as this would mean a yield of about 2½ cwt. of real anthracen per ton of the distillate, this cannot be the case.

Again, he says—"The solid portion deposited in the tank will now be found to contain at least 17 per cent of real anthracen, and will be much easier to treat either by fractional distillation or washing." Mr. Graham here writes as if the method produced anthracen cake ready the market, without the use of filter or press; but

even if he did produce such a cake, which I question, what is the utility of treating 17 per cent anthracen by fractional distillation? Why not state the quality and quantity of anthracen cake produced from a given bulk of oil, and the comparative yields by the residual and direct method of distillation, also the specific gravity of the various oils operated on.—I am, &c.,

WILLIAM W. STAVELEY.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Seances de l'Academie des Sciences. No. 10, March 6, 1876.

Absorption of Bicarbonates by Plants in Natural Waters.—M. A. Barthélemy.—The author concludes that there are roots capable of absorbing dissolved gases, and likewise roots destined for the absorption of liquids and of dissolved mineral bodies. The quantity of bicarbonates absorbed is in proportion to the amount of water taken up, varying also with the nature of the plant. The roots of plants excrete carbonic acid, which maintains the bicarbonates in a state of saturation.

White Fumes Given Off by a Blast-Furnace in the Neighbourhood of Longwy.—M. L. Gruner.—The ores treated in this furnace are obtained from the ferruginous öolite, and have an argillaceo-calcareous gangue. The deposit from the dense white fumes consisted of—

Sulphate of potash	0.3782
Carbonate of potash	0.0390
Chloride of potassium	0.0152
Soluble silica	0.0012
Soluble in water	— 0.4336
Silica	0.2298
Lime	0.1588
Alumina	0.0962
Ferrous oxide	0.0400
Manganous oxide	0.0016
Magnesia	0.0036
Sulphur (not determined)	—
Insoluble in water	— 0.5300
Hygrometric and combined water	0.0320
	— 0.9956

The slag produced is basic, and is readily attacked by hydrochloric acid.

Action of Electrolytic Oxygen upon Glycerin.—M. Ad. Renard.—The product obtained is glyceric aldehyd, $(C_3H_6O_3)_4H_2O$.

Note on Inverted Sugar.—M. E. J. Maumené.—A continuation of the controversy with M. Dubrunfaut.

Justus Liebig's Annalen der Chemie,
Band 180, Heft 1 and 2.

Contributions to a Knowledge of the Condensation-Products of Aceton.—L. Claison.—The author concludes that the formation of mesitylen from the condensation-products of aceton is preceded by their re-solution into aceton or its derivatives. Mesitylic oxide and phoron are not intermediate products of the formation of mesitylen. The mesitylic oxide which accompanies mesitylen must be regarded as a secondary, but not as an intermediate product.

Researches from the Laboratory of the University of Tübingen.—Communicated by R. Fittig.—These re-

searches consist of—"Contributions to a Knowledge of the Chinons," by R. Fittig and W. Siepermann; "On the Constitution of Terebic and Pyro-terebic acids," by B. Mielck; and "On the Oxidation Products of Terpin," by Carl Hempel.

Communications from the Chemical Laboratory of Griefswald.—These include papers on the constitution of the three amido-sulpho-benzolic acids, by H. Limpricht; on para-bromo-sulpho-benzolic acids and on para-chloro-sulpho-benzolic acid, both by C. Goslich; and on meta-chloro-sulpho-benzolic acid, by E. Kieselsky.

Communications from the Laboratory of Prof. V. Meyer, of Zurich.—These papers consist of researches on the differences of the primary, secondary, and tertiary nitro compounds, and include the following:—On the bromo derivatives of the nitropropanes, by V. Meyer and J. Tschermak; on the preparation of mono-bromated nitro compounds of the fatty series, and on the bromo-substitution products of nitro-methan, by Dr. J. Tschermak; on the pseudo-nitrols, the isomers of the nitrolic acids, by V. Meyer and J. Locher; on tertiary nitrobutan, by Dr. J. Tschermak; on the action of acids upon nitro compounds of the fatty series, by V. Meyer and J. Locher; on methyl-nitrolic acid, by J. Tschermak; on the action of tin and hydrochloric acid upon ethyl-nitrolic acid and nitroform, by V. Meyer and J. Locher; researches on the constitution of ammonium compounds and of sal-ammoniac, by V. Meyer and M. Lecco; and on the quantivalence and combining power of carbon, by V. Meyer.

Oxidation of Carboniferous Compounds.—E. Erlenmeyer, O. Siegel, and L. Belli.—The authors have examined the oxidation of the butyric acid of fermentation by means of nitric acid, and that of the capronic acid (of fermentation) of succinic and oxalic acid as produced by the same agent.

Tannic Acid of the Hop.—Carl Etti.—The tannin of the hop seems to be nearly related to, if not identical with, the tannins of oak-bark, of ratanhia-root, of the rhizome of *Filix mas*, and of the bark of *China nova*. It is not to be regarded as an acid, but as a compound ether.

Pachnolite of Greenland.—(From the *Transactions of the Royal Academy of Sciences of Gottingen*.)—Pachnolite may be regarded as a hydrated cryolite, in which two-thirds of the sodium are replaced by calcium. Its composition is—

Aluminium	13.43
Calcium	17.84
Sodium	10.75
Water	8.20
Fluorine	49.78
	100.00

It occurs along with cryolite.

Note to the Paper on Diphenylen-Disulphide contained in these "Annalen," 179, p. 178.—C. Græbe.—A brief note referring to the date of the first discovery of diphenylen-disulphide.

Certain Compounds of Ether with Anhydrous Metallic Chlorides.—P. P. Bedson.—An inaugural dissertation read at Owens College, Manchester.

Occurrence of Palladium, Platinum, and Selenium in Silver Coins.—Dr. H. Rössler.—An account of the methods used in the Frankfurt refinery for the separation of the above-mentioned bodies.

Conversion of the Hydrocarbons of the Ethylic Series into the respective Alcohols.—A. Butleroff.—Not adapted for abstraction.

Hyawa Resin.—J. Stenhouse and C. E. Groves.—Derived from an English source.

Compounds of Phenol with Neutral Salts of Quinine.—J. Jobst and O. Hesse.—The authors have obtained and analysed the phenol-sulphate, phenol-hydrochlorate, and

phenol-hydrobromate. They find that in these compounds the carbolic acid has lost its corrosive and poisonous properties, still retaining its useful attributes. They suggest the use of these compounds in typhus.

Gazzetta Chimica Italiana.
Anno vi., 1876, Fasc. i.

Synthesis of Sulphuretted Tannic Acids.—Ugo Schiff.—A lengthy memoir, incapable of useful abstraction.

Elasticity of Metals at Different Temperatures.—G. Pisati.—The first chapter only of this paper is given, treating on the elasticity of iron and steel at different temperatures.

Modification of the Process for the Separation of Poisonous Alkaloids from the Intestines.—F. Selmi.

Method of Detecting Traces of Phosphoric Acid in Toxicological Investigations.—F. Selmi.—These two papers are reserved for insertion in full.

Action of Iodide of Allyl and Zinc upon Oxalic Ether.—E. Paterno and P. Spica.—The reaction yielded an oily body containing 64.74 per cent of carbon and 8.96 of hydrogen.

Chemical Researches on Twelve Solid Colours found at Pompeii.—P. Palmieri.—The author identifies ten of the specimens with the following colours, referred to by Pliny:—Ochra, rubrica, minium secundarium, sinapis, ærugo, viride Appianum.—A rose colour appeared to be a lake consisting of mineral matter—chiefly alumina combined with a nitrogenous organic body. Its reactions were not very different from those of cochineal and madder lakes. The author concludes that it is a compound of the celebrated purple dye of antiquity, mixed possibly with the colouring-matter of kermes and of madder. On some future occasion we may give this interesting paper in full.

Les Mondes, Revue Hebdomadaire des Sciences,
No. 10, March 9, 1876.

This issue contains no chemical matter.

Reimann's Farber Zeitung,
No. 8, 1876.

Solutions of Egg Albumen.—At 17.5° solutions of egg albumen contain, according to G. Witz, the following proportions of solid albumen (with 15 per cent of hygroscopic water):—

Albumen Per cent.	Degree of Baumé.	Specific Gravity.
1	0.37	1.0026
2	0.77	1.0054
3	1.12	1.0078
5	1.85	1.0130
10	3.66	1.0261
15	5.32	1.0384
20	7.06	1.0515
25	8.72	1.0644
30	10.42	1.0780
35	12.12	1.0919
40	13.78	1.1058
45	15.48	1.1204
50	17.16	1.1352
55	18.90	1.1511

No. 9, 1876.

Referring to the sensational *canards*, circulating in political and literary papers, about poisonous dye-ware

and their dreadful effects, Dr. Reimann advises dyers and printers to challenge the propagators of such stories to prove their truth.

Retoration of Albumen.—Albumen is rendered partially insoluble and useless for printing by a variety of causes, such as long standing, heating above 35°, &c. G. Wagner treats it then with parings of the stomach of calves at blood-heat. He takes 400 grms. of the albumen, 30 of calves stomach, washed in cold water and cut up into square pieces, and adds them to 1 litre of water acidulated with 10 grms. of hydrochloric acid. After 36 hours he strains and neutralises with ammonia. The solution of albumen is then again fit for use. G. Witz effects the same process with the stomach of sheep, digesting in the acidulated liquid at 40° for 40 hours. With pepsin, colours fixed with albumen on printed goods may be removed. In like manner the starch may be removed from tissues not well finished by treatment with malt, which converts it into sugar. This process is preferable to boiling. Pepsin acts equally well upon the albumen of eggs and of blood.

Removal of Burls from Wool and Cloth.—Introduce the wool or the woollen goods into 100 litres of sulphuric acid at 6° B. in which 500 grms. alum, 250 grms. salt, and 50 grms. of borax have been dissolved. Work in this bath for one or two hours, drain in the centrifugal, and hang up at 100° to 120°. Wash for 1½ hours in clear water, treat for 2 hours with fuller's earth, soda, and lime, and wash again for 2 hours. Sulphuric acid is adapted only for whites and indigo blues. For coloured goods solutions of chloride of tin and chloride of manganese (?) at 6° B. are recommended. The editor points out that the addition of borax is absurd.

No. 10, 1876.

The Association of German Distillers is about to establish, in Berlin, a college for the especial cultivation of the branches of service bearing upon their business.

G. Ruckensteiner calls attention to the destructive action of animal and vegetable oils upon steam engines and boilers. Steam at high pressure decomposes these bodies and liberates fatty acids, which, as he has proved by an extended series of experiments, attack the iron. He recommends the use of mineral oils boiling at high temperatures.

The dyeing-receipts in this issue contain nothing noteworthy.

No. 11, 1876.

Vanadium for Aniline Black.—A friend in Rouen announces that aniline blacks with vanadium are there in use on a large scale. Per litre of colour 0.001 gramme, or 1 milligramme of the vanadate of ammonia is employed, *i.e.*, the one-millionth part. Sulphide of copper, &c., are no longer thought of. Aniline salts, chlorate of potassa and vanadate of ammonia—that is all. The misfortune is the scarcity of the vanadium preparations. The colourists of Rouen are already studying another aniline black which can be really dyed upon linen, cotton, silk, and wool, and without damaging the fibre. A new method has also been discovered in Rouen of fixing the aniline colours along with madder colours, but without albumen. Thus the important albumen question may perhaps be got rid of altogether.

It must be confessed that the colourists of Rouen produce far more novelties than their colleagues in all the rest of the world. What can be the cause? Simply that most of them have had a thorough scientific training. And how do they obtain there colourists scientifically trained? Because France, in spite of its defective system of elementary education, possesses excellent schools for specialities in which youths are not "crammed" but scientifically prepared for their future callings. This is the point in which we require reformation. (If Germany makes such a confession what must England say? We

have often maintained that it is not elementary, but technico-scientific education which is wanted if we are to preserve our national manufacturing pre-eminence. We want more inventors, not more readers and writers.)

NOTES AND QUERIES.

** Our Notes and Queries column was opened for the purpose of giving and obtaining information likely to be of use to our readers generally. We cannot undertake to let this column be the means of transmitting merely private information, or such trade notices as should legitimately come in the advertising columns.

Mineralogy.—Can you inform me of the best and most complete works on mineralogy and applicability of minerals, ores, &c., to manufactures, and where obtainable?—GEO. G. BLACKWELL.

Infusing Tea.—Since no one has answered the query upon this subject, may I suggest that it is perhaps owing to the escape of dissolved gases that water which has been kept in a state of ebullition does not make such good tea as water just upon the boil. I once made some experiments to try the truth of this supposition, and found that water boiled for ten minutes through which a brisk stream of CO₂ was passed did not differ so much in its effects upon the tea as when the water was boiled for a similar length of time without the addition of the CO₂.—A. P. S.

MEETINGS FOR THE WEEK.

- MONDAY, 3rd.—Medical, 8.
 — Royal Institution, 2. General Monthly Meeting.
 — London Institution, 5.
 — Society of Arts, 8. Cantor Lectures. "Wool Dyeing," by George Jarman.
- TUESDAY, 4th.—Civil Engineers, 8.
 — Royal Institution, 3. "On the Classification of the Vertebrated Animals," by Prof. Garrod.
 — Zoological, 8.30.
- WEDNESDAY, 5th.—Society of Arts, 8. "The Cultivation in India of Caoutchouc-yielding Trees," by Clements R. Markham, C.B.
 — Geological, 8.
 — Microscopical, 8.
 — Pharmaceutical, 8.
- THURSDAY, 6th.—Royal, 8.30.
 — Royal Institution, 3. "Polarised Light," by Mr. Spottiswoode.
 — Royal Society Club, 6.30.
 — London Institution, 7.
 — Chemical, 8. "Note on the Action of Sulphuric Acid on Naphthalen," by Mr. C. E. Groves and Dr. Stenhouse. "Notes from the Laboratory of the Yorkshire College of Science," by Prof. Thorpe. "On Systematic Nomenclature," by Dr. Armstrong. "On the Manufacture of Sulphuric Acid on the Large Scale, Experimentally Illustrated," by Dr. Messel and Mr. Squire. "On the Action of Certain Metallic Chlorides upon Benzene, Toluene, and Naphthalen at High Temperatures," by Mr. Watson Smith.
- FRIDAY, 7th.—Royal Institution, 9. "Ordeals and Oaths," by E. B. Tylor.
 — Geologist's Association, 8.
- SATURDAY, 8th.—Royal Institution, 3. "Wagner," by Mr. Dannreuther.
 — Physical, 3.

THE QUARTERLY JOURNAL OF SCIENCE.

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Correspondence, Notices of Scientific Works, Progress of the Various Sciences, &c., &c.,

London: 3, Horse-Shoe Court, Ludgate Hill, E.C.

THE CHEMICAL NEWS.

VOL. XXXII. No. 854.

ON REPULSION RESULTING FROM RADIATION.*

By WILLIAM CROOKES, F.F.S., &c.

PART IV.

(Concluded from page 131).

The Measurement of the Force.

THE author describes a torsion-balance in which he is enabled to weigh the force of radiation from a candle, and give it in decimals of a grain. The principle of the instrument is that of W. Ritchie's torsion-balance, described in the *Philosophical Transactions* for 1830. The construction is somewhat complicated, and cannot be well described without reference to the diagrams which accompany the original paper. A light beam, having 2 square inches of pith at one end, is balanced on a very fine fibre of glass stretched horizontally in a tube, one end of the fibre being connected with a torsion-handle passing through the tube, and indicating angular movements on a graduated circle. The beam is cemented to the torsion-fibre, and the whole is enclosed in glass and connected with the mercury-pump and exhausted as perfectly as possible. A weight of 0.01 grain is so arranged that it can be placed on the pith or removed from it at pleasure. A ray of light from a lamp reflected from a mirror in the centre of the beam to a millimetre-scale 4 feet off shows the slightest movement. When the reflected ray points to zero, a turn of the torsion-handle in one or the other direction will raise or depress the pith end of the beam, and thus cause the index ray to travel along the scale to the right or to the left. If a small weight is placed on one end so as to depress it, and the torsion-handle is then turned, the tendency of the glass fibre to untwist itself will ultimately balance the downward pressure of the weight, and will again bring the index ray to zero. It was found that when the weight of the 1-100th of a grain was placed on the pith surface, the torsion-handle had to be turned 27 revolutions and 353°, or 10,073° before the beam became horizontal. The downward pressure of the 1-100th of a grain was therefore equivalent to the force of torsion of the glass thread when twisted through 10073°.

The author next ascertained what was the smallest amount of weight which the balance would indicate. He found that 1° of torsion gave a very decided movement of the index ray of light, a torsion of 10073° balancing the 1-100th of a grain, while 10074° overbalanced it. The balance will therefore turn to the 99-100,000,000th of a grain.

Divide a grain weight into a million parts, place one of them on the pan of the balance, and the beam will be instantly depressed.

Weighed in this balance the mechanical force of a candle 12 inches off was found to be 0.000444 grain; of a candle 6 inches off 0.001772 grain. At half the distance the weight of radiation should be four times, or 0.007088 grain; the difference between theory and experiment being only four-millionths of a grain is a sufficient proof that the indications of this instrument, like those of the apparatus previously described by the author, follow rigidly the law of inverse squares. An examination of the differences between the separate observations and the mean shows that the author's estimate of the sensitiveness of his balance is not excessive, and that in practice it will safely indicate the millionth of a grain.

One observation of the weight of sunlight is given: it was taken on December 13; but the sun was so obscured by thin clouds and haze that it was only equal to 10.2 candles 6 inches off. Calculating from this datum, it is seen that the pressure of sunshine is 2.3 tons per square mile.

The author promises further observations with this instrument, not only in photometry and in the repulsion caused by radiation, but in other branches of science in which the possession of a balance of such incredible delicacy is likely to furnish valuable results.

ON THE

ANALOGY OF CYANOGEN TO OXYGEN.*

By WILLIAM SKEY,

Analyst to the Geological Survey of New Zealand.

I HAVE to preface my remarks upon this subject by the statement that they are entirely of a theoretical nature, and therefore unsupported by the results of that kind of experimental research, the details of which it will be remembered have hitherto constantly formed the groundwork of those previous papers of mine read before you; however, for this once I must beg your kind indulgence for a hearing upon that which, if it has any value, owes it to chemical researches and chemical records, long since accumulated by other chemists.

Presuming, then, upon your indulgence, I will at once state that the subject of this paper is the true position of a certain compound body among the elements as deducible from its known chemical reactions, that now assigned to it being, I think, incorrect.

The great importance of interpreting those facts correctly by which we compare our imitative with our real elements is so obvious to those anxious to apprehend more of the true nature of the elements than at present we do, that I need not excuse myself for bringing such a matter as this before you.

The substance, the supposed position of which I take exception to, is cyanogen, a compound, as you are aware, of carbon and nitrogen in equivalent quantities. It and a number of other compounds into which it enters are now classed indiscriminately and collectively with the chlorine group as salt radicals, but to cyanogen itself "par excellence" is attributed this character.

That this is in reality the position assigned to cyanogen is indisputable. Brand and Taylor, in their excellent work on chemistry, designate this substance as a compound radical, and associate it with chlorine, bromine, and iodine for reasons I shall presently show. Prof Roscoe, too, in his "Elementary Chemistry," 1871, describes cyanogen in terms which certainly have a tendency to keep it so classified. The special grounds upon which cyanogen is classed with these radicals are, I believe, as follows:—

- (1.) That it and its hydride combine directly with the least oxidisable metals generally, as gold and silver.
- (2.) That it also combines with hydrogen, and forms with it a compound analogous to hydrochloric acid.
- (3.) That when passed into a solution of any caustic alkali it is in part oxidised, alkaline cyanates and cyanides forming.
- (4.) That when cyanides are electrolysed the radical is evolved at the positive pole.

If to this we add that cyanide of potassium crystallises in cubes, as do the chlorides, bromides, and iodides of this base, I think we exhaust the evidence which can as yet be alleged in favour of the analogy of cyanogen to the elements referred to.

* Abstract of a paper read before the Royal Society, Feb. 10, 1876.

* Read before the Wellington Philosophical Society.

Such are the reasons for classifying cyanogen with these radicals, and I will now go over them *seriatim*.

In the first place it is true that cyanogen combines directly with the least oxidisable metals, but so does oxygen when in the allotropic state, also sulphur at a slight elevation of temperature, and, further, its hydride (sulphuretted hydrogen) imitates hydrocyanic acid in presence of the metals instanced. Oxygen and sulphur should therefore on this principle be admitted along with cyanogen into the group of radicals, which would be absurd, as they are not admitted as radicals at all; therefore these tests are unreliable.

In the second place, besides cyanogen, sulphur and a number of other elements combine readily with hydrogen, the bulk of which are not halogens, while, in reference to the supposed analogy existing between hydrocyanic and hydrochloric acids, I really fail to see any grounds for this.

Hydrochloric acid is a very strong one, intensely acid, and forms salts with the alkaline metals which are quite neutral. Hydrocyanic acid, on the other hand, if acid at all (which I doubt), is so feebly so that "it scarcely affects the blue litmus paper;" indeed I believe it to be neutral, as any minute acid reaction which has been obtained in respect to it may be due to carbonic acid, hydrocyanic acid being very prone to decompose with evolution of carbonic acid.

Further, in accord with this, the salts of cyanogen with the alkaline metals are not neutral, as are the corresponding salts of the chlorine group, but strongly alkaline.

In reference now to the third supposed joint characteristic of cyanogen and the radicals to which it is compared, we can parallel this in the case of sulphur and phosphorus. Thus, either of these elements, when warmed with a solution of any caustic alkali, forms oxygenated and haloidal salts, a part of them being oxidised at the expense of the oxygen of the alkali, as in the case of cyanogen, chlorine, &c., under these circumstances.

Lastly, as to the polar affinities of cyanogen and the crystalline form of its potassium salt. Sulphur and oxygen, when liberated by voltaic action, also detach from the negative pole; while, in reference to the similarity in the crystalline forms of cyanide of potassium to the chloride, so many substances crystallise in the same form, though these are of a widely different nature, that, as a single test of position, form can be of little value.

Thus I think the grounds upon which we class cyanogen in this manner do not, when carefully examined, prove at all sound, but it rather appears, if admission to the group of halogens is given to cyanogen, that we must upon principle further admit within it substances, such as sulphur and oxygen, which obviously should not enter there.

But, outside anything yet stated, it is indisputable that the heavy metallic cyanides do not correspond in general with the chlorides, bromides, &c., of this series of metals; except in the case of the silver salts, there is no appearance even of harmony in this direction.

Again, the most stable oxygen compound of cyanogen is, according to the new chemical notation, CyO , while that of chlorine is Cl_2O_5 , of bromine Br_2O_5 , &c. Further, CyO (cyanic acid) forms compounds with the metals which are generally insoluble in water, alcohol, or ether; while chloric acid, its alleged analogue, generally forms compounds with them, possessing considerable solubility in these liquids. There is, in fact, as great a difference between the two acids and their metallic compounds on these points as there is between carbonic and nitric acid and between their respective metallic compounds. Further, the composition of platino-chloride of potassium is $K+Pt+Cl_4$, while that of the platino-cyanide is $K_4+Pt_2+Cy_{10}$, exhibiting again a marked difference.

The great dissimilarity existing between cyanogen and the elements of the chlorine group, analogically considered, being thus shown, and the ground I hope consequently ready for the reception of a better classification than the one attacked, I now proceed to show what I conceive to

be the proper position of cyanogen in regard to the elements. For this purpose I will refer you to the supposed points of resemblance between this substance and chlorine, which I have just criticised, and I think you will find that, wherever the true character of cyanogen is correctly stated, it agrees precisely with that of oxygen.

Thus, to recapitulate a little, oxygen, especially when in the allotropic form, combines directly with metals generally, including gold and silver; moreover, it combines with hydrogen to form a neutral compound, and this, when electrolysed, delivers its oxygen at the positive pole. Besides this, cyanogen resembles oxygen, wherein, as shown, it differs from the chlorine group, its compound with the alkaline metals being caustic, and those with the heavy metals characterised by great insolubility in water, while several of these cyanides are soluble in alkaline cyanides, precisely as several of the metallic oxides are soluble in alkaline oxides; further, cyanogen, like oxygen, is capable of assuming an allotropic condition.

Following up analogies here, I would class cyanogen and sulphur together, and so I would their hydrides. HS , like cyanogen, is not strongly acid, indeed probably not acid at all, for, as in the case of hydrocyanic acid, HS exhibits a great tendency to oxidise when in contact with water and to form oxyacids, so that in testing this gas for acidity we are liable to obtain reactions not due to the gas itself.

Our new nomenclature, by doubling the equivalents of oxygen and sulphur, has disturbed the uniformity which before this existed between their common hydrides and that of cyanogen; thus one point of resemblance has been removed, but I think this has been done somewhat arbitrarily in regard to cyanogen. Certainly, when the equivalent of cyanogen is retained, its hydride then being CyH (hydrocyanic acid), comparing with that of chlorine, the supposed similarity of these substances is maintained; and this, by the way, may have been one of the reasons for which the doubling process described was broken off at cyanogen. However, if I am correct in assuming that this compound is analogous with oxygen rather than with chlorine, its equivalent will also require doubling. If you now agree with me, or at least will contemplate the possibility, that cyanogen is not analogous to chlorine and its isomorphs, but rather to oxygen, you will be in a position to perceive certain interesting relations which it bears to oxygen, and which could not well have presented themselves had the assumption I have here attempted to disprove remained unassailed.

Thus ferro- and ferri-cyanogen become, upon this view, ferri-oxides in which oxygen is replaced by its isomer, cyanogen, and the same being true for the rest of the metallic cyanides, these substances should be, I think, viewed as comparing with the oxides of sulphur and chromium as they exist in the sulphates or chromates; further, sulpho-cyanogen and seleno-cyanogen, the only compounds containing cyanogen (or at least its elements), which do compare with the simple halogens, are not at all analogous with cyanogen. The cyanides thus viewed are not salts at all any more than the oxides are: sulpho-cyanides, on the other hand, are true salts, comparing exactly with the corresponding salts of the halogens.

Further, in regard to the question often raised as to the nature of certain of our elements, whether compound or not, it seems interesting that in this compound (cyanogen) we have a substance very similar to the element oxygen, one which at least only varies from it within the limits we are compelled to allow for *variation* in the members of certain well defined natural groups of our elements. We are thus, as far as is allowable from such apparent resemblances, justified in entertaining the supposition that oxygen itself is also a compound body. I need not remind you in this connection that any theory which touches upon the nature of this gas has now an especial interest to us, for, as you will be aware, this and our most common gases or gaseous vapours are, for good reasons, considered to be

distributed throughout the earths and suns generally,* and even to pervade the spaces between them, and to perform all the functions we have hitherto allotted to a purely hypothetical substance. The nature, therefore, of any gas which is possibly a constituent of that which we now consider to be a universal atmosphere becomes invested with an importance to us far beyond what we could even conceive of a short time since.

Lastly, in regard to the question as to the nature of our elements, it appears a very noteworthy circumstance that, by combining cyanogen with sulphur, which is also an analogue of oxygen, we obtain a compound analogous to the halogens I have referred to. That this ternary compound, sulpho-cyanogen, should be thus a true salt radical is strongly favourable to the idea that one or more of the chlorine group of elements is of a compound nature, and in relation to this it is worthy of record that, as I have already pointed out, the "equivalent number of sulpho-cyanogen is one which is very nearly the mean between that of chlorine and bromine."

However, whether these facts indicate anything of this kind or not, I think the object of this paper has been fulfilled, for I believe I have shown that, to use a familiar but significant phrase, cyanogen has not the "stuff" in it for making a salt radical singlehanded, therefore it is not in any way analogous to one, but in order to make it so we must combine it with another element, so that three elements in place of two are as yet the smallest number required to form a compound salt radical.

In concluding my paper I cannot avoid expressing a wish that the question which I have raised here had been taken up by some one more accustomed, by training and association, than myself to grapple it by the aid of what is well termed the "New Chemistry." I have worked at this question by the old lights, but if by this I am successful in inducing anyone to take it up who will work at it by the new ones I shall be satisfied with the result.

PHOTOGRAPHY AS APPLIED TO ECLIPSE OBSERVATIONS.†

By ALFRED BROTHERS, F.R.A.S.

SINCE 1860, when photography was first applied to eclipse observations, almost every eclipse of the sun has been photographically recorded—from 1860 to 1868 for the purpose chiefly of determining the nature of the red prominences; and in 1870 and 1871 to ascertain whether the corona is an appendage of the sun or an effect produced in our own atmosphere. Previous to 1870 the ordinary telescope, uncorrected for the chemical rays, had been almost exclusively used. But in 1870 it was determined to adopt a properly corrected photographic lens, and by a graduated series of exposures to obtain, if possible, the whole pictorial effect. This method was successful, and has been adopted in all eclipse work since. That more suitable apparatus has not been employed may be due to the fact that the funds provided by the Government, the Royal and Royal Astronomical Societies, for the observation of the various eclipses have either all been spent at the time, or the balances have been returned. As good work has been done with the apparatus referred to, it may be asked why anything different should be used. It was by mere accident that a lens of a certain kind was used in 1870, no other suitable was to be had, and the image obtained with it is small. Photography was not employed during the eclipse of 1874, almost the only observer on that occasion being the Astronomer Royal at the Cape of Good Hope, Mr. Stone, who observed with the spectroscope under the most favourable conditions, and it is much to be regretted that no photographs were obtained. On the occasion of

the recent eclipse no preparations were made until the invitation from the King of Siam was received, and then, as on almost every occasion since 1868, all arrangements have been hurriedly made. No apparatus for obtaining a picture of the corona different from what has been previously used was employed, and consequently no superior result may be anticipated. The lenses used in 1870 and since for photographing the corona give an image of the sun of about three-tenths of an inch in diameter, and although suitable for small pictures, such lenses cannot be said to be the best for the purpose. I would suggest, therefore, that at least three achromatic lenses of 5 or 6 feet focal length, corrected for the actinic rays, should be constructed, with all suitable apparatus, so as to be ready for use when required. The light of the corona is sufficiently actinic to produce good pictures when an instrument of long focus is used—it is only a question of time in the exposure and accuracy in the adjustment of the driving clock apparatus attached to the equatorial mounting. There cannot, I think, be any doubt that under favourable atmospheric conditions some features of interest would be revealed during every eclipse, and it is undesirable to allow any eclipse of the sun to occur without some attempt being made to record such phenomena permanently by means of photography. It seems to me also equally certain that pictures of greater dimensions, such as the instrument suggested would give, would be proportionally more valuable than any hitherto obtained.

The photographic process used has always been the wet collodion. It might be advantageous to use daguerreotype plates, but I see no reason why both methods should not be employed.

REPORT ON THE DEVELOPMENT OF THE CHEMICAL ARTS DURING THE LAST TEN YEARS.*

By Dr. A. W. HOFMANN.

(Continued from p. 132.)

THE author in his treatise gives a calculation which verifies these statements. In the above-mentioned experiment with the hand machine 1 kilo. of charcoal yielded $3\frac{1}{2}$ kilos. of ice. Vaass and Littmann give estimates of the first cost of the various machines, and of the price of the ice produced, which varies according to the size of the apparatus, producing respectively from $\frac{1}{2}$ to 10 cwts. hourly, from 1 mark 15 pfennige to 30 pfennige per cwt., including interest on capital, depreciation, and waste. The Nordhausen Company give a calculation for a 250 kilos. machine based on 300 days uninterrupted work (day and night), according to which the ice costs 36 pfennige per cwt. Up to the end of the year 1873 the latter establishment had finished 60 machines, 29 of them for Germany. Vaass and Littmann had completed 42 machines, 20 of which were for Germany, including 2 for Vienna.

At the London Exhibition of 1862, and that of Paris, 1867, Carré's machines were exhibited by Mignon and Rouart, of Paris; at the Vienna Exhibition the two German firms made their appearance.

Carré's machine is without doubt a very perfect, manageable, and effective apparatus for producing ice everywhere and to any extent. In many cases it may successfully compete with the natural article, especially in large towns where the demand is great, and where luxury plays an important part. Artificial ice is often more palatable, since natural ice is too frequently dirty, and even when perfectly clean possesses a swampy flavour. Among the ice manufactories whose existence we have

* "Fuel of the Sun," by W. Mattieu Williams, F.C.S.

† Read before the Physical and Mathematical Section of the Manchester Literary and Philosophical Society.

* "Berichte über die Entwicklung der Chemischen Industrie Während des Letzten Jahrzehends."

ascertained may be mentioned that of A. Pokorny, in Vienna, which was supplied by Kropff, in 1869, with a machine yielding 5 cwts. hourly. The proprietor has courteously informed the author that he is perfectly satisfied with the working of the machine. It yields 10 cwts. ice per 1 cwt. of charcoal consumed. The cost of the ice amounts to 35 Austrian kreutzers per cwt., the sale price fluctuating from 70 kreutzers to 3 florins 20 kreutzers. The loss of ammonia amounts to 2 kilos. per 50 cwts. of ice produced. The machine has remained in good order for four years, but the connections are not perfect. Ice machines have been set up in various German breweries, to which the makers refer in their circulars. Mignon and Rouart are said to have made in 1869 a machine of the value of 20,000 florins (?) for the Joint Stock Brewery, at Deux-ponts.*

Carré's machine has been from its very origin a very carefully constructed apparatus in which essential improvements are scarcely conceivable. The difficulty as regards the material was soon overcome by making all the parts of wrought-iron coated with zinc, copper, and its alloys being entirely avoided, as they are rapidly attacked by ammonia. Reece, however, in 1870, patented in England an improvement with the object of preventing the simultaneous evaporation of the water in the boiler. He asserts that the liquid which arrives in the ice generator consists of 25 per cent of water and 75 of ammonia. His arrangement, which corresponds in the main with dephlegmation and rectification as customary in distilling, is said to condense the ammonia practically free from water.† He also utilises the tension of the evaporating ammonia in a machine which works the pumps.

In September, 1867, Toselli, of Paris, obtained an English patent (in the name of Clark) for an ammonia ice machine arranged on the principle of Carré's portable apparatus. It consists of two cylinders, united axially by means of a tube, and turned continually by a handle. The ammonia contained in one of the cylinders gave off, when heated, its ammoniacal gas into the other cylinder; the residual water afterwards re-absorbed the gas, the apparatus remaining all along hermetically closed. It was planned both for domestic use on the small scale and for manufacturing purposes. As regards its performance nothing has transpired.

Ammonia Air Pump Machine.—In 1869, Mort and Nicolle, of Sydney, patented an ammonia machine‡ differently arranged from that of Carré's and capable of being regarded as a combination of the latter with the ether machine. The inventors use an air pump, but assist its action by absorption. As cooling agent they apply not volatilised ammonia, but concentrated aqueous ammonia.

(To be continued.)

ON ISOMERIC ROSANILINES.

By M. A. ROSENSTIEHL.

In 1868, after the discovery of pseudo-toluydins, I showed that commercial magenta does not result from the concurrence of two alkaloids only—aniline and toluydin—as was then admitted, but that to these bodies must be added pseudo-toluydin, as one of the most important factors in the production of this remarkable red colouring matter. I indicated at the same time the existence of two isomeric bodies obtained, the one from aniline and toluydin, the other from aniline and pseudo-toluydin, whose physical properties are so nearly allied that they cannot be distinguished. This delicate isomerism—which depends

principally on the nature of the generating bodies—has appeared to me important enough to be confirmed by a great number of experiments. In this work of revision I had in view especially (after having prepared isomeric rosanilines with materials as pure as the actual state of the question permits it to be done) to regenerate the alkaloids in sufficient quantity to be able to determine the relative proportions. This manner of procedure has appeared to me so much the more necessary as experience has shown me in these last years the impossibility of preparing aniline free from pseudo-toluydin. I conclude that it is impossible to separate the three alkaloids so as to obtain each in a state of absolute purity, and I admit that each of them, although prepared with great care, contains the two others in small quantities. I ought, then, to find pseudo-toluydin in alkaloids regenerated from rosaniline prepared with aniline and toluydin: inversely, I ought to find this last in rosaniline prepared with aniline and pseudo-toluydin. It is fit, moreover, to determine the importance of this cause of errors and its influence on the final result.

I prepared rosanilines corresponding to the following mixtures:—

<i>Rosaniline α.</i>	<i>Rosaniline β 2.</i>
Crystallised toluydin.	Pseudo-toluydin.
Aniline.	Aniline.
<i>Rosaniline β 1.</i>	<i>Rosaniline α, β 1.</i>
Pseudo-toluydin alone.	Crystallised toluydin.
	Pseudo-toluydin.
<i>Rosaniline α, β 2.</i>	
Crystallised toluydin.	
Pseudo-toluydin.	
Aniline.	

After having obtained in a state of purity the rosanilines corresponding to each of these mixtures, and having compared their physical properties, I treated them with hydriodic acid under pressure, to regenerate the alkaloids. It would be too tedious to describe here the method of separation applied to the mixture of regenerated alkaloids. To show, however, the confidence that it merits I will cite an example. I made with pure alkaloids a typical mixture, I then separated it into its elements:—

	Composition of the Mixture.	Found on Analysis.
	Grain.	Grain.
Aniline	0.376	0.375
Pseudo-toluydin.. ..	0.532	0.534
Toluydin	0.040	0.039

I treated with hydriodic acid, besides the isomeric rosanilines, some secondary products of their preparation. These are, first, the products that remain in the mother-liquors of the crystallisation of rosaniline β; second, the insoluble products that are formed at the same time. The results are summarised in the table. (See next page).

From these analytical results we may draw the following conclusions:—

Each rosaniline regenerates in reality three alkaloids, but in a proportion such that there can be no doubt on the isomerism.

The ratio of 1 molecule of aniline to 2 molecules of toluydin demanded by the formula of rosaniline, as established by M. Hofmann, is found pretty nearly in the regenerated alkaloids; however, the aniline is in rather smaller proportions, because it is partly transformed into ammonia by the action of hydriodic acid.

Pseudo-toluydin is in itself alone capable of producing a rosaniline, because by the destruction of CH₂ it is partly transformed into aniline.

There exists three isomeric rosanilines—the first derived from 1 molecule of aniline and 2 molecules of toluydin; the second derived from 1 molecule of aniline and 2 molecules of pseudo-toluydin; the third is formed from 1 molecule of aniline, 1 of toluydin, and 1 of pseudo-toluydin. This latter constitutes the bulk of the magentas of commerce.—*Comptes Rendus.*

* *Dingl. Pol. Journ.*, xciii., 432.

† Reece, *Dingl. Pol. Journ.*, 1870, 40. See also *CHEMICAL NEWS*, vol. xxxiii., p. 130.

‡ Mort and Nicolle, *Mech. Mag.*, 1870, 189. *Dingl. Pol. Journ.*, cxvii., 311.

	α	β 1.	Rosanilines.		β 2.	β 2.		α β .
			β 1.	β 1.		Mother Liquor.	Residue.	
Aniline	25	24	30	32	32	34	50	28
Toluydin (crystalline)	75	3	traces	0	4	2	2	38
Pseudo-toluydin	traces	73	70	68	64	64	48	34

ON CERTAIN CIRCUMSTANCES WHICH AFFECT THE PURITY OF WATER SUPPLIED FOR DOMESTIC PURPOSES.

By M. M. PATTISON MUIR, F.R.S.E.,
Assistant Lecturer on Chemistry, Owens College.

(Continued from p. 126.)

II. Action on Copper.—The experiments under this heading were carried out in a manner similar to that already described. The pieces of copper foil presented a surface of 420 sq. m.m. to the action of the various solutions. The results obtained were with one exception negative; no copper was dissolved. The action of the following liquids was examined:—*Distilled water*; the same containing *ammonium nitrate* in quantities varying from 0.02 grm. per litre (=1.4 grs. per gallon) to 0.408 grm. per litre (=28.56 grs. per gallon); the same containing *potassium nitrate* in like amounts; the same containing *ammonium sulphate* in quantities varying from 0.10 to 0.20 grm. per litre (=7 to 14 grs. per gallon); and also distilled water containing simultaneously *carbonates and nitrates*, *carbonates and sulphates*, and *chlorides and nitrates*. The length of time during which the copper was exposed to the action of these solutions varied from 18 to 150 hours. The only liquid which exercised any solvent action upon the copper was that containing the large quantity of 28.56 grs. per gallon of *ammonium nitrate*; this action was manifested only after 150 hours' contact of the liquid with the copper, the amount of metal which had then passed into solution being equal to 3 milligrms. per litre, or 0.21 gr. per gallon.

The general conclusion to be drawn from these experiments therefore undoubtedly is, that at ordinary temperature neither distilled water nor water containing

the salts which commonly occur in drinking waters exercises a solvent action upon copper.

That water charged with carbon dioxide will dissolve copper is apparent from the following figures, which represent the amounts of that metal found by Dr. Milne in various samples of aerated beverages.

TABLE E.
Copper Found in Various Aerated Beverages.

Description of Liquid.	Quantity of Copper in grs. per gallon.
Soda water	0.084
Potash water	0.098
Lemonade	0.053
Ginger ale	0.053
Potash water	0.100
Aerated water	0.089
Soda water	0.100
Aerated water	0.084
Soda water	0.036

In order to arrive at some accurate measurements of this solvent action of water containing carbon dioxide upon copper, I prepared a number of solutions charged with that gas at the ordinary atmospheric pressure, and placed in each a piece of clean copper foil exposing a surface of 2100 sq. m.m. The amount of copper dissolved was estimated by adding sulphuretted hydrogen and comparing the depth of colour produced with that in a standard liquid; the process is exceedingly accurate and delicate. (Table F.)

The general conclusions which I would draw from these results are:—

(1) Distilled water, charged with carbon dioxide, exercises a notable solvent action upon copper, the amount of metal dissolved increasing with the length of time during which it is exposed to the action of the water.

(2) The salts which have the greatest effect in increasing

TABLE F.
Copper Dissolved by Water Charged with Carbon Dioxide at Ordinary Pressure.

Salt.			Mgrms. per litre.	Grains per gall.	24 hrs.	Copper Dissolved.						
						In Mgrms. per Litre.				In Grains per Gallon.		
						48 hrs.	72 hrs.	120 hrs.	24 hrs.	48 hrs.	72 hrs.	120 hrs
Potassium carbonate	200	14.0	0.1	—	0.15	0.20	0.007	—	0.0105	0.014
Calcium chloride	200	14.0	0.7	—	1.20	1.80	0.049	—	0.0840	0.126
Ammonium nitrate	20	1.4	0.3	—	0.60	1.40	0.021	—	0.0420	0.098
"	40	2.8	0.6	—	0.80	1.40	0.042	—	0.0560	0.098
Potassium carbonate	}	..	100	7.0	0.2	—	0.30	1.00	0.014	—	0.0210	0.070
and			and	and								
Ammonium nitrate	}	..	20	1.4	trace	—	trace	0.10	trace	—	trace	0.007
Potassium carbonate			and	and								
Ammonium nitrate	}	..	40	2.8	0.6	—	2.40	3.60	0.042	—	0.1680	0.252
Ammonium nitrate			and	and								
Calcium chloride	}	..	200	14.0	0.1	0.3	—	1.00	0.007	0.021	—	0.070
Distilled water										

TABLE G.
Copper Dissolved by Water Charged with Carbon Dioxide at a Pressure of about 6 Atmospheres.

Salt.	Mgrms. per litre.	Grains per gall.	Copper Dissolved.			
			In Mgrms. per Litre.		In Grains per Gallon.	
Potassium carbonate	40	2.80	24 hours.	48 hours.	24 hours.	48 hours.
Ammonium nitrate	16	1.12	—	0.8	—	0.056
"	80	5.60	1.2	1.4	0.084	0.098
Distilled water	—	—	0.4	0.6	0.028	0.042

this action are *chlorides* and *nitrates*, especially the latter: if they are *both* present the action is very largely accelerated.

(3) *Carbonates*, especially when present in large quantities, very materially diminish this solvent action.

(4) If *carbonates* and *nitrates* are *present together* the solvent action of the latter is much diminished by the presence of the former salts, so much so indeed that if the carbonates be present in proportionately large quantities the solvent action upon the copper almost entirely disappears.

I have also carried out a few experiments with the view of determining the amounts of copper which are dissolved by water charged with carbon dioxide under a pressure of several atmospheres; the results are subjoined. (Table G.)

The apparatus were the same as that employed in the experiments with lead.

Surface of copper exposed = 2100 sq. m.m.

Distilled water, charged with carbon dioxide, under a pressure of (approximately) 6 atmospheres, dissolves about three times as much copper as the same water charged at the ordinary atmospheric pressure. Nitrates increase this action and carbonates diminish it.

(To be continued.)

DR. LETHEBY.

At the comparatively early age of sixty died, on the 30th ult., Dr. Henry Letheby, who for many years had been eminent in his profession, who had justly gained an extensive popularity, and whose advice was eagerly sought after, and greatly valued by those who required the assistance of a chemical expert.

As a technological chemist Dr. Letheby was second to none; and in whatever capacity he was acting—whether as Lecturer on Chemistry, Toxicology, and Technology; as Gas and Water Examiner; as Medical Officer of Health; or as Analytical and Consulting Chemist—he always gave evidence of having industriously mastered the minutest details of his subject. His complete knowledge of Chemistry and Toxicology, and his intimate acquaintance with the Sciences of Comparative Anatomy and Physiology, rendered his opinion on subjects connected with medical jurisprudence of especial value.

His writings and labours are so varied and numerous that we cannot refer to them all. To show, however, that we have not unduly magnified his high qualities, and also that in his death Chemical Science has sustained a great loss, we may refer to his admirable Lectures on "Food" delivered before the Society of Arts, and afterwards, at our request, revised and published in book form; to his Lectures on "Practical Toxicology;" to his papers on the "Mode of Conducting Post-mortem Examinations in Cases of Suspected Murder;" to his reports "On the Sanitary Condition of the City of London;" on the "Practice of Disinfection and the Right Use of Disinfectants;" on the "Utilisation of the Waste Products in the Manufacture of Coal Gas;" on "Noxious and Offensive Trades and Manufactures;" on the "Detection of and Tests for Aniline;" &c.

THE RESEARCH FUND OF THE CHEMICAL SOCIETY.

THE importance of original chemical research is universally acknowledged. No other branch of Science offers such a wide field for investigation, yet we fear we are not going far from the truth when we state that no other branch of Science has at the present time fewer investigators. In looking through the Proceedings of the

Royal or of the Chemical Societies during the last few years, one is struck with the great decrease in the number of papers contributed by our leading chemists, while in other sciences many of the most eminent professors in our Colleges and Universities are continually making valuable additions to our stores of knowledge.

We are, then, especially glad to find that a small additional effort will place the Chemical Society in a position to aid experimental research by grants of money. The limited resources of the Society have hitherto restricted the number and amount of these grants within very narrow limits. In 1872 Mr. T. Hyde Hills placed at the disposal of the Society the sum of £10, as the nucleus of a fund for promoting original research; and offered under certain conditions, made with the object of securing the co-operation of others, to contribute a like sum annually. This attempt of Mr. Hills to form a research fund was not, at the time, seconded. The Council have, however, recently received from Dr. G. D. Longstaff, one of the original members, the generous offer to place at the disposal of the Society the sum of £1000, towards establishing a permanent fund for promoting the advancement of Chemical Science, on the condition that not less than an equal amount be subscribed for the same purpose. Dr. Russell tells us that £640 are already promised, in sums varying from £100 to £1. Those who know the value of original research will, we trust, contribute to the fund, so that the required balance may be speedily raised. By securing to the Society the benefit of Dr. Longstaff's generous offer, contributors are forwarding the interests of Chemical Science, and they are, at the same time, we consider, advancing great national interests.

Subscriptions may be forwarded to Dr. Russell, F.R.S., Treasurer of the Chemical Society, Burlington House.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Anniversary Meeting, Thursday, March 30th, 1876.

Professor ABEL, F.R.S., President, in the Chair.

THE PRESIDENT said he had to congratulate the Fellows on the flourishing state of their Society, the number of admissions during the past year having been 103, whilst the losses by death and other causes had been 23, so that the Society now numbered 881 members. He then read obituary notices of the eight Fellows deceased, namely, Thomas Jennings, Henry Letheby, Edward Meldrum, J. Middleton, S. W. Moore, General J. W. Reynolds, Dr. Robert Schenck, and William Smith. The distinguished foreign members we had lost during the past year were Dr. Schrötter, and Professors Hlasiwetz and Kopp. Before reading the list of papers sent in during the past year—the largest number since the foundation of the Society—he said he must congratulate them on the great improvements in the illumination of their rooms, the result of careful experiments made by their Secretary, Mr. W. H. Perkin, who had most generously defrayed the considerable outlay incurred in making the requisite alterations. After alluding to the Faraday lecture, and the one by Dr. Frankland "On some Points in the Analysis of Potable Water," he said the thanks of the Society were due to Prof. Frankland for the present of a Sprengel pump, to Dr. Longstaff for a balance, and also to Mr. James Duncan for a most valuable and life-like bust of Dr. Hofmann. He then passed to a consideration of the state of the Society's Journal, which, notwithstanding its present size, would require to be considerably increased in the matter of abstracts, in order to keep pace with the large number of papers now published in foreign journals,—and this of necessity requires an increased expenditure.

As the income at present at the disposal of the Society is insufficient to meet this, it was proposed to discontinue the gratuitous distribution of the Royal Society's Proceedings to the Fellows, and also to increase the admission fee for Fellows from two to four pounds. In conclusion, the President mentioned that they had had a legacy left them by the late Mr. Henry Dircks, and that Dr. Longstaff had munificently placed at the disposal of the Society the sum of £1000, provided an equal amount should be raised by subscription, towards establishing a permanent fund for promoting the advancement of the science. He was happy to say that already more than half the amount had been subscribed. After making some reference to Mr. Jodrell's gift to the Royal Society, for promoting original research, and the possibility of adequate State aid being provided for the same purpose, the President concluded amidst great applause.

The TREASURER then read his Report, after which the Fellows proceeded to elect the Officers and Council for the ensuing year, Messrs. C. E. Groves and W. E. Heathfield acting as Scrutators. The following gentlemen were elected:—

President—F. A. Abel, F.R.S.

Vice-Presidents who have filled the Office of President—

Sir B. C. Brodie, F.R.S.; Warren De la Rue, D.C.L., F.R.S.; E. Frankland, D.C.L., F.R.S.; A. W. Hofmann, D.C.L., F.R.S.; W. Odling, M.B., F.R.S.; Lyon Playfair, Ph.D., C.B., F.R.S.; A. W. Williamson, Ph.D., F.R.S.

Vice-Presidents—T. Andrews, M.D., F.R.S.; W. Crookes, F.R.S.; J. H. Gilbert, Ph.D., F.R.S.; J. H. Gladstone, Ph.D., F.R.S.; G. D. Longstaff, M.D.; J. Stenhouse, Ph.D., F.R.S.

Secretaries—W. H. Perkin, F.R.S.; H. E. Armstrong, Ph.D.

Foreign Secretary—H. Müller, Ph.D., F.R.S.

Treasurer—W. J. Russell, Ph.D., F.R.S.

Other Members of the Council—J. Attfield, Ph.D.; Dugald Campbell; A. H. Church; J. Dewar, F.R.S.E.; F. Field, F.R.S.; C. W. Heaton; David Howard; Nevil Story Maskelyne, F.R.S.; J. A. Phillips, R. V. Tuson; W. Valentin; C. R. A. Wright, D.Sc.

A vote of thanks to the President, proposed by Prof. Williamson and seconded by Prof. Odling, was carried by acclamation, as was also one to the Officers and Council, proposed by Mr. T. Hyde Hills and seconded by Mr. J. Newlands. There was also a vote of thanks to Mr. Henry Watts, the accomplished Editor of the Society's Journal, and to the Abstractors, proposed by Mr. E. C. Nicholson and seconded by Mr. J. Newlands.

Mr. FRISWELL said he would like to ask the President whether there was any bye-law which would give the Council some control over the use which Fellows might make of the privilege of membership, and then proceeded to detail three or four flagrant instances in which the letters F.C.S. had been used for purposes of advertisement.

Mr. KINGZETT then spoke on the same subject, citing other cases, after which—

Prof. WILLIAMSON said it occurred to him to make a suggestion which might be practicable, which was to make the newly-elected Fellow sign some declaration to the effect that he would not make any improper use of his membership. If he did break the contract, it would then be expedient to expel him from the Society.

Dr. RUSSELL having read the present obligation from the obligation-book,

The PRESIDENT said that as it at present stood it was somewhat vague and indefinite, but that the subject had already been under the notice of the Council, and would receive their careful attention. He then declared the meeting to be a Special General Meeting for considering an alteration in the Bye-law relating to the admission of Fellows. After a short discussion it was decided that the admission fee should be raised from two to four pounds, the annual subscription remaining the same as at present.

The next ordinary meeting will be on Thursday, April 7.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, February 22nd, 1876.

EDWARD SCHUNCK, Ph.D., F.R.S. &c., President, in the Chair.

"Notes on a Collection of Apparatus employed by Dr. Dalton in his Researches, which is about to be exhibited (by the Council of the Literary and Philosophical Society of Manchester) at the Loan Exhibition of Scientific Apparatus at South Kensington," by Prof. ROSCOE, F.R.S.

The apparatus employed by John Dalton in his classical researches, whether physical or chemical, was of the simplest, and even of the rudest, character. Most of it was made with his own hands, and that which is to be exhibited has been chosen as illustrating this fact, and as indicating the genius which with so insignificant and incomplete an experimental equipment was able to produce such great results. The Society has in its possession a large quantity of apparatus used by Dalton, most of which, however, consists of electrical apparatus, models of mechanical powers, models of steam-engines, air-pumps, a Gregorian telescope, and other apparatus of a similar kind, which was either bought or presented to him. It has not been thought necessary to exhibit these, but rather to show the home-made apparatus with which Dalton obtained his most remarkable results.

I. Meteorological and Physical Apparatus made and used by Dr. Dalton.

Throughout his life Dalton devoted much time and attention to the study of meteorology; indeed his first work, published in 1793, was entitled "Meteorological Observations and Essays," and his last paper, printed in 1842* (*Mem. Lit. and Phil. Soc.*, vi., 617), consists of auroral observations. Hence the first of Dalton's apparatus which claims attention are the meteorological instruments.

No. 1 is Dalton's mountain barometer, with accompanying thermometer, made for him by the late Mr. Lawrence Buchan, a member of the Society. The barometer is enclosed in a wooden case which Dalton was accustomed to carry in his hand.

Several home-made barometers used by Dalton in his observations are in possession of the Society. They are all of them filled, and the scales prepared, by Dalton himself, and are simple syphon tubes with a bulb blown on at the bottom to serve as a mercury reservoir. These are attached to plain pieces of deal, upon the upper part of which the paper scale is pasted. One of these, which has probably also served for tension experiments (No. 2), has been placed in the collection.

Many of the thermometers appear also to have been home-made. No. 3 is a mercurial thermometer, evidently made and graduated by Dr. Dalton, and marked with his initials, J. D. The freezing-point of this thermometer was tested recently by Mr. Baxendell, who found that it had not altered since the instrument was graduated. Another (No. 4) is of the same kind, and bears the date 1823; No. 5 is a third mercurial thermometer, with long stem and wooden scale; No. 6 is an alcohol thermometer, with wooden scale; and No. 7 a registering maximum and minimum thermometer employed by Dalton—maker's name, J. Ronchetti, 29, Balloon Street, Manchester.

II. Apparatus constructed and used by Dalton in his Researches.

(1) "On the Constitution of Mixed Gases," (2) "On the Force of Steam or Vapour from Water or Other Liquids, at Different Temperatures, both in a Torricellian Vacuum

* Vide "Life of Dalton," by Dr. Henry, published by the Cavendish Society; "Mémorial of Dr. Dalton and the History of the Atomic Theory," published in the *Memoirs of the Literary and Philosophical Society of Manchester*, 2nd series, vol. i.; Dr. Lonsdale's "Life of Dalton," Longmans, 1874.

and in Air," (3) "On Evaporation," and (4) "On the Expansion of Gases by Heat." *

No. 8 is an apparatus used for the determination of the tension of volatile liquids at low temperatures: it consists of a syphon tube, at the upper end of which is a scale in inches in Dalton's handwriting. He describes it thus:—"I took a barometer tube 45 inches in length, and, having sealed it hermetically at one end, bent it into a syphon shape, making the legs parallel, the end that was closed being 9 inches long, the other 36 inches. I then conveyed two or three drops of ether to the end of the closed leg, and filled the rest of the tube with mercury, except about 10 inches at the open end. This done, I immersed the whole of the short leg containing the ether into a tall glass containing hot water."

No. 9 is a smaller tube, containing another liquid, also having a graduated scale written on paper and attached to the tube. Nos. 10, 11, 12, 13, 14, are tubes used by Dalton for measuring the tension of vapour from water and other liquids at higher temperatures than their boiling-points, both in a vacuum and air. No. 15 is a tube used by Dalton for measuring the tension of the vapour of bisulphide of carbon, labelled "Sulphuret carb.," with a paper scale in Dalton's handwriting, and a cork showing that the upper portion of the tube containing the bisulphide of carbon could be heated in a water-bath to various temperatures. No. 16 is a manometer tube, fixed into a board, divided and numbered by Dalton. No. 17 is an apparatus used by Dalton for the determination of the tension of the vapour of ether, and is interesting as being the instrument by means of which Dalton arrived at one of his most important experimental laws. It is described as follows (p. 564):—"The ether I used boiled in the open air at 102°. I filled a barometer tube with mercury moistened by agitation in ether: after a few minutes a portion of the ether rose to the top of the mercurial column, and the height of the column became stationary. When the whole had acquired the temperature of the room (62°) the mercury stood at 17.00 inches, the barometer being at the same time 29.75 inches. Hence the force of the vapour from ether at 62° is equal to 12.74 of aqueous vapour at 172° temperature, which are 40° from the respective boiling-points of the liquids." This is generally known as Dalton's law of tensions, since shown by Regnault not to be rigorously true.

No. 18 is a wet and dry bulb mercurial thermometer, made by H. H. Watson, of Bolton.

III. Apparatus for Measuring Gases, and for Determining the Solubility of Gases in Water.

No. 19 is an apparatus with a graduated tube, probably used by Dalton for the determination of the laws regulating "The Absorption of Gases by Water and Other Liquids," read October 21st, 1803.† No. 20 is a graduated glass tube attached to a bottle of india-rubber, also probably used in his researches on the absorption of gases by water. Nos. 21 and 22 are divided eudiometer tubes, employed by Dalton for measuring the volumes of gases. No. 23 is a spark eudiometer. Nos. 24, 25, and 26 are glass tubes, pipettes, and funnels, graduated by Dr. Dalton and used by him for measuring gases. No. 27 is a graduated glass bell-jar, used for measuring gases. No. 28 is a phial, with graduated tube attached by cement, for collecting and measuring gases. Nos. 29 and 30 are stoppered phials with the bottoms cut off, used as gas jars for collecting and measuring gases. No. 31 is a thousand grains specific gravity bottle, with its counterpoise of lead stamped "175," and paper labelled "Bottle balance." No. 32 is a pipette. No. 33 a square bottle of thin glass, fitted with brass caps, and probably used for the determination of the specific gravities of gases. No. 34 is an earthenware cup, used by Dalton as a mercury-trough, and

containing a small phial with mercury. Nos. 35 and 36 are bulb-tubes, with graduated scales, which may have served for the determination of the coefficients of expansion of gases. No. 37 is a Florence flask with cork and valve, for determining the specific gravity of gases. No. 38 is a glass alembic.

IV. Weights, Balances, Apparatus, Reagents, and Specimens used by Dalton.

No. 39, eleven phials, containing creosote, iodine, amalgam of bismuth and mercury, quercitron bark, grana sylvestra cochineal, and other substances, labelled in Dalton's handwriting. No. 40, three divided blocks, used by Dalton for the illustration of his lectures: these are not, however, the balls an inch in diameter (referred to in his latest memoir on the "Analysis of Sugar") which he employed occasionally in his lectures, as illustrating his newly-discovered laws of combination and the atomic theory; these appear, unfortunately, to be no longer in existence. No. 41 is a common pair of scales used by Dalton. No. 42 a pair of apothecary's scales and weights employed by Dalton, with a paper of weights made of wire, labelled in his handwriting, "rooth grains." No. 43 is a box of weights used by Dalton, and containing a pill-box labelled "Platina," another pill-box labelled "Hund," and containing rooth of grains, and another wooden box containing brass gramme weights, labelled "Weights, French:" the other ordinary weights are of lead. No. 44 is Dalton's pocket-balance, consisting of a small pair of apothecaries' scales, with beam about 4 inches long, and having the pans attached by common string; it is contained in a tin case for the pocket. No. 45 is a penholder used by Dalton. No. 46, leaden grain weights made by Dalton from sheet lead, and stamped in numbers by him; No. 47, iron punches used by Dalton for this purpose. No. 48, a glass lens, wrapped in a piece of paper labelled, in Dalton's writing, "Sun's focus 4.2 inches." No. 49 is a paper containing "roth of grains," made by Dr. Dalton of iron wire. The paper in which these are wrapped is part of a note from one of Dr. Dalton's pupils (as is well known he lived by teaching mathematics at half-a-crown per lesson), in which the writer presents his "compliments to Mr. Dalton, and is sorry that he will not be able to wait upon him to-day, as he is going to Liverpool with a few friends who are trying the Railway for the first time. Mr. D. may fully expect him on Monday at the usual time." No. 50 are bottles of tin, earthenware, and silver, some of them being common penny pot ink-bottles. Each has a thermometer tube cemented into the neck of the bottle, and these tubes are provided with paper scales. These were used by Dalton probably for experiments on radiant heat. No. 51 is a manometer tube used by Dalton: it consists of a tin vessel attached on either side to leaden tubing, and having a thermometer-tube closed at the upper end, and provided with a divided scale, fixed into the upper portion of the tin vessel. No. 52, Dalton's balance, made by Accum, and capable of arrangement as hydrostatic balance, with weights and counterpoises.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Seances, de l'Academie des Sciences. No. 11, March 13 1876.

Silicidation of Platinum and certain other Metals.—M. Boussingault.—The author finds that platinum, palladium, iridium, and ruthenium, if heated to redness in charcoal, do not become carburetted. At very high temperatures silica is reduced by carbon. If platinum is introduced into a mixture of carbon and silica, heated to

* Experimental Essays on the above subjects, by John Dalton, read October 2nd, 16th, and 30th, 1801, and published in the 1st series. vol. v., part 2, of the *Memoirs of the Literary and Philosophical Society of Manchester*.

† *Manchester Memoirs*, 2nd series, vol. i.

whiteness in order to convert it into a silicide, the platinum does not exert an action of presence which determines the reduction of the silica, but merely seizes the silicium as it is set at liberty by the carbon. If silica mixed with charcoal is ignited at a high temperature, free silicon is not found in the mixture after it has been allowed to cool with exclusion of air.

Source of Oxide of Carbon Characteristic of the Formines and the Polyatomic Alcohols.—M. Lorin.—The formic ethers of the monatomic alcohols do not yield carbonic oxide on decomposition, hence the decomposition of the crude formines becomes a new characteristic of the polyatomic alcohols.

Reimann's Farber Zeitung,
No. 12, 1876.

This issue contains an article on cochineal and a paper on preparations for rendering textile fabrics unflammable. The salts recommended for this purpose are the sulphate of ammonia and lime, obtained by mixing sulphate of ammonia with gypsum, and the borate of magnesia, formed by the double decomposition of borax and sulphate of magnesia. The experiments carried out with these salts, at the Geological Institute of Vienna, were very successful.

A letter from Rouen notices the extensive and successful use of the new vanadium aniline-black, both in printing and dyeing. Per litre of dye the proportions preferred are 40 grms. salt of aniline, 20 grms. chlorate of potash, and 0.001 grm. vanadate of ammonia.

Solution of methyl-violet is best made by adding to 1 part of the broken dye 20 parts of water a little below the boiling-point, pouring in the water slowly and stirring slowly but constantly. The colour thus obtained is gradually diluted by the addition of water, with continual stirring. It must be allowed to cool very slowly, and not be filtered till quite cold.

Bulletin de la Société d'Encouragement pour l'Industrie Nationale, No. 27, March, 1876.

This issue contains no chemical matter.

Les Mondes, Revue Hebdomadaire des Sciences,
Nos. 11 and 12, March 16 and 23, 1876.

These issues contain no chemical matter.

MISCELLANEOUS.

The Royal Society.—The President's reception, on Wednesday evening last, was numerously attended, and included a large number of distinguished guests. There were exhibited in the several rooms many objects of interest. The treasurer, Mr. W. Spottiswoode, showed a pair of the largest Nicol prisms yet made; Prof. Tyndall, some infusions exposed to self-cleansed air; Dr. Siemens, his Bathometer, an instrument for indicating the depth of the sea without the use of a sounding-line; also an Attraction Meter, by which the attraction of masses is demonstrated. Mr. H. C. Sorby exhibited a new method of measuring the position of absorption-bands in spectra, and specimens of pigments from human hair; Dr. Schuster, some experiments with the view of showing that the force discovered by Mr. Crookes reacts on the vessel in which the vacuum is; a Photometer, devised by Prof. Osborne Reynolds and Dr. Schuster, for the purpose of measuring the heating effect of light, was also exhibited. Mr. C. J. Woodward showed his new form of Wave-apparatus; while Crookes's Radiometers were exhibited by the manufacturer, Mr. J. J. Hicks. Mr. Crookes exhibited a Torsion Balance and some Experimental Radiometers, illustrating various phenomena connected with the Repulsion resulting from Radiation. A description of the Torsion Balance will be found on page 141. The Experimental Radiometers were as follows:—

1. The Turbine Radiometer. In this Radiometer the vanes are black on both sides, and are inclined at an angle like the sails of a windmill instead of being in a vertical plane. The instrument is not sensitive to horizontal radiation, but moves readily in one or other direction to a candle held above or below.

2. Radiometer with the vanes blacked on both sides, showing rotation in either direction according to the way the light falls on them.

3. Radiometer showing the very small amount of residual air which is present. The vanes of the Radiometer move past a piece of pith suspended by a silk fibre. Rotation with great velocity scarcely causes sufficient motion of the residual air to move the suspended pith.

4. Radiometer showing rotation of the glass envelope when the vanes are held fixed in space. The Radiometer carries a magnet on its arms, and is floated on water so as to be free to move. The vanes are held stationary by an outside magnet. On allowing radiation to fall on the black surfaces of the vanes the glass envelope rotates.

5. Radiometer having inside it a platinum spiral. The Repulsion of the white and black surfaces is equal when the spiral is below redness. Above a red-heat the black is repelled more than the white, and rotation takes place.

6. Radiometer with one vane counterpoised by a mirror, showing method of keeping the steel point from falling off the cup.

7. Radiometer constructed of metal, showing reverse movement on cooling.

8. Bar Photometer, showing the method of balancing one light by another.

9. Heat Engine. A Turbine Radiometer, having ice below and hot air above; working by difference of temperature.

Mr. Apps, Mr. Browning, Messrs. Elliott Bros., Messrs. Tisley and Spiller, and Mr. W. F. Stanley, were also among the exhibitors.

Royal Institution of Great Britain.—The following are the arrangements for Lectures after Easter:—

Prof. P. M. Duncan, F.R.S.—Four Lectures on the Comparative Geology and former Physical Geographies of India, Australia, and South Africa; on Tuesdays, April 25 to May 16.

Prof. Tyndall, D.C.L., LL.D., F.R.S.—Seven Lectures on Voltaic Electricity; on Thursdays, April 27 to June 8.

Prof. W. K. Clifford, F.R.S.—Two Lectures on the Present Relations of Science and Philosophy; on Saturdays, April 29 and May 6.

Prof. W. G. Adams, F.R.S.—Three Lectures on some of Wheatstone's Discoveries and Inventions; on Tuesdays, May 23 to June 6.

Frederick J. Furnivall.—Two Lectures on Chaucer; on Saturdays, May 13 and 20.

J. A. Symonds.—Three Lectures on the Medici in relation to the Renaissance; on Saturday, May 27 to June 10.

The Friday evening meetings will be resumed on April 28th, at 8 p.m. Prof. Gladstone will give a Discourse on Methods of Chemical Decomposition illustrated by Water, 9 p.m. The following Discourses will probably be given by Messrs. G. J. Romanes, W. Froude, C. T. Newton, J. F. Moulton, Sir J. M. Lubbock, and Prof. Tyndall. To these meetings Members and their friends only are admitted.

New Disinfecting Powder.—We have examined an excellent disinfectant sold under the name of "The Universal Disinfecting Powder." It consists of Cooper's salts and sulphate of zinc, and is certainly most effective. We extract the following from a report by Prof. Wanklyn:—"This powder contains 70 per cent of mixed chloride of sodium and chloride of calcium, and about 6 per cent of anhydrous sulphate of zinc (equal to about 12 per cent of hydrated sulphate), a little insoluble matter, and 15 per cent of moisture. I have no doubt that it is an excellent disinfectant. It is, comparatively speaking, non-poisonous and harmless."

TO CORRESPONDENTS.

F. A. Cole.—We should advise you to advertise.
W. M. Paterson.—Asher and Co., of Bedford Street, Covent Garden, will probably supply it.
R. Picard.—Write to the editor of *The Brewers' Journal*.
G. G. Blackwell.—We do not know Dr. Fremy's address.

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CONTENTS OF No. IV.—APRIL, 1876.

Notes on the Composition and Testing of Tin Red Liquors, by Mr. J. H. Jones—On the Recovery of Indigo from Spent Vats, by Mr. G. H. Underwood—Vanadium in Dyeing and Calico Printing—The Manuscript of Jehan le Begue—On the Dip-blue Styles of Calico Prints, by Mr. G. H. Underwood (continued)—Note upon a Method of Preventing the Action of Iron upon Alizarine and Madder Extracts, by M. J. Wagner—Critical and Historical Notes concerning the Production of Adrianople or Turkey Red, and the Theory of this Colour, by Theodore Chateau (continued)—List of English, Scotch, and Irish Calico Printers in 1840—Abridgment of Complete Specifications of Patents Recently Published—British and Foreign Patents, from the Commissioners of Patents Journal, February 22nd, to March 17th, 1876, inclusive—Supplement: The Practice and Principles of Chemical Printing, Bleaching, Dyeing, &c., by Charles O'Neill. Printing (continued).

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PRELIMINARY NOTICE ON THE ACTION OF SULPHURIC ACID ON NAPHTHALENE.

By JOHN STENHOUSE, LL.D., F.R.S., &c., and CHARLES
E. GROVES.

FARADAY,* many years ago, observed that when naphthalene was heated with concentrated sulphuric acid, a mixture of two sulphonic acids was formed. These acids yielded baryta salts, which differed from one another in that one of them burned with flame when heated, whilst the other merely smouldered. Merz and Weith, who have carefully examined these acids,† found that when sulphuric acid (400 grms.) was heated with excess of naphthalene (500 grms.), at 160°, for eight hours, the product, besides "*unaltered naphthalene*," consists almost entirely of β -naphthalene sulphonic acid, with traces of the α -sulphonic acid and of the disulphonic acid. On examining what these chemists believed to be "*unaltered naphthalene*," however, we found that it contained a comparatively large quantity of sulphur compounds insoluble in water, and from which, after a few preliminary trials, we succeeded in isolating three distinct crystalline compounds.

In order to obtain satisfactory results, and to facilitate the subsequent purification of the compounds, it is necessary to employ pure naphthalene. This may be obtained by heating the purest commercial naphthalene, with a small percentage of sulphuric acid, to about 180°, for some hours, and distilling the resulting black mass in a current of steam; repeating the process until the hydrocarbon will dissolve at 100°, in an excess of concentrated sulphuric acid, without the slightest tinge of colour. A mixture of 8 parts of the hydrocarbon with 3 of sulphuric acid are then heated to 180° in a large retort: after the lapse of two or three hours, water begins to come off, and the mercury in the thermometer falls, so that it is necessary to increase the heat in order to maintain the temperature constant. At this stage a reaction appears to set in, which continues for an hour or two, accompanied by the elimination of a considerable quantity of water, which distils over. About an hour after water ceases to come over, the source of heat is withdrawn, and the retort and its contents allowed to cool to 100°: 4 parts of boiling water are then added, and the mixture poured out into a basin or other convenient vessel. On examining the product when cold, it will be found to consist of two layers; the lower one being a dirty greenish, pasty, mass of crystals, of the nearly pure β -sulphonic acid; whilst the upper solid cake, which is of a pale brownish yellow colour, consists of the new sulphur compounds mixed with excess of naphthalene. The solid cake is separated from the crystals of acid, and submitted to distillation in a current of steam in the usual way: the naphthalene then passes off with the aqueous vapour, leaving a brown oily layer at the bottom of the water in the distilling flask, which oil, however, becomes solid on cooling.

Naphthalene Sulphones.—The most convenient way of obtaining the sulphones in the crystalline state from this cake is to powder it and boil it up with carbon bisulphide, filtering hot, and washing the undissolved portion with carbon bisulphide until the washings pass through colourless. The solution, if sufficiently concentrated, when allowed to stand, deposits a considerable quantity of hard, thick, prismatic crystals of α -naphthalene sulphone, mixed, however, with nodular masses of needles, from which

they may be separated mechanically or by repeated fractional crystallisation from alcohol and from carbon bisulphide. The α -sulphone may very readily be purified by alternate crystallisation from these solvents, and it then melts at 123° C. When submitted to analysis it gave numbers corresponding closely with those required by the formula $C_{20}H_{14}SO_2$. It is colourless and insoluble in water, but moderately soluble in boiling alcohol, from which it crystallises out in great part, on cooling, in opaque nodules and transparent plates. It is also moderately soluble in ether and in hot carbon bisulphide, crystallising from the latter in compact, transparent, oblique prisms. It is easily soluble in hot benzene or glacial acetic acid, but only very slightly soluble in petroleum. Hot concentrated sulphuric acid dissolves it, apparently forming a sulphonic acid, whilst with nitric acid the sulphone yields a readily-fusible nitro-derivative, which may be obtained in a crystalline state from hot alcohol. Although the sulphone is not acted on by potassium dichromate and dilute sulphuric acid, a solution of chromic anhydride in glacial acetic acid readily oxidises it. On adding water to the solution a pale yellow substance is precipitated, which is easily soluble in alcohol.

The portion left undissolved, when the crude substance is treated with carbon bisulphide, consists almost entirely of β -naphthalene sulphone, and may readily be purified by a few crystallisations from alcohol, although it is somewhat difficult to obtain it free from all trace of colour. The tufts of needles which crystallise out of the carbon bisulphide solution, along with the α -sulphone, also consist of β -naphthalene sulphone. When pure it crystallises from boiling alcohol, in which it is but slightly soluble, in colourless silky needles. It melts at 177° C., and the results of the analyses that were made showed it to be isomeric with α -naphthalene sulphone. Like that substance, therefore, it has the formula $C_{20}H_{14}SO_2$. The β -sulphone is insoluble in water, very slightly soluble in petroleum, carbon bisulphide, or cold benzene, although it is tolerably soluble in hot benzene and glacial acetic acid. It dissolves in hot concentrated sulphuric acid, forming a sulphonic acid, and when gently heated with nitric acid, sp. gr. 1.45, it first fuses to an orange-brown oily layer, which then rapidly dissolves in the hot acid. If the solution be now allowed to cool, most of the nitro-derivative is deposited as an oil, which solidifies after a time; but if the hot acid solution is diluted with nitric acid, of sp. gr. 1.35, boiled for a minute or two, and set aside to cool, the new compound is deposited in microscopic crystals. These are only very slightly soluble, either in alcohol, glacial acetic acid, or benzene. It yields an amido-compound when treated with tin and hydrochloric acid. The β -sulphone, like its isomeride, is not attacked by potassium dichromate and dilute sulphuric acid, although chromic anhydride and glacial acetic acid readily oxidise it: the addition of water then precipitates a pale yellow body, easily soluble in alcohol.

It was pointed out by one of us some years ago* that phenyl sulphide, $C_{12}H_{10}S$, when treated with oxidising agents, was readily converted into benzene sulphone, $C_{12}H_{10}SO_2$. It seemed of interest to ascertain whether naphthyl sulphide, $C_{20}H_{14}S$, would behave in a similar manner. For this purpose some α -naphthyl sulphide, kindly placed at our disposal by Dr. H. E. Armstrong, was treated with potassium dichromate and dilute sulphuric acid: the compound was rapidly oxidised, but the product was resinous, and resisted all our efforts to obtain from it any crystalline compound resembling the sulphones above described.

We hope soon to be able to give further details with regard to the properties and reactions of the various compounds mentioned in this paper, and also to describe the other crystalline substance which accompanies the sulphones, but differs from them in being exceedingly soluble in carbon bisulphide.

* *Phil. Trans.*, 1826, 140.

† *Deut. Chem. Ges. Ber.*, iii., 195, and *Zeits. Chem.*, vi., 169.

* *Proc. Roy. Soc.*, xiv., 384.

ON THE
INTERFERENCE OF CERTAIN METALS
WITH THE ACCURACY OF
THE PROCESS FOR ESTIMATING COPPER
BY CYANIDE OF POTASSIUM.*

By ROBERT T. THOMSON.

I AM well aware that experiments have been made on this subject to a certain extent by other parties, and that it is generally recognised as a fact that zinc, arsenic, and some other metals interfere with the estimation of copper by the cyanide process, while iron, calcium, magnesium, &c., as far as I am aware, are supposed not to have any effect.

I have undertaken the following experiments with the view of ascertaining the precise effect of the more commonly occurring metals under the usual working conditions. The experiments also include a series instituted to determine the effect of different proportions of hydrate, chloride, and nitrate of ammonium.

In the case of the metals the estimation of the copper in each case was made as follows:—

To the solution containing the copper and the other metal (0.2 grm. of each), a quantity of ammonium chloride solution containing 2 grms. of that salt was added, then ammonia till the precipitated hydrate of copper was just beginning to dissolve, and finally 40 c.c. of ammonia (sp. gr. 0.963) which completely dissolved the hydrate of copper and formed a blue solution. This solution was now made up to 150 c.c. with water, and the cyanide solution added from a burette till only the faintest violet tinge was perceptible in the fluid. The solution was well stirred and allowed to stand for a few minutes after each addition of the cyanide, and in cases where precipitates were formed, the latter were allowed to settle before determining whether the fluid was sufficiently free from colour to indicate that enough cyanide solution had been added.

Potassium and Sodium.—These metals, as was expected, have no appreciable effect on the estimation of copper by this process.

Calcium.—When the potassium cyanide was added to the solution containing the copper and this metal, a white precipitate of calcium carbonate was formed. The formation of this precipitate was owing to the presence of a considerable quantity of potassium carbonate in the potassium cyanide used, which is usually the case with the commercial salt. The white precipitate was allowed to settle after each addition of the cyanide before judging from the colour of the fluid whether the operation was finished. In case, however, the precipitate should in any way interfere with the colour of the liquor (as will be seen afterwards seems to be the case with some other metals) the precipitate was filtered off when the operation was considered as finished, but from the colour of the filtered liquid it was evident that the precipitate did not interfere, the same faint violet tinge remaining as when the precipitate was mixed with the fluid.

It will be seen from Table I. that a larger volume of cyanide solution was consumed than when no calcium was present, showing 0.0027 grm. more copper than was really present. As this difference does not seem to be due to an optical effect (as is probably the case with some of the metals which give coloured precipitates), it may be fairly presumed that it arises from some chemical action between the potassium cyanide and the calcium salt.

Barium and Strontium.—The same remarks are applicable to these metals, although the interference of each metal is greater than that of calcium.

Magnesium.—There is nothing special to note about this metal, the solution remaining perfectly clear, and its interference being comparatively little.

Zinc.—In the case of this metal the solution remained

TABLE I.

Showing Results Obtained by the Cyanide Process for the Estimation of Copper in Presence of some other Metals.

Amount of copper used, 0.2 grm.
Amount of other metal used, 0.2 grm.
Amount of NH_4Cl used, 2.0 grms.
Amount of NH_4HO (sp. gr. 0.963) in excess used, 40 c.c.
Total volume of each solution before adding the cyanide solution, 150 c.c.
C.c. of cyanide used for copper alone:—36.2—36.3;
mean 36.25.

Metal used.	C.c. of Cyanide consumed.	Copper found (apparent)	Error (mean).	Condition of Solution.
Potassium ..	36.1—36.2	0.1991—0.1997	0.0006 L.	Clear
Sodium ..	36.2—36.3	0.1997—0.2003	—	"
Calcium ..	36.8—36.7	0.2030—0.2024	0.0027 H.	Ppt.
Barium ..	37.1—36.8	0.2046—0.2030	0.0038 H.	"
Strontium ..	37.1—37.3	0.2046—0.2057	0.0051 H.	"
Magnesium ..	36.5—36.7	0.2013—0.2024	0.0018 H.	Clear
Zinc ..	43.7—43.8	0.2411—0.2416	0.0413 H.	"
Cadmium ..	38.7—38.8	0.2135—0.2140	0.0137 H.	"
Aluminium ..	37.5—37.6	0.2069—0.2074	0.0071 H.	Ppt.
Iron (ous) ..	—	—	—	"
Iron (ic) ..	35.9—36.0	0.1980—0.1986	0.0017 L.	"
Manganese ..	27.4—27.1	0.1511—0.1495	0.0497 L.	"
Chromium ..	—	—	—	"
Cobalt ..	—	—	—	Clear
Nickel ..	80.0—79.8	0.4413—0.4403	0.2408 H.	"
Uranium ..	37.0—37.3	0.2041—0.2057	0.0049 H.	"
Tin (ous) ..	33.0—32.8	0.1820—0.1809	0.0186 L.	Ppt.
Tin (ic) ..	36.8—36.7	0.2030—0.2024	0.0027 H.	"
Molybdenum ..	36.2—36.2	0.1997—0.1997	0.0003 L.	Clear
Arsenic ..	36.2—36.3	0.1997—0.2003	—	"
Antimony ..	36.9—37.0	0.2035—0.2041	0.0038 H.	Ppt.
Bismuth ..	37.0—37.3	0.2041—0.2057	0.0049 H.	"
Lead ..	36.4—36.4	0.2008—0.2008	0.0008 H.	"
Mercury(ous) ..	43.3—43.1	0.2388—0.2377	0.0382 H.	Clear
Mercury(ic) ..	44.6—44.8	0.2460—0.2471	0.0465 H.	"
Thallium ..	36.9—37.1	0.2035—0.2046	0.0040 H.	Ppt.
Silver ..	47.7—47.8	0.2631—0.2637	0.0634 H.	Clear
Gold ..	48.1—48.1	0.2653—0.2653	0.0653 H.	"
Platinum ..	43.2—43.3	0.2383—0.2388	0.0385 H.	Ppt.
Palladium ..	58.8—59.0	0.3244—0.3255	0.1249 H.	Clear

(H., high. L., low.)

clear. It interferes with the estimation in a greater degree than any of the metals yet noticed, giving about one-fifth more copper than was actually present.

Cadmium.—This metal also interferes in the same direction as zinc, though not nearly to such an extent as the latter metal.

Aluminium.—In the case of this metal the white gelatinous precipitate of alumina was thrown down by the ammonia used. The amount of copper obtained was considerably too high.

Iron (ous).—An attempt was made to estimate the copper in presence of iron in the ferrous state of combination, but it was unsuccessful owing to the incomplete precipitation of the iron as ferrous hydrate by the ammonia, and the consequent continuous precipitation of oxide of iron from the solution by the action of the air. The precipitation of the oxide of iron went on for hours, keeping the solution so muddy that it was impossible to perceive the blue colour of the fluid. By this time the ferrous hydrate was pretty well converted into ferric hydrate, and though the precipitate now settled and left the fluid clear, it was useless to proceed with the operation, as it would simply be the estimation of copper in presence of iron in the ferric state. This matters the less as it is not necessary to estimate copper in presence of iron in the ferrous state, the conversion from the latter into the ferric condition being very easy.

Iron (ic).—All the iron is precipitated in this case by the ammonia as ferric hydrate, and it is customary and is

* Read before the Chemical Section of the Philosophical Society, Glasgow, December 6, 1875.

considered most accurate to estimate the copper in presence of this precipitate. This is done because of the difficulty of separating the copper by precipitating the ferric hydrate with ammonia, a portion of the copper being retained by the precipitate even though the latter be re-dissolved and re-precipitated.

It will be seen from Table I. that when the ferric hydrate precipitate is retained in the liquor a low result is the consequence. After the operation seemed to be finished, the precipitate was filtered off, but on examining the filtrate the colour was found not to be discharged to the proper extent. The addition of the cyanide solution to the clear fluid was therefore continued till the proper point was reached, and in the first example given in the Table 1.1 c.c., and in the second 1 c.c., more of the cyanide was consumed, making in all 37 c.c. in each experiment. These results show that when the precipitate was retained in the liquor, the error was, as a mean, 0.0017 grm. too low, and in the filtrate 0.0041 grm. too high. Other experiments made when the ferric hydrate precipitate was not allowed to settle so completely as in the experiments just mentioned, gave considerably lower results than those given in the Table.

The cause of these differences is manifestly of a two-fold character. In the case of the determination of the copper in the presence of the ferric hydrate, the result, as already stated, is too low, and this seems to arise from an optical effect, the colour of the precipitate seeming to neutralise the blue tint of the fluid above it, so that when the clear solution is poured or filtered off, the blue tint still remaining is intense enough to be perceived at once. This view is supported by the fact that when a disc of red paper is placed at the bottom of the filtered fluid, the blue colour of which is very distinct, the colour of the fluid is partly neutralised. The fact that the filtered fluid consumed such an additional amount of cyanide points to the probability that there is some chemical action between the cyanide and the iron compound.

Manganese.—With regard to this metal it will be seen from Table I. that the amount of copper found was about one-fourth too low, an effect quite the reverse of that produced by zinc. In this case also, as in that of iron in the ferric state, the presence of the precipitate influences the colour of the solution, making it appear nearly colourless, when really its blue tint is quite apparent on being filtered off. Indeed the filtered liquid consumed in the first example given in the Table 1.2 c.c., and in the second 2.3 c.c., more of the cyanide solution, giving still a mean error of 0.04 grm. too low.

Chromium.—The oxide of chromium was precipitated by the ammonia, and after the addition of a limited quantity (about 24 c.c.) of the cyanide solution, the fluid, after the precipitate had settled, was seen to have a greenish blue colour. The precipitate was now filtered off and more cyanide added, but when from 30 to 35 c.c. had been used, the filtrate assumed a pinkish brown colour. It is obvious, therefore, that it is impossible to decide when the estimation is finished.

Cobalt.—When about 40 c.c. of the cyanide solution were added to the solution, the latter assumed a greenish blue colour, and on further addition the solution became greenish with a brown tinge, but after the addition of about 70 c.c. the green altogether disappeared, and the solution acquired a brownish colour. It is evident that it is impracticable to estimate copper in presence of large quantities of cobalt, owing to the impossibility of determining the point at which the operation is completed, and even though that were possible, a high result would be obtained.

Nickel.—In the case of this metal the solution remained clear during the whole process. The amount of copper brought out, as will be seen in Table I., is more than double the actual quantity present.

Uranium.—When ammonia was added to the solution a precipitate of uranate of ammonium was thrown down, which dissolved on the subsequent addition of the cyanide

of potassium. After about 35 c.c. of the cyanide solution had been added the solution became green, but the estimation could not be considered as finished, because this colour was evidently owing to the mixture of the blue copper salt with the yellow uranium salt, and therefore as long as the green colour remained enough cyanide had not been added. The addition of the cyanide solution was now continued, and when 37 c.c. in the one experiment, and 37.3 c.c. in the other, were consumed, the solution assumed a yellow colour.

Tin (ous).—With tin in the stannous condition a white precipitate was formed by the ammonia, which remained insoluble. The copper found was much too low.

Tin (ic).—With tin in the stannic condition a white precipitate was formed by the ammonia, which did not dissolve to any extent sensible to the eye. The amount of copper found was somewhat too high, an effect the reverse of that in the case of the stannous compound.

Molybdenum.—This metal has no appreciable effect.

Arsenic.—This metal, whether present in the form of arsenious or arsenic acid, does not interfere with this process, the mean of the two experiments, as will be seen by reference to Table I., giving exactly the amount of copper present.

Antimony.—In the case of this metal a white precipitate was formed, which remained insoluble. The amount of copper found was considerably too high.

Bismuth.—With this metal the white hydrate of bismuth was precipitated by the ammonia, and it remained insoluble during the whole process. The error was considerable, and on the high side.

Lead.—A white precipitate was also formed with this metal, which did not re-dissolve. The interference was very slight.

Mercury (ous).—On the addition of the ammonium chloride, mercurous chloride was precipitated, which was turned black by the ammonia, and dissolved by the potassium cyanide. About one-fifth more than the actual amount of copper was indicated in this case.

Mercury (ic).—In this case mercuric chloride was used, and when the ammonia was added "white precipitate" was formed, which dissolved completely in the potassium cyanide subsequently added. The error in this case was rather greater than, and in the same direction as, in the preceding case.

Thallium.—This metal, which was used in the form of nitrate, gave a white precipitate of thallous chloride by the addition of the ammonium chloride, but this precipitate was partially dissolved by the ammonia and potassium cyanide subsequently added. The mean of the two experiments shows an error of 0.004 grm. too high.

Silver.—The addition of the ammonium chloride produced a precipitate of chloride of silver which immediately dissolved in the ammonia, and the solution remained clear during the estimation. A result giving 0.0634 grm. of copper too high, which is about one-third of the amount of copper present, was obtained.

Gold.—By the addition of the ammonia the yellow aurate of ammonium was precipitated, but was gradually dissolved by the potassium cyanide. The error is equal to nearly one-third of the copper present (too high).

Platinum.—The ammonium chloride caused the precipitation of the yellow crystalline ammonio-chloride of platinum, but on the addition of the potassium cyanide the greater part of the precipitate dissolved, and only a small quantity of a greenish flocculent precipitate remained insoluble. A mean error of 0.0385 grm. too high was the result. A curious circumstance in connection with the interference of this metal may here be noticed. After the process seemed to be finished the small precipitate was filtered off, and after the filtered solution had stood over night (sixteen or eighteen hours) the solution again assumed a blue colour, and in the first experiment given on Table I., required 1.8 c.c., and in the second 2 c.c. of the cyanide solution to discharge the colour to the proper

extent. This returning of the blue colour is remarkable, because, when copper alone is present, or when it is along with any of the other metals noticed, the tendency is for the faint colour left in the solution, altogether to disappear and leave the solution colourless.

Palladium.—On the addition of the ammonia, the flesh-coloured ammonio-chloride of palladium was precipitated, which did not dissolve to any extent perceptible to the eye, in the excess of ammonia. When the potassium cyanide was added, using a few c.c. at a time, and allowing to stand for a short time after each addition, the precipitate gradually dissolved. In the second experiment the potassium cyanide was added rapidly, and when about 48 c.c. had been added the blue colour of the fluid was completely discharged, but the ammonio-chloride of palladium was not dissolved. After standing for a few minutes, however, the precipitate dissolved and the liquid became blue again, and 11 c.c. more of the potassium cyanide solution were required to complete the estimation. The mean error shown by these experiments is 0.1249 gm. too high, or above one-half more than the real amount of copper present.

The Interference of Ammonia in Various Proportions with the Accuracy of the Process for Estimating Copper by Cyanide of Potassium.

In the experiments made on the interference of ammonia, the same quantities of copper and ammonium chloride were used as with the metals, and the estimation of copper taken as the standard of reference was that obtained when 40 c.c. of ammonia of sp. gr. 0.963 was present, being also the same as that used in the case of the metals. The amount of interference caused by ammonia does not rise in proportion to the quantity used, but as more ammonia is used the additional amount of interference becomes less, as will be seen by reference to Table II.

TABLE II.

Showing Results Obtained by the Cyanide Process for the Estimation of Copper in Presence of Ammonia in Various Proportions.

Amount of copper used, 0.2 gm.

Amount of NH_4Cl used, 2.0 grms.

Total volume of each solution before adding the cyanide solution, 150 c.c.

C.c. of cyanide consumed in presence of 40 c.c. of ammonia (sp. gr. 0.963):—36.2—36.3; mean 36.25.

Amount of Ammonia (sp. gr. 0.963) used.	C.c. of Cyanide consumed.	Copper found (apparent).	Error (mean).
60 c.c.	37.0—37.1	0.2041—0.2046	0.0043
80 „	37.6—37.8	0.2074—0.2085	0.0079
100 „	37.9—38.0	0.2090—0.2096	0.0093
120 „	38.1—38.2	0.2102—0.2107	0.0104

The Interference of Ammonium Chloride in Various Proportions with the Accuracy of the Process for Estimating Copper by Cyanide of Potassium.

In the experiments made on the interference of ammonium chloride, the same quantities of copper and ammonia were used as in the case of the metals, but the standard to which the results were referred was changed to the result obtained when no ammonium chloride was used. The extent of interference, as in the case of ammonia, does not rise in proportion to the quantity of ammonium chloride present, as will be seen by reference to Table III.

The Interference of Ammonium Nitrate in Various Proportions with the Accuracy of the Process for Estimating Copper by Cyanide of Potassium.

In the experiments made on the interference of this salt, the same quantities of copper and ammonia were used as in the experiments with ammonium chloride, and the results were compared with that obtained when no nitrate or chloride of ammonium was present. The interference

caused by 2, 4, and 6 grms. of ammonium nitrate corresponds very closely to that caused by the same quantities of ammonium chloride, but the interference of 8 and 10 grms. differs entirely from that of the corresponding quantities of ammonium chloride, the former being considerably greater than the latter. The results are given in Table IV.

TABLE III.

Showing Results Obtained by the Cyanide Process for the Estimation of Copper in Presence of Ammonium Chloride in Various Proportions.

Amount of copper used, 0.2 gm.

Amount of NH_4HO (sp. gr. 0.963) used, 40 c.c.

Total volume of each solution before adding the cyanide solution, 150 c.c.

C.c. of cyanide consumed.

Amount of NH_4Cl used.	C.c. of Cyanide consumed.	Copper found (apparent).	Error (mean).
2 grms.	36.2—36.3	0.2077—0.2083	0.0080
4 „	36.9—36.9	0.2117—0.2117	0.0117
6 „	37.4—37.5	0.2146—0.2152	0.0149
8 „	37.6—37.6	0.2157—0.2157	0.0157
10 „	37.7—37.8	0.2163—0.2169	0.0166

TABLE IV.

Showing Results Obtained by the Cyanide Process for the Estimation of Copper in Presence of Ammonium Nitrate in Various Proportions.

Amount of copper used, 0.2 gm.

Amount of NH_4HO (sp. gr. 0.963) used, 40 c.c.

Total volume of each solution before adding the cyanide solution, 150 c.c.

C.c. of cyanide consumed.

Amount of NH_4NO_3 used.	C.c. of Cyanide consumed.	Copper found (apparent).	Error (mean).
2 grms.	36.2—36.3	0.2077—0.2083	0.0080
4 „	36.8—36.9	0.2111—0.2117	0.0114
6 „	37.5—37.4	0.2152—0.2146	0.0149
8 „	38.3—38.4	0.2197—0.2203	0.0200
10 „	38.6—38.6	0.2215—0.2215	0.0215

It is obvious from these results that, in the estimation of copper by cyanide of potassium, the presence of the common metals, with very few exceptions, must be avoided. It is also evident that great care must be taken as to the quantity of ammonia and ammonium salts present; indeed that the same proportion of these salts should be used in each estimation as is used in the standardising of the cyanide of potassium solution.

In conclusion, it may be noticed that the slight interference cause by some of the metals may be accounted for, in part at least, by the action of the salt of ammonium formed by the decomposition of the salt of the metal when the ammonia is added.

STATE OF ANIMAL CHEMISTRY IN AUSTRIA.

(OPEN LETTER TO THE IMPERIAL ACADEMY OF SCIENCES AT VIENNA, CONTAINING AN EXAMINATION OF THE RESEARCHES ON THE COLOURING MATTER OF BILE, BY RICHARD MALY, OF GRAZ.)

By J. L. W. THUDICHUM, M.D., London.

1. THE 72nd volume of the *Proceedings of the Imperial Academy of Sciences* (Part 3, October, 1875) contains a paper by Richard Maly, of Graz, entitled "On the Action of Bromine upon Bilirubin," which compels me to communicate to the Imperial Academy the following statement:—

2. The paper by Prof. Maly alluded to is described as the fifth of a series, the first and second members of

which are printed in the 57th and 59th volumes of the *Proceedings* of the Academy: the third paper is contained in *Liebig's Annalen* (vol. clxiii.); the fourth, again, in the *Proceedings* of the Academy (vol. lxx.). I am compelled to refer to these four previous papers by the contents of the fifth in the 72nd volume of the *Proceedings* of the Academy.

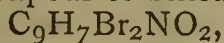
3. In the introduction to the fifth paper Prof. Maly states that, through some experiments, undertaken with the view of learning something about the blue product of oxidation which nitric acid produces with bilirubin, he "has been led to the study and discovery of a body which was totally different from the one he had proposed to himself." Incidentally he remarks that his experiments are the fruit of a year's labour.

4. Prof. Maly next recants the opinion which he had maintained in all his former papers, and which in the second paper he had endeavoured to prove by many and laborious volumetric experiments, according to which the products of the action of bromine upon bilirubin owe their origin to a process of oxidation; and he then describes, after many remarks upon the alleged striking similarities between the action of nitric acid and that of bromine upon bilirubin, his great surprise on finding that the products of the action of bromine upon bilirubin are by no means oxidised, but contain bromine in substitution for hydrogen.

5. In a footnote Prof. Maly then states:—"After I had concluded the following research, and drafted the manuscript, I became acquainted with a research by L. W. Thudichum, 'Further Researches on Bilirubin and its Compounds,' which is contained in the lately-published (May) number of the *Journal of the Chemical Society*. I have learned from this that Thudichum also has recognised that the action of bromine upon bilirubin produces *bromo-products*; therefore this observation, which has also been made by Thudichum, is in any case, on account of prior publication, to be considered as his property."

6. I interrupt the quotation of the footnote in order to point out that Prof. Maly represents to the Academy that he had discovered the substitution by bromine, of hydrogen in bilirubin, by means of his own experiments; that he had himself independently found out and corrected his former errors, which he had maintained for years, and which have been repeated in many chemical publications; and particularly that he had only obtained knowledge of my researches after he had completed his research and written out the draft of his paper.

7. However, on June 11, 1874, I had directed a letter to Prof. Maly, in the course of which I had made to him the following communication, given in the words of the copy which I preserved:—"I was pleased that as regards biliverdin you now agree with me. You double the formula, but without giving any reason for it. However, in the course of time our agreement will, no doubt, become greater yet. You still believe, per example, that the products of the action of bromine upon bilirubin are products of oxidation, whereas I have shown already, two years ago, that they are products of substitution. Simple addition of bromine vapour to bilirubin gives—



and determines the atomic weight of bilirubin accurately at 163, the same number which results from all my other researches. Städeler has made many calculations to be enabled to use my analyses for the construction of a theory of a hexabasic acid, but has not succeeded. I rely upon numerous experiments and analyses, and these cannot be calculated away on paper."

8. That Prof. Maly had received the letter containing the foregoing passages is proved by the reply which he addressed to me, dated from Innsbruck, June 14 (1874), now before me. Prof. Maly, therefore, before he began the experiments which are so exhaustively described in the fifth paper, was not only informed of his error, but actually in possession of the key to his alleged discovery, and it was therefore impossible that he should have been led to this discovery by his experiments.

9. It is further quite clear, from the connection of the statement of Prof. Maly, that if he had not obtained information of my paper in the *Journal of the Chemical Society*, he would in that case also have ignored my letter to him, and would have claimed the priority of the discovery of the brominated product of bilirubin. For although, in the beginning of his fifth paper, he declares, as he expresses himself, "the characterisation of the hitherto existing knowledge concerning the blue product of bilirubin" to be a "usual duty," yet in enumerating previous data he neither mentions my letter nor the fact that I had published the existence of the bromo substitution-product of bilirubin already, in the year 1872, in my "Manual of Chemical Physiology" (London, 1872, p. 72), and had communicated this observation to the Chemical Section of the German Association for the Advancement of Science, at the meeting at Wiesbaden. He is perfectly silent regarding the numerous blue and variously coloured derivatives of the colouring matters of gallstones, which I was the first to characterise from my own researches, chemically as well as spectroscopically, in my Report in the 9th and 10th "Reports of the Medical Officer of the Privy Council" (1866 and 1867, pp. 251 to 260), and in the Manual alluded to. That these researches and publications should have remained unknown to the editor of an annual report on the progress of animal chemistry is not impossible, but that he excluded the contents of my letter from the circumference of the "usual duty" admits of only one explanation, but not of justification.

10. I now proceed to consider the representations concerning my results which Prof. Maly has made to the Academy, in the footnote already partially quoted, and in the appertaining text of his paper. This description leaves the main points which have been established by my researches entirely out of consideration, and in all particular statements it is completely incorrect. Indeed I can hardly believe that Prof. Maly has read my paper; it is certain he has not understood it.

11. In the note alluded to Prof. Maly says—"Thudichum has not analysed his body, but has only drawn a conclusion concerning its composition from the increase in weight which bilirubin undergoes when bromine vapour is passed over it. When bromine was passed for a short time Thudichum obtained a body which was soluble in alcohol with almost monochromatic blue colour, and contained 35.30 per cent Br; and this, in accordance with Thudichum's formula for bilirubin, is said to be mono-bromo-bilirubin, for which, however, calculation requires 33.0 per cent Br. This substance, from its properties, may correspond to the body which I describe in this paper, but without being approximately pure."

12. These statements are exclusively taken from the preliminaries of my paper, which I relate only because they lead to and necessitate the performance of my cardinal experiment. The product containing 35.30 per cent Br I have never declared to be mono-bromo-bilirubin; on the contrary, I have stated that by solution in concentrated sulphuric acid, and precipitation with water, to which it had been subjected, it had acquired different properties—among others, a green colour. I say expressly that the product before treatment with sulphuric acid appeared to have been a mixture of mono- and dibromo-bilirubin. I then assert that these bodies cannot easily be separated from each other by ordinary solvents.

13. Prof. Maly says further in the note—"When bromine is passed for a long time (over bilirubin), then, according to Thudichum, a body is produced which is yet richer in bromine, and this he claims to be bromo-bilirubin." And further in the text—"It is therefore" (namely, because according to Maly the proper point of bromination can only be discovered with the aid of solvents) "impossible to draw any conclusion regarding the composition of the products of the two experiments of Thudichum, in which bilirubin was exposed for some time to bromine vapour, and then weighed, for in this form the body is always fused like a resin."

14. This statement of Prof. Maly is not only contrary to my actual description, but is directly opposed to the stated motives which led to my experiment. For I say that it was evident, from the preliminary experiments, that bilirubin in the presence of hydrobromic acid and moisture could not be completely brominated, just because it deliquesced, and that for this reason I had instituted an experiment in which this accident was completely avoided.

15. I then describe the experiment as follows:—"A quantity of finely-powdered bilirubin was placed in a Liebig's drying apparatus, and dried at 100° C. in a current of dry air. Next, a current of dry air, which had passed over some dry bromine contained in a little bulb, was passed slowly over the bilirubin in the cold, as long as the bromine was absorbed. Care was taken to agitate the bilirubin, and keep it throughout in a mobile powdery state. When the bromine passed unchanged through the apparatus into the aspirator, the bromine bulb was removed, and dry air only was conducted through the apparatus. It was now found that the weight of the bilirubin had exactly become treble what it was at the beginning of the experiment. The apparatus was now heated to 100°, while a continuous current of dry air was passed through it. Much hydrobromic acid was removed, and after several hours of drying the weight of the apparatus became stationary.

Bilirubin employed dry at 100° C. = 1.2280 grms.

Bromine added -2H at 100° C. = 1.1942 grms.

The equation—

Increase by Br :	Wgt. of Bilirubin :	Atomic Weight :	Atomic Weight
	taken.	of 2Br - H ₂ .	of Bilirubin.
1.1942 :	1.2280	=	158 : 162.4.

This number of 162.4 is therefore the atomic weight of bilirubin as deduced by this experiment, and this is very close to 163 as deduced by all my other experiments (see *Erdmann's Journ. d. Pract. Chemie*, 104, 193). The atomic weight of 163 requiring an addition of 158 to form C₉H₇Br₂NO₂, requires by calculation an addition of 1.1903 to the bilirubin employed, which, again, is very near to the actual number found."

16. I further describe the properties of the new compound, and particularly that it becomes quickly changed in several solvents. It would have been more to the interest of Prof. Maly if he had studied my statements relating to these changes, and had repeated the experiments,—if he had heeded my warning, and excluded moisture and hydrobromic acid from his experiments: it would have saved him much disappointment had he comprehended that I saturated bilirubin with bromine by offering it an excess, and did not only, as Maly reports, pass it over it for some time. All these necessary precautions Prof. Maly has neglected, and in consequence has arrived at conclusions which have no foundation.

17. Prof. Maly further endeavours to influence the judgment of the Academy by raising doubts in general regarding my experiments; first, on the ground that I had performed each experiment only once; secondly, because I had not analysed the final product. Against these objections I maintain that the above experiment, considered by the light of my former researches in the *Journ. d. Pract. Chem.* (civ., 193), requires no further analysis. I thought and think every analysis of the product to be a mere waste of time,—every repetition on my part a waste of labour and material. However, in order to meet the objection, and from a high regard for the Academy, I have repeated the experiment described under 15, yet two several times, and have analysed the products by determining quantitatively the amounts of carbon, hydrogen, nitrogen, and bromine. I give here the results of the experiments and analyses, and observe, at the same time, that during the elementary combustion none of the hypothetical bromide of carbon was observed, with the alleged escape of which Prof. Maly endeavours to explain his discordant carbon-numbers.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, April 7th, 1876.

Professor ABEL, F.R.S., President, in the Chair.

THE minutes of the previous meeting having been read and confirmed, and the presents made to the Society announced, the following names were read for the first time:—Messrs. C. G. Matthews, M. Hawkins Johnson, H. C. Jones, A. J. Brown, Purna Chandra Sen, E. Hunter, G. Allanby, H. A. Milnes, and D. Sullivan. Messrs. Thomas Berham Udall, M.A., and Josiah Christopher Gamble were duly elected, after their names had been read the third time.

The first paper, "*A Preliminary Notice on the Action of Sulphuric Acid on Naphthalene*," by Dr. J. STENHOUSE and Mr. C. E. GROVES, was read by the latter. (This is published *verbatim* at page 151 of this journal.)

Prof. T. E. THORPE communicated three notes from the laboratory of the Yorkshire College of Science. The first of these was "*On the Action of the Copper-Zinc Couple on Potassium Chlorate and Perchlorate*," by Mr. H. ECCLES. It has been found that the copper-zinc couple, when properly prepared, completely reduces the chlorates and iodates, so that this reaction may be made the basis of a quantitative method. As the couple is entirely without action on potassium perchlorate, the author has taken advantage of this fact to verify the statement that the decomposition of potassium chlorate, when heated, takes place in two phases, with the intermediate production of perchlorate.

The second of these notes was on "*Thallium Chlorate*," by Mr. J. MUIR. The author prepared the chlorate from thallium sulphate and barium chlorate in equivalent proportions, concentrating the filtered solution by evaporation. The salt forms anhydrous microscopic crystals of the formula TlClO₃. They are permanent in the air, and readily soluble in hot water without decomposition. The specific gravity of the crystals at 9° C. is 5.5047.

Mr. J. WILLIAMS said that some years ago he had occasion to prepare several pounds weight of thallium chlorate, which he did by decomposing thallium sulphate with barium chlorate, and could entirely confirm the author's statements with regard to the properties of the salt.

The last note was by Mr. THORPE himself, "*On the Isometric Relations of Thallium*." The author has compared various thallium salts with the corresponding potassium and ammonium compounds, and finds in many instances that their specific volumes are identical. This is the case with potassium and thallium chlorates, carbonates, and nitrates, and also with ammonium and thallium chlorides, sulphates, dihydrogen phosphates, hydrogen oxalates, and tartrates. These salts are isomorphous as well as isometric.

Prof. THORPE, in reply to a question put by Prof. McLeod, said he had not determined the molecular volume of any of the triad compounds of thallium:

The last paper was by Dr. H. E. ARMSTRONG, "*On the Nomenclature of the Carbon Compounds*." After adverting to the paper on this subject by Prof. Odling in the *Philosophical Magazine*, especially in relation to Schorlemmer's classification of the paraffins, he said that his main objection to Prof. Odling's system was that in going as far as it does, it did not go far enough. The speaker thought that a systematic nomenclature should indicate as clearly as possible the nature of the relation between the homologues and isomerides, and also that each member of the series of isomerides should bear the same root name, and that this should be disguised as little as possible by the index, prefix, or suffix employed to distinguish one member of the series from the other. He proposed to consider the alcohols as derivatives of methyl-alcohol, and adopting

Kolbe's nomenclature, to divide them into primary, secondary, and tertiary carbinols according as one, two, or three of the hydrogen atoms in carbinol, CH_3OH , were replaced by monad radicals, and, further, to extend the terms primary, secondary, and tertiary to the aldehyds and acids as derived from acetic aldehyd, CH_3COH , and acetic acid, CH_3COOH . The method by which he proposed to distinguish the isomerides formed from the same parent substance by the introduction of isomeric radicals, was by the employment of the letters of the Greek alphabet, not merely to distinguish them one from the other, but also to indicate their position in the series; the member which has the highest boiling-point and specific gravity being placed first, the remaining terms being arranged in the order of their boiling-points; for it is difficult to avoid the impression that the physical properties, and what is termed the chemical constitution, of a compound are in some way correlated. This system was illustrated by reference to the alcohols. He proposed to call the two propylic alcohols $\text{C}_2\text{H}_5\text{CH}_2\text{OH}$, and $(\text{CH}_3)_2\text{CH.OH}$ α and β propylic alcohols respectively, and the corresponding monad radicals derived from these by the subtraction of OH , α and β propyl. By the substitution of an atom of hydrogen in carbinol by one or other of these two radicals we should have two primary tetrylic alcohols, α -propyl-carbinol and β -propyl-carbinol, besides a secondary alcohol, ethyl-methyl-carbinol $(\text{C}_2\text{H}_5)(\text{CH}_3)\text{CH.OH}$, and a tertiary alcohol, trimethyl-carbinol, $(\text{CH}_3)_3\text{C.OH}$. These four alcohols might be called α , β , γ , and δ butyl alcohols respectively, and the corresponding monad radicals would be α , β , γ , and δ butyl. In a similar way these radicals would give rise to four primary amylic alcohols, which, with the three secondary and one tertiary amylic alcohol, would yield eight corresponding monad radicals (amyls). By extending this principle to the C_6 series we could, in like manner, provide a systematic nomenclature for the hexylic alcohols.

The PRESIDENT said the importance of the subject introduced by Dr. Armstrong could not be over-estimated, for all workers in chemical research must wish for the attainment of something like uniformity in the nomenclature employed to distinguish the numerous isomeric compounds. He thought, however, that as there was comparatively little time that evening, it would be advisable to adjourn the discussion until the next meeting, Thursday, April 20. He would also remind the Fellows that there would be a Special Meeting on Friday, April 28, when Prof. Andrews would deliver a lecture "On Certain Methods of Physico-Chemical Research."

PHYSICAL SOCIETY.

April 8th, 1876.

Mr. W. SPOTTISWOODE, Vice-President, in the Chair.

THE following gentleman was elected a Member of the Society:—Mr. H. M. Klaassen.

Prof. FOSTER exhibited and described an instrument for illustrating the law of refraction. It is founded on the well known method of determining the direction of the ray after refraction by means of two circles described from the point of incidence as centre, the ratio of whose radii is the index of refraction. If the incident ray be projected to meet the inner circle, and through the point of intersection a vertical line be drawn, the line drawn from the point of incidence to the point where this meets the outer circle is the direction after refraction. This principle is applied in making a self-adjusting apparatus as follows:—A rod representing the incident ray is pivoted at the point of incidence, and projects to a point about 4 inches beyond. To this extremity is attached a vertical rod, which slides through a nut in another rod also pivoted at the point of incidence. The lower extremity of the vertical rod is attached to a link, so fixed as to constrain it to remain vertical. By this means the two rods always represent respectively the incident and refracted rays, and the index of

refraction can be varied by altering the position of the nut, through which the vertical rod passes, on the rod to which it is attached.

Prof. FOSTER then exhibited a simple arrangement for showing the interference of waves. It consists of two glass plates, placed one in front of the other, on each of which is drawn the ordinary sine wave. They are supported in a frame, and behind them is a paper screen, bearing lines to indicate the points of maximum and minimum displacement. The plates can be made to slide in opposite directions, and all the phenomena of wave motion generally and the state of the air in open and closed tubes can be shown.

Lastly, he exhibited a method, which has been suggested by Prof. Kundt, for showing in a simple manner that the air in an organ-pipe is in a constant state of alternate condensation and rarefaction. At the upper end of a closed pipe are placed two valves opening inwards and outwards respectively, and the chambers behind these are connected by india-rubber tubes with small water gauges, which, for the sake of exhibition, were projected on the screen. The gauges were to the eye permanently set, showing at the same time condensation and rarefaction, an appearance which was, of course, due to the rapidity of change. It was shown that beats cause the air to approximate to its normal density.

Prof. GUTHRIE exhibited and described an arrangement which he thought might be useful for determining the rate at which machinery is revolving. The instrument is analogous to one which he devised some years ago for rendering a galvanic current constant. The chamber of a manometer is connected with a small force-pump, which makes one complete stroke for every revolution of the engine. A capillary glass tube affords a means of escape for the air introduced by the pump into the manometer. If now the pump be worked uniformly—that is, the engine rotates uniformly—the pressure in the manometer will shortly attain a position of equilibrium, so that the mercury will remain stationary. But if the velocity of the engine increase, the mercury will immediately ascend, and so indicate this increase of speed. The main objection to the instrument, as exhibited, was the oscillation of the mercury, but this might be avoided in several ways which were pointed out.

Mr. COFFIN referred to some works in America where he had seen a similar principle applied. The engine was connected with an air-chamber, to which was applied a Bourdon's gauge, the indications of which gave an approximate measure of the revolutions of the engine.

Prof. UNWIN thought there would be some difficulty in keeping the capillary orifice perfect for any length of time. He referred to a proposal made by Prof. Thomson in about 1852 to use a centrifugal pump for a similar purpose.

ROYAL SOCIETY OF EDINBURGH.

AT the meeting held on the 3rd inst. Prof. DITTMAR, of Glasgow, gave a description of two new laboratory appliances of his invention, viz., (1) of a modification of the *precision balance*, professing to enable one to execute exact weighings with great rapidity; and (2) of a *gas governor* founded on electro-magnetic principles.

The *new balance* differs from the customary instrument only in this, that two contrivances which are met with in every complete balance—viz., the rider arrangement and the "gravity bob"—are brought into new forms in which their potential usefulness is more completely exhausted. To enable one to dispense with weights less than 0.1 grm., both arms of the beam are graduated so that in each case both the 0 and the 10 mark are at accessible points of the respective arm, the 0 and the 10 on the left side being situated symmetrically to respectively that 10 and the 0 on the right. The right-side scale consists of the customary marks the left-side one of *notches* filed into the back of the beam. There are two riders, one weighing p centigrms.

for the left, and another weighing p milligrms. for the right scale, p being chosen so that, supposing the heavier (or the lighter) rider to be shifted towards the right through n divisions, this virtually amounts to the addition of n centigrms. (or milligrms.) to the charge in the right pan. The weight of this pan is so adjusted against that of the left one that the balance is in equilibrium when the two riders are suspended at their respective zero-points. The gravity-bob arrangement differs from the customary one in this, that the bob (which is very small) is fixed by *mere friction* to a triangular rod forming part of the needle, so that, by a mere touch with the finger or forceps, the bob can be made to pass from the top end to the bottom end of the rod, matters being arranged so that when the bob passes from the latter to the former position the sensibility increases in an exactly pre-determined ratio—for instance, in the ratio of 1:10. The author does not consider it necessary to explain the way in which the balance is meant to be worked; he prefers, by way of appendix, to direct attention to the following inferences from the theory of the balance, which, obvious as they are, he thinks have hitherto not been sufficiently appreciated by either the authors of our physical handbooks or by practical balance makers.

Given a balance in which everything is constant except the distance s of the centre of gravity of the beam from the axis of rotation, and it is easily shown that, supposing s to vary, the tangent value a of the deviation corresponding to a given overweight Δ ; or, with other words, the sensibility $\frac{a}{\Delta}$ is inversely proportional to s . This, of course, is duly stated in all handbooks. But what is always forgotten to be added are two things, viz., (1) that this sensibility has nothing to do with the *inherent precision* of the instrument (which, in fact, is a function of only the practically unavoidable inconstancy λ in the arm-length); and (2) that supposing the sensibility to be increased (by diminishing s) all the other good qualities of the balance get less. We diminish the rate of vibration, this rate being, as is easily shown,—

$$\propto \text{to } \frac{1}{\sqrt{\frac{a}{\Delta}}};$$

we diminish the range of overweight determinable by the method of vibrations; we diminish the relative constancy, in opposition to variations in the charge, of the sensibility, and of the time of vibration. If, now, physicists or opticians would only present us with some simple arrangement which, without any visual effort on the part of the operator, would apparently magnify the linear magnitudes of the excursions of a balance needle to, say, ten times their real value, the problem of constructing a quick-working precision balance would then be solved quite satisfactorily.

The author is indebted to Messrs. Becker and Sons for having realised his ideas in an actual instrument, which, he states, is now being exhibited at South Kensington. To increase the usefulness of the balance, the author has caused Messrs. Becker to add to it a glass plunger displacing exactly 10 grms. of water of 15° C., and consequently enabling one to determine specific gravities of liquids with great rapidity by the method of immersion.

In the new *Gas Governor* the most essential part is a mercury manometer, of which the one limb—which communicates with the gas-pipe on the one hand, and the gas-lamp on the other—is about 20 m.m. wide, and stands vertical, while the other is of the width of a thermometer-tube, and lies horizontal. The bulk of the mercury is adjusted so that—supposing the gas pressure in the supply pipe to be at its minimum—the mercurial index in the horizontal limb stands at some definite convenient point, a . With the manometer there is connected an electro-magnetic arrangement in such a manner that, whenever the

index travels ever so little to the wrong side of a , the current is closed, and, by converting a certain iron lever into a magnet, shuts off the gas. Of course there is a spring in connection with the gas-tap, which opens the tap as soon as the mercury has gone back to its original position.

GLASGOW PHILOSOPHICAL SOCIETY: (CHEMICAL SECTION).

March 27, 1876.

Professor JOHN FERGUSON, M.A., President, in the Chair.

DR W. WALLACE, F.R.S.E., F.C.S., read a paper on "*Germination, particularly with reference to the Preparation of Malt.*" He said that, in conjunction with Dr. Stevenson Macadam, of Edinburgh, he had occasion some months ago to make a rather extensive series of experiments on the germination of barley and on the properties of malt; and the object of his paper was to state briefly some of the results arrived at. The object of the researches was to ascertain whether or not grain which had once germinated and been kiln-dried, as in the manner of malt, would, under favourable circumstances, germinate again, and also whether barley might resist the process of malting in the first instance, and afterwards germinate. After making a few general observations on germination, he went on to say that in the preparation of malt there were four stages:—1, the steeping; 2, the couching; 3, the flooring; and 4, the drying. The steeping, he said, was performed in large stone or wooden cisterns, the barley being completely covered with water. In course of time it swells up, and increases about one-fifth in bulk and one-third in weight. The process occupies two days, the water being then drawn off, and the grain couched or laid out in square heaps, so that it may be accurately measured by the excise officer. After the lapse of twenty-four hours it is laid upon the malt-house floor, when there is developed a considerable rise of temperature; growth goes on vigorously, the little radicles or rootlets appearing at one end of the grain, while the plumule or acrospire extends in the opposite direction under the integumentary coating of the seeds. To prevent too rapid growth, the grain is spread to the depth of only a few inches on the floor, and frequently turned over. The greatest rise of temperature during any part of the process is about 10° F., but by turning over and spreading out it is kept down, so that it is seldom that the temperature is over 60° F. The time on the floor is usually about seven days, but sometimes during cold weather a longer time is necessary, in order to develop the germinating process to a sufficient extent. The grain is then removed to the kiln, on which it is gradually dried, the temperature on the first day reading about 110° F.; and the drying is finished in three days, the temperature finally becoming about 140°.

During the germination sugar is produced by the alteration of and absorption of the elements of water by the starch, and the extent to which the process has been carried may be roughly estimated by the amount of solid extract dissolved out of the crushed malt by the action of alcohol. In three samples of raw barley tested in this way the author obtained 2.74, 2.72, and 2.74 per cent of dissolved matter; and in seven samples of malt the quantities of extract so dissolved out were, respectively, 7.42, 9.28, 8.00, 8.00, 7.82, 7.80, and 8.88 per cent, the average being about 8.27 per cent. In a sample of "lie-backs"—that is, grain which had passed through the malting operation without germinating—the solid extract weighed 4.24 per cent.

In the malting of barley much depends upon the condition of the grain. In the beginning of winter, especially if the barley is not well desiccated, a large proportion of the grains do not germinate, and the number of the "lie-backs" is great—it may be 25, 30, or even, in extreme cases, 50 per cent. The same grain if kiln-dried before

malting gives a much better result, the "liebacks" being much reduced in number. Now, said the author, it is evident that the "liebacks" in the first instance could not have been dead grain, for if so no amount of kiln-drying, or any other treatment, could have restored their vitality. If the grain is well dried as the season progresses its germinating power gradually improves, but if not well kept there will inevitably be a large proportion of "liebacks."

"Liebacks" consist of two distinct kinds of grains,—those which are dead, and which, like addled eggs, no power can restore to life; and those which are merely backward in germinating, and which in malting, especially when insufficient time is given, show no radicle or acrospire. These have their germinating power increased by the kiln-drying following the malting process, and afterwards germinate pretty freely. Malt germinates a second time,—at least a certain proportion of the grains sprout again, but they differ from "liebacks" in producing, when tried in the ordinary way by being placed between folds of moistened blotting-paper, only a plumule, and no radicle. When planted in the earth, however, the effect is different. The grains of malt not only germinate a second time, but also produce plants, some of which the author has grown to the extent of $5\frac{1}{4}$ inches in 21 days, while raw barley grew about 8 inches in the same length of time. But only a limited number of the malt grains grew,—in some cases only 4 per cent, in others as many as 38 per cent. The author offered the following explanation of these facts:—In their fully developed condition the plants of barley have seven rootlets; that is the nominal number, but they are not all developed at once. He took twelve plants of barley, all grown to about 8 inches in height, and he found that eight had the normal number of rootlets, namely, 7; one had 6, and three had only 5. If these last four plants had had a little more time given them they would all have developed the normal number of rootlets. If, again, ordinary malt be taken in its various stages of progress, it will be found that in almost all cases the rootlets develop gradually, so that there are in some cases 2, in others 3 or 4 radicles, and so on up to the complete number 7, the others being rudimentary or as yet altogether undeveloped. In those cases where the full number of rootlets have grown, and the grain has afterwards been kiln-dried, no amount of coaxing can induce it to re-germinate. The plumule may swell out and extend a little, but in a day or two it dies down and begins to mould. But in other cases it is entirely different; the rootlets that were not developed during the malting now appear,—somewhat tardily it is true, but they do come eventually,—and the result is that a plant is formed; not so large, certainly, as one grown from raw barley, but still a good healthy plant. These plants, however, are not perfect; they resemble a man who has been accidentally deprived of one of his limbs; they never have, and as a matter of fact they never do have, their full complement of rootlets. The author examined ten plants grown from malt, and found that in three weeks—

2	had	1	rootlet
3	"	2	rootlets
3	"	3	"
1	"	4	"
1	"	0	"

The one which had no rootlets was very feeble, but still it was not moulding like those that were not growing at all; and if the experiment had been continued longer the plant would probably have thrown out a single rootlet.

Then as to "liebacks" which in the malting had not germinated at all, the author found them to be of two kinds: a large proportion were dead, and those which did grow formed perfect plants with a full number of rootlets; and this, he said, makes it difficult to distinguish raw grain mixed with malt from raw grain.

Hitherto it has been assumed (1) that "liebacks" are dead grains, that have not sprouted simply because they are dead, which the author has already shown to be absurd

and contrary to fact, for, if dead, kiln-drying could not restore their germinating power; and (2) that malt which has sprouted well and been properly developed, and which has afterwards been dried in the kiln, may throw up a plumule of an inch in length or so, but will then die down, and under no circumstances produce rootlets and form a healthy plant. This, the author says, he has shown most conclusively to be incorrect.

The results obtained with various samples of malt were very different, and the author has been led to believe that malt will not germinate at all unless the experiment is made within a year or so. He tried some malt which was only two years old, and could not get any of it to grow; and even some samples which were comparatively fresh gave no plants, while others germinated freely, giving in one instance 38 per cent of plants. Probably a good deal depends upon the heat of the kiln, which if too high would undoubtedly kill all life, either of raw grain or malt. But the age of the malt has also something to do with it, for malt which will sprout freely when comparatively new will refuse to grow if kept for a couple of years. On the other hand, raw barley, if kept dry, appears to preserve its vitality for almost any length of time. Malt, in consequence of the changes in the original constituents of the grain, will probably attract moisture more freely than barley, and so become spoiled in an atmosphere that would not be injurious to raw grain.

Although the immediate object of his enquiry had already been attained, and his experiments all ruthlessly stopped, Dr. Wallace expressed a hope that he would, during the coming summer, be able to sow a larger quantity of malt, and prove still more conclusively, if it were necessary, that malt, although "scotched," is not killed; and, in conclusion, he said that he had found that the malt which contained the fewest "liebacks"—in other words, the most perfect malt—gave the largest number of healthy plants. That sample which gave 38 per cent of plants contained only 2 per cent of "liebacks."

A discussion followed, in which Messrs. Arrol (a practical brewer), John Jex Long, Tatlock, Coleman, Wood, Mayer, and the President took part. The remarks made by the various speakers had reference chiefly to the influence exerted by air, and by sunlight, and light passing through windows of yellow or blue glass in the malt-house, upon the process of germination. One of the speakers stated that he had seen the windows of a malt-house actually glazed with blue glass.

CORRESPONDENCE.

GENERAL INDEX.

To the Editor of the Chemical News.

SIR,—I am sure that a General Index to the whole series (or say to even thirty volumes) of the CHEMICAL NEWS would be of immense utility, and would be appreciated by its readers. The main question is, I suppose, *how much* it would be appreciated, if at the cost of production. It is, I think, difficult to overvalue such a work. It often happens to me to have to search nearly the whole series to find a desired note, and I for one should think relief from such trouble and loss of time cheaply purchased at the cost of, say, an average volume.—I am, &c.,

SYDNEY GIBBONS.

Melbourne, January, 1876.

CAUSTIC SODA.

To the Editor of the Chemical News.

SIR,—In the CHEMICAL NEWS (vol. xxxiii., p. 137) Dr. Ph. Pauli writes to say that Mr. McBryde, of the Union Alkali Works, St. Helens, was the "original inventor" of the "process of making white caustic soda." Dr. Pauli relates that in July, 1860, he was appointed chemist to

Messrs. Evans and McBryde, and that he was informed by Mr. McBryde that "the year before" (1859 I presume) "he had heated caustic soda to redness, and oxide of iron separated out," &c.; and at the meeting of the British Association at Manchester, in 1862, Dr. Pauli exhibited a large sample of this white caustic soda, and it was then generally acknowledged to be a perfectly new commercial product.

I beg this opportunity of supplementing these statements and dates. We commenced the manufacture of caustic soda at these works, under Mr. W. Jowage's patent of the 18th May, 1853, on the 7th August, 1857. On the 17th September, 1857, we made our first batch of white caustic, just as Mr. McBryde did in 1859,—i.e., by heating to redness and separating the iron, &c. Of course we went the usual round of blue, green, &c., and, amongst other common varieties, a speciality of hydrated crystals. The British Association usually meets about August: 1862 was also the year of the International Exhibition in London, which opened in May. My firm exhibited white caustic soda at that Exhibition, say four months earlier than Dr. Pauli's exhibition of it in Manchester. I do not know if we are the "original inventors" of this process; in fact the separating of iron, when caustic soda is made red-hot for the first time, is not the only phenomenon that occurs, as Dr. Pauli himself has pointed out that graphite is often separated at the same time; but it is precisely one of those things that manufacturers as a rule are more ready to utilise than to talk about. And, besides that, the separation of iron, &c., under the circumstances, is so necessary a consequence, and the heating red-hot almost so inevitable a consequence of making caustic soda on the large scale, that I quite expect to hear that some of the caustic soda makers who had begun before us—such as Messrs. J. C. Gamble and Son, of St. Helens, or others—have a better right to the title of original inventors of this process than ourselves. In fact I question very much if it be such a process as to admit of having been "invented" at all.

In connection with this history, the following extract from the specification of a patent granted to Mr. George Brown, a partner in the firm of Messrs Charles Tennant and Co., of St. Rollox, will interest some of your readers. The patent is dated February 20, 1845. "Red liquor . . . is brought into a metal pan and evaporated to about the consistency of tar: the proper quantity of nitrate of soda is then added, and heat applied to expel the remaining water and flux the mass," &c.

Whilst I have my pen in hand, as something has recently been said about Mr. Lee being the first to introduce the present form of salt-cake decomposing-pots, may I ask if the following is not about the outline of the dates and facts? March 14, 1839: a patent by Mr. J. C. Gamble for some arrangement of iron retorts, &c., which were flat and containing false bottoms, all of cast-iron. An action-at-law between Mr. Gamble and Mr. Lee, and the purchase by Mr. Lee of a patent granted to Mr. Lutwyche on October 13th, 1836, and defeat of Mr. Gamble's suit, and filing of a disclaimer by him in consequence. Mr. Lee's adoption of "pots" like the bowl of a spoon, the broad end towards the workman and the narrow end for pushing up into the calcining furnace. A second suit-at-law between Mr. Gamble and Mr. Lee, the former being a second time defeated, and the adoption by Mr. Gamble of round "pots," smaller and deeper than now common, covered, and contained in an outer "pot," but much nearer the present shallow, uncovered, and unprotected "pot" than Mr. Lee's spoon-shape. Whether Mr. Lee, or who, was the first to remove the lid, or cover, and the "shell," I do not know; but Mr. Gamble's somewhat rectangular, Mr. Lee's spoon, and Mr. Gamble's round, I believe is the order in which the forms followed each other.—I am, &c.,

HENRY DEACON.

Alkali Works, Widnes, Lancashire,
April 6, 1876.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Seances, de l'Academie des Sciences. No. 12, March 20, 1876.

New Rays of Calcium.—A letter from Mr. Lockyer to M. Dumas relating to the supposed dissociation of calcium: will be given in full.

Actinometric Measurements taken on the Summit of Mont Blanc.—M. J. Violle.—The author tabulates his observations, and announces his intention of giving the results arrived at in a future communication.

Approaching Maturity of the Winter Eggs of the Phylloxera.—M. Balbiani.

Influence of Temperature upon Magnetisation.—M. J. M. Gaugain.—The author finds that when a bar of steel placed in contact with a magnet is heated gradually till it takes a blue tint, the magnetisation increases at first, attains a maximum, and then retrogrades. When the bar, after having been strongly heated, remains in contact with the magnet during all the time of its cooling, the total magnetisation increases as the bar cools, and when the latter has resumed the temperature of the surrounding medium it proves a value much higher than what it had during the heating of the bar.

Rock Intervening between the Gneiss Rocks of Mantiqueiro (Brazil).—M. H. Gorceix.—This rock approximates to epidote, and appears to consist of a single mineral, with the exception of some small granules of infusible quartz. Its specific gravity is 3.4; it fuses easily, leaving a black slag, the specific gravity of which is only 2.86; its hardness is between 6 and 7. The composition of this rock is—

Silica	38.5
Alumina	25.1
Lime	23.2
Protoxide of iron	10.4
Magnesia	traces
Loss on ignition	2.6
						99.8

Gazzetta Chimica Italiana.

Anno vi., 1876, Fasc. i.

Use of Phyllocyanin as a Reagent.—Guido Pellagri.—Phyllocyanin, the blue colouring matter of flowers, is a reagent which may render some service in chemistry on account of its exquisite sensibility to the least traces of alkali. It is best preserved along with such a slight trace of acid as may give it a dull purple colour. Phyllocyanin will prove a valuable substitute for litmus in volumetrical operations, as there can be no doubt as to the end of the reaction. The following table shows its sensitiveness as compared with litmus:—

Potash.	Phyllocyanin.	Litmus.
400	Transient green, then yellow.	Distinct reaction.
1000	Green rather stable, and then yellow.	" "
10000	Stable green.	" "
100000	" "	" "
200000	" "	Reaction scarcely manifest.
400000	At first green, then stable blue.	" "
500000	Stable blue.	No reaction.
1200000	" "	" "

Phyllocyanin is most readily obtained from the iris and the blue violet.

Revue Universelle des Mines, de la Metallurgie, de Travaux Publics, des Sciences et des Arts Appliqués à l'Industrie, January and February, 1876.

Procedure for Detecting Arsenic in Paper-Hangings and in other Combustible Matters.—M. Kupferschläger.—Schweinfurt-green, known also as mitis-green and mountain-green, is a compound of the arsenite and acetate of copper too frequently used, in spite both of advice and of legal prohibition, for colouring paper-hangings, ball-dresses, artificial flowers, wafers, bon-bons, and toys. Every winter we hear of needlewomen experiencing serious affections after having made up costumes of tarlatan coloured or printed with Schweinfurt-green; that dancers wearing similar materials, and with wreaths of artificial flowers in their hair, experience violent headache on the morning after the ball; that similar symptoms have been experienced by persons who have slept for some nights in rooms papered or painted with green; and, lastly, that children have been seized with vomiting after having eaten bon-bons or sucked toys coloured green. The author's method of detection is as follows:—The stained paper, divided into strips is placed in a porcelain soup-plate, and covered with a solution of chlorate of potash saturated when hot, and the whole is heated in the water-bath till the paper is completely dry. Then it is set on fire, and instantly covered with a large glass bell so that nothing may be lost. The ash is pulverised, and immediately exhausted in the cold with the water which has served to rinse out the bell and the plate after the combustion. Thus all the arsenic combined with the potash is dissolved, and not the oxides of chrome, copper, aluminium, and of tin and lead if present. This colourless solution, filtered, and mixed with sulphuric acid till a slight acid reaction is distinguished, is then introduced into a Marsh's apparatus which has been submitted to a blank test, and found free from arsenic.

MEETINGS FOR THE WEEK.

TUESDAY, 18th.—Civil Engineers, 8.

WEDNESDAY, 19th.—Society of Arts, 8.
Meteorological, 7.

THURSDAY, 20th Chemical, 8. Adjourned Discussion on Dr. Armstrong's Paper on "Systematic Nomenclature." "On the Manufacture of Sulphuric Anhydride, Experimentally Illustrated," by Dr. Messel and Mr. Squire. "On Glycerine-Phosphoric Acid, and on Fermentation," by Dr. Thudichum and Mr. Kingzett. "Note on the Occurrence of Benzene in Rosin Light Oils," by Watson Smith. "On the Action of Water and Various Saline Solutions on Copper," by Thomas Carnelly.

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THE CHEMICAL NEWS.

VOL. XXXII. No. 856.

ON THE MOVEMENT OF THE GLASS CASE OF A RADIOMETER.*

By WILLIAM CROOKES, F.R.S., &c.

DURING the discussion which followed the reading of Prof. Reynolds's and Dr. Schuster's papers, at the last meeting of the Royal Society, I mentioned an experiment which confirmed the observations of Dr. Schuster. I have since tried this in a modified form; and as the results are very decided, and appear calculated to throw light on many disputed points in the theory of these obscure actions, I venture to bring a description of the experiment, and to show the apparatus at work, before the Society.

I made use of a radiometer described in a paper communicated to the Society in January last. I quote the description from paragraph 184:—"A large radiometer in a 4-inch bulb was made with ten arms, eight of them being of brass and the other two being a long watch-spring magnet. The disks were of pith, blackened on one side. The power of the earth on the magnet is too great to allow the arms to be set in rotation unless a candle is brought near, but once started it will continue to revolve with the light some distance off."

This radiometer was floated in a vessel of water, and four candles were placed round it so as to set the arms in rotation. A mark was put on the glass envelope so as to enable a slight movement of rotation to be seen. The envelope turned very slowly a few degrees in one direction, then stopped and turned a few degrees the opposite way; finally it took up a uniform but excessively slow movement in the direction of the arms, but so slow that more than an hour would be occupied in one revolution.

A powerful magnet was now brought near the moving arms. They immediately stopped, and at the same time the glass envelope commenced to revolve in the opposite direction to that in which the arms had been revolving. The movement kept up as long as the candles were burning, and the speed was one revolution in two minutes.

The magnet was removed; the arms obeyed the force of radiation from the candles, and revolved rapidly, whilst the glass envelope quickly came to rest and then rotated very slowly the same way as the arms went.

The candles were blown out; and as soon as the whole instrument had come to rest a bar-magnet was moved alternately from one side to the other of the radiometer, so as to cause the vanes to rotate as if they had been under the influence of a candle. The glass envelope moved with some rapidity (about one revolution in three minutes) in the direction the arms were moving. On reversing the direction of movement of the arms the glass envelope changed direction also.

These experiments show that the internal friction, either of the steel point on the glass socket, of the vanes against the residual air, or of both these causes combined, is considerable. Moving the vanes round by the exterior magnet carries the whole envelope round in opposition to the friction of the water against the glass.

As there is much discussion at present respecting the cause of these movements, and as some misunderstanding seems to prevail as to my own views on the theory of the repulsion resulting from radiation, I wish to take this opportunity of removing the impression that I hold opinions which are in antagonism to some strongly urged explanations of these actions. In the arguments which Prof. Reynolds has credited me with, in the paper which was read last Thursday, I fail to recognise my own ideas. I

* A Paper read before the Royal Society, March 30, 1876.

have on five or six occasions specially stated that I wish to keep free from theories. During my four years' work on this subject I have accumulated a large fund of experimental observations, and these often enable me to see difficulties which could not be expected to occur to an investigator who has had but a limited experience with the working of one or two instruments.

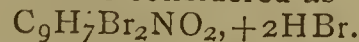
STATE OF ANIMAL CHEMISTRY IN AUSTRIA.

(OPEN LETTER TO THE IMPERIAL ACADEMY OF SCIENCES AT VIENNA, CONTAINING AN EXAMINATION OF THE RESEARCHES ON THE COLOURING MATTER OF BILE, BY RICHARD MALY, OF GRAZ.)

By J. L. W. THUDICHUM, M.D., London.

(Concluded from page 156).

18. 0.5570 gm. dry bilirubin was treated as described in the above experiment under 15. At no period of the experiment did the substance assume a green colour: it became, however, tolerably warm. After the absorption of bromine vapour had completely ceased, the product was left for twelve hours longer in the bromine atmosphere, and then freed from all bromine vapour by a current of dry air. The dark purple substance now weighed 1.1646 grms., corresponding to an addition of a little more than four atoms of bromine. The excess amounts to 0.0712 gm., and is evolved subsequently on heating. The compound must therefore be considered as—



The combination with HBr is, however, very loose, so that, similar to many hydrates, it is gradually decomposed by passing a current of air over it for a very long time, or quickly by heating it to 100°, all hydrobromic acid being at the same time expelled. At the same time the substance contracts to a smaller volume, but fuses in no manner or measure, and remains perfectly powdery. After many hours drying in the current of dry air at 100°, the increase in weight amounted to 0.5312 gm. Consequently 2 atoms of bromine had entered the bilirubin with expulsion of 2 atoms of hydrogen, and the atomic weight of bilirubin is 165—again very near to the figure 163 already mentioned.

19. The elementary analysis of this product yielded—

a. 0.1066 gm. gave 0.1210 silver bromide = 48.31 per cent Br.

b. 0.385 gm., burned with lead chromate, gave 0.4750 CO₂ = 33.64 per cent C, and 0.1150 H₂O = 3.31 per cent H.

c. 0.315 gm., burned with copper oxide, gave 13.1 c.c. nitrogen gas normal = 5.16 per cent N.

The preparation marked I. in the table below had been made by myself; the analyses were executed by my assistant, Mr. C. T. Kingzett. Several bromine determinations executed by my assistant, Dr. H. Hake, completely confirmed the theory to be seen below.

20. Further, Dr. Hake has executed the bromination of a third preparation (tabulated below as II.), under my superintendence, with all the precautions described, and has obtained results which fully confirm the foregoing.

a. 0.4502 substance gave 0.5277 silver bromide = 49.87 per cent Br.

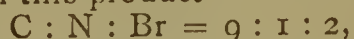
b. 0.2314 gave 9 c.c. N normal = 4.86 per cent N.

c. 0.3043 gave 0.3760 CO₂ = 33.69 per cent C, and 0.0978 H₂O = 3.56 per cent H.

These results compare with the theory of C₁₇H₁₇BrNO₂ as follows:—

		Theory.	Found.	
			I.	II.
C	33.64	33.64	33.96
H	2.18	3.31	3.56
Br	49.84	48.31	49.87
N	4.36	5.61	4.86
O	9.98	9.58	8.02

It is unnecessary to accumulate figures, and impossible here to discuss the excess of hydrogen, which might possibly require an increase of the figure for hydrogen in the formula. These variations from theory, however important they appear to me, effect no difference in the great features of the fact that in this product—



and that it therefore fully confirms the theory of the red colouring matter of gallstones, which I have maintained since 1865.

21. In his remarks on the formula assumed for his bromine product, Prof. Maly says—"The most simple expression of the composition of bilirubin is, according to the analyses of Prof. Städeler and myself, $C_{16}H_{18}N_2O_3$." In making this statement Prof. Maly loses sight of "the usual duty of characterising previous knowledge," or other knowledge. I therefore felt myself called upon to search for the analyses of Prof. Maly, of which he claims that they prove the above formula, but with the exception of one carbon and two hydrogen determinations, in the *Reports of the Vienna Academy* (1868, vol. lvii., p. 97). I could not find any proofs of this assertion. In particular, Prof. Maly has not made any determinations of the nitrogen, and, above all, no determination of the atomic weight, and has not even subjected his results to control on different preparations. For these reasons I consider his formula as of little significance opposite my theory, which is based upon numerous preparations and well-defined compounds, and now more than fifty elementary analyses.

22. Prof. Maly, in the paper just quoted, supports his formula for bilirubin by the authority of Städeler, but on p. 102, note 2, he censures the proceedings of Städeler, who had produced a formula for biliverdin by re-calculating an old analysis of Heintz, which had been executed upon material declared by Städeler himself to have been a mixture of pigments. Prof. Maly himself points out that this proceeding was characteristic of the work of Städeler. I dare not trouble the Academy with a criticism of the researches of Städeler, particularly as I have given one in my paper contained in the *Journal of the Chemical Society*, 1875, I think it, however, necessary to direct the attention of the Academy to the circumstance that the doubling of the formula $C_{16}H_{18}N_2O_3$ into $C_{32}H_{36}N_4O_6$ —which Prof. Maly introduces in the fifth paper, in 1875, as a necessary consequence of his discovery of a brominated compound, and then further particularly as a novelty—had already been adopted by Städeler, before 1870, in a letter to the editor of "Gmelin's Handbuch," Herr Karl Kraut, and been published by the latter in an Appendix to the last volume of the "Handbuch." In the letter alluded to, Städeler, in view of my researches, abandons all his former formulæ, and coerces my results by an utterly unjustifiable process of re-calculation, in which no single analytical result harmonises with the new hypothesis into some sort of support for his doubled formula and hexa-basic acid hypothesis, without having produced a single compound or made a single new analysis.

23. Prof. Maly causes to himself many difficulties by his preconceived opinions and uncontrolled imagination, as I am obliged to prove now more in particular. At first he believed every green product of the colouring matter of gallstones to be "biliverdin:" thus the product by chloroform and glacial acetic acid, which seduced him into the belief that he had transformed bilirubin, an alleged amide, into ammonia, on the one hand, and an acid free from nitrogen, viz., biliverdin, on the other; and, what was still more surprising, that he had re-transformed this acid into the original amide, bilirubin, by simply mixing it with ammonia (see "Preliminary Communication on the Colouring Matter of Bile"—*Reports of the Meetings of the Vienna Academy*, vol. xlix.). A part of this conclusion Prof. Maly has withdrawn (*Reports of Vienna Acad.*, 1868, p. 98), namely, the one concerning the decomposition of bilirubin into ammonia and "biliverdin." The ammonia which he had found he now explained to have been an

impurity of his preparation. But in 1868 he still insisted upon the proposition that the product of heating bilirubin, glacial acetic acid, and chloroform, in sealed glass tubes, was "biliverdin." To this opinion he was probably led by the green colour only; at least he advanced no analytical proof in its support, and it will probably be long ere this will be forthcoming. I have explained the circumstances attending the experiment of Prof. Maly in my paper, and therefore refer to it.

24. For a long time he also believed his brominated product to be "biliverdin" (*Vienna Acad. Reports*, 1868, p. 104). I have shown that in its first form it was a mixture of blue brominated bilirubin with orange bilirubin: this Prof. Maly now admits himself, in his fifth paper, as his original discovery.

25. In the fifth paper Prof. Maly again produces "biliverdin," by treating the brominated product with alkalis. But it contains always yet a small trace of bromine, and a little ash, which is deducted. In the analyses 3 and 4 of his former "biliverdin" (*Vienna Acad. Reports*, vol. lvii., p. 105) he was even obliged to deduct as much as "Circa 2 Pet." of ash. How can an author who works with such preparations call others to account for the alleged impurity of their preparations!

26. In short, Prof. Maly is unacquainted with the fact that there is a great number of derivatives of the colouring matter of gallstones, which all have this in common, that they are green, but are not for that reason alone biliverdin ($C_8H_9NO_2$). Thus the green cholothallin obtained by the action of oil of vitriol upon bilirubin, and subsequent treatment with water, is bilirubin to which the elements of water have been added. By the action of bromine, and particularly hydrobromic acid, at least three green products are produced, which all contain bromine, viz., hydrobromo-bilirubide, C_9H_8BrNO , containing 35.39 per cent Br; further, a compound of this body with bilirubin, $C_9H_8BrNO + C_9H_9NO_2$, requiring 20.56 per cent Br, actually found in it 19.68 per cent Br; further, a mixture of this body with a further molecule of hydro-bromo-bilirubide, therefore about $C_{18}H_{17}BrN_2O_3 + C_9H_8BrNO$, which contains 27.945 per cent Br, and is probably nothing else than the product described in Maly's fifth paper.

27. The Academy may justly demand of me to prove these statements. I am ready, on receiving a request to that effect, to communicate to the Academy details, the extent of which are excluded from the present letter on account of their length. I will, however, state the principles, found by experiment, upon which my conclusions are based:—

- Bromine, acting upon bilirubin, produces probably at first monobrominated bilirubin, $C_9H_8BrNO_2$: this body is blue; it has neither been obtained in the pure state nor analysed.
- Bilirubin saturated with bromine becomes dibromobilirubin, $C_9H_7Br_2NO_2$, which body is violet.
- Bilirubin heated with hydrobromic acid in chloroform becomes hydro-bromo-bilirubide-bilirubin, $C_9H_8BrNO + C_9H_9NO_2$: this body is green.
- The bodies under *a* and *b*, when treated for some time and at elevated temperatures with alcohol, or ether, or with oil of vitriol, and then water, are changed first to hydrobromo-bilirubide, C_9H_8BrNO ,—therefore a product of reduction,—and afterwards into bodies which, in solution, constantly lose bromine, and ultimately (probably or perhaps) become quite free from bromine.

28. Prof. Maly operated with bromine in moist chloroform. Every molecule of bromine yielded him a molecule of hydrobromic acid, which now in its turn attacked the bilirubin. The moisture precipitated the mixture. The precipitate consisted proximately of bromo-bilirubin, with some hydrobromic acid, of which it yet lost some on drying, and of perhaps little hydrobromo-bilirubide, for the action of HBr requires more time than that of Br. But now the product was put in alcohol, which immediately began its reducing action. Consequently the powder was

"dark blue-green," while the probable monobromobilirubin is monochromatic blue, as I state in my essay (and not "nearly" monochromatic blue, as Prof. Maly reports my description).

29. Again, the ethereal solution which Prof. Maly obtained when treating bilirubin in ether with bromine was "dark greenish blue," and became blue only when the monobromo-bilirubin prevailed, by means of its greater absorptive power for green. The residue of this ether process was "always in thin layers, green." This change from blue to green was evidently the reason which caused Prof. Maly to abstain from further attempts at purifying his products. The incessant loss of bromine which the products of substitution, as well as of reduction, experience in alcohol or ether, is so great and marked that in some of my experiments the amount of bromine fell from 49.8 to 16 per cent, and the violet product, which had been soluble in ether with a violet colour, became green, quite insoluble in ether, but soluble in alcohol with a green colour.

30. After the foregoing I hardly know whether there is any part of Prof. Maly's research left which requires refutation or explanation, excepting perhaps the remarkable results of his carbon determinations in which the carbon found varied between 35.51 and 47.83 per cent. However, this is by no means incumbent upon me, but upon Prof. Maly. The assumption of "a very volatile bromide of carbon" cannot satisfy the demand for explanation,—much less can the proposition that an accurate determination of the quantity of carbon was not essential to the ascertaining of the composition of the bromine product. On the contrary, it must be maintained that such results and corollaries are directly opposed to the principles of chemical science, and slap the endeavour for final accuracy rudely upon the face.

31. We have seen above how Prof. Maly believed every green product of the metamorphosis of the colouring matter of gallstones to be "biliverdin." In the same manner he assumed every blue product to be a result of oxidation, and identical with the blue product which forms transiently during the reaction of bilirubin with nitric containing nitrous acid. But the blue product which is obtained with bromine is now transmuted into a product of substitution, and there is no reason to believe that the blue product obtained by means of nitrous acid is a product of oxidation. Indeed it is impossible to predict what it is. Pure nitrous acid forms no blue product with bilirubin. It seems that the presence of alcohol is necessary for the production of a blue body. According to my experiments, nitrous acid, when brought in contact with bilirubin, produces a violent reaction, which, when the apparatus is not cooled down, causes the bilirubin to take fire and burn like glowing charcoal. When the apparatus is carefully cooled down the reaction furnishes a new product, which has the same weight as the bilirubin employed. From this it follows, first, that the formula which Prof. Maly has given for his "choloteline" is improbable, and that the diminution of the carbon in this reaction which Prof. Maly observed—but left out of consideration or explained away—is much more probable than the alleged oxidation without any loss of carbon, particularly as the nitrogen in the product is the same quantity as that which was present in the bilirubin employed. The three silver compounds of choloteline, in which the silver rose in quantity from the first to the third, of which, however, only the second one furnished an acceptable theory (*Vienna Acad. Rep.*, vol. lix., p. 605), are analogous to the zinc compounds of hydro-bilirubin, in which the zinc rose from 14.6 to above 37 per cent (*Liebig's Ann.*, clxiii., 86). Such proceedings must necessarily lead to end and limit of even the plausible in animal chemistry.

32. The observation of the influence of sodium amalgam upon bilirubin, which led Prof. Maly to the discovery of the so-called hydro-bilirubin, would have been an interesting progress in our knowledge concerning bilirubin. But as the author starts from erroneous views regarding

the composition and molecular weight of bilirubin, his conclusions regarding his product and its composition, and regarding the formula of the change, are necessarily erroneous. In my letter to Prof. Maly, previously quoted, I wrote to him on this experiment:—"I have repeated successfully your experiment on the reduction of bilirubin, and find that the reaction is completed in half an hour. However, I have also treated bilirubin during some days with sodium-amalgam, just as you prescribe, and the result did not differ from that obtained after half an hour. The product seems to me to be hydrogenised biliverdin, $C_8H_{10}NO_2$."

33. The announcement at the head of this article on hydro-bilirubin, of the transformation of bilirubin into the colouring matter of urine, caused me to read it with expectations which were speedily disappointed when the absolute "urine-colouring matter" became limited to Jaffé's urobilin. I thereupon made many experiments, which I have described in my paper (in the *Fourn. Chem. Soc.*, May, 1875), and which, without exception, negative the alleged metamorphosis. On this point I wrote to Prof. Maly:—"I have now compared the products (of the reduction of bilirubin) with all urinary colouring matters with which I am acquainted, and with Jaffé's product, which I have produced for the purpose, but have not discovered any identity. Urochrome and uroerythrine are quite different as regards solubility and chemical properties; uroxanthine, which it is now the fashion to call 'indican,' also. The products of the cleavage of urochrome—namely, uromelanine, uropittine, and omicholine—also. These products can be easily obtained from urochrome by acids; but your hydro-bilirubin yields nothing of the kind, and is not much changed by boiling with hydrochloric acid. Jaffé's urobiline has never been isolated, never been analysed. According to my comparison the mass is a mixture of urochrome, uroerythrine, with a little omicholine already separated. The process (of Jaffé) is not inviting, and the result, in any case, a complicated tincture. I am therefore unable to adopt your conception of the metamorphosis of the colouring matter of bile, into the or a colouring matter of the urine, in even a single particular." The statement of my experimental proof is contained in the above-quoted paper, of which I subjoin a copy for reference at the Academy's pleasure.

34. In his fourth paper (*Vienna Acad. Rep.*, vol. lxx. 1874) on biliverdin, Prof. Maly relates how he, "already earlier and independently of that Thudichum," had arrived at the formula $C_{16}H_{18}N_2O_4$. However, I had only arrived at half that formula, namely, $C_8H_9NO_2$; but this difference is unimportant. It is quite true that I arrived at this formula independently of Prof. Maly, but I have also a priority with reference to it, which does not appear quite clearly in Prof. Maly's representation. For my research on biliverdin was communicated to the Royal Society already on November 14th, 1867, and published in abstract in the *Proceedings* of that Society, vol. xvi., p. 217. The relative research of Prof. Maly, on the other hand, was communicated to the Academy of Vienna only on February 6th, 1868.

35. In the paper No. IV. just alluded to, concerning biliverdin, Prof. Maly communicates two further elementary analyses of biliverdin, and, as now his results exactly correspond with mine from the year 1867, he is satisfied that the composition of this body can now be considered as definitely established. This time the biliverdin did not contain any ash, and was made according to the orthodox method (of Heintz), with soda and air. There still figures the biliverdin made by means of chloroform and glacial acetic acid, but it cannot easily be precipitated by water. A new green matter, obtained by mono-chloroacetic acid from bilirubin, also makes its first appearance. The metamorphosis each time yields less "biliverdin" than the weight of the employed bilirubin, but was obliged in each case to support the theory without analysis.

36. But now comes the buttress of Prof. Maly's theory, according to which biliverdin is bilirubin plus oxygen only,

$C_{16}H_{18}N_2O_3 + O = C_{16}H_{18}N_2O_4$. Bilirubin is transformed into biliverdin, according to the orthodox method, with soda and air, and the product weighed; 0.4558 grm. bilirubin yields actually 0.4458 grm. biliverdin, both dried at 110° . A loss was therefore sustained. There was no guarantee that the biliverdin did not yet contain bilirubin. But the hypothesis demanded a greater weight of biliverdin than was that of the bilirubin employed. In consequence the filtrates were now evaporated, and what they lost by ignition was scored as biliverdin. Even the washing water is put into requisition, and compelled by means of a process termed "ocular measurement" (*augenmessung*) to supply more than half of the desired increase. Thus by hook and crook Prof. Maly succeeds to calculate an increase of 4.3 parts upon 100 of bilirubin, whereas his hypothesis demands an increase of 5.6 parts. "This," says Prof. Maly, "agrees as accurately as can be demanded under such circumstances."

37. As often as the experiments agree very badly with the hypotheses of Prof. Maly, and prove rather the contrary than the hypothesis, he consoles himself with the expression that the result was as good as could be demanded under the circumstances. Thus again, in his first synthetical bromination experiment, paper V., he finds an addition of only 2.74 atoms of bromine to his new bilirubin, $C_{32}H_{36}N_4O_6$, but observes—"This accorded so far with the increase by 3 atoms of bromine which were demanded by calculation and supported by experiment, as could be demanded in determinations of this kind."

38. But the very next synthesis yields 3.1 atoms of Br; the experiment could therefore be made in a more accurate manner than could previously be demanded. But this experiment also has no value at all. Bilirubin, treated with bromine in excess, yields, as I have proved, always dibromo-bilirubin, $C_9H_7Br_2NO_2$, in which $N:Br = 1:2$. In Maly's supposed molecule $N:Br = 4:3$. Consequently if my product were quadrupled, in order to let it contain 4N, it would contain 8Br,—say 8 atoms of bromine. In any case, therefore, Maly would only have introduced 3 atoms of bromine out of 8 which can be introduced. But this high formula is so entirely negated by my compounds of bilirubin with silver, calcium, barium, lead, zinc, and others, that it cannot come into consideration. The formulæ of bilirubin and of its compounds, as ascertained by me, are—

Crystallised bilirubin .. $C_9H_9NO_2$
Neutral silver salt .. $C_9H_8AgNO_2 + H_2O$
(analogous to silver hippurate, which, according to Liebig, contains a molecule of water).

Basic silver salt .. $C_9H_7Ag_2NO_2$
Neutral barium salt .. $C_{18}H_{16}BaN_2O_4 + 2H_2O$

Half-acid ditto .. $\{ C_{18}H_{16}BaN_2O_4 + C_9H_9NO_2 + 2H_2O$

Neutral calcium salt .. $C_{18}H_{16}CaN_2O_4 + 2H_2O$

Half-acid ditto .. $\{ C_{18}H_{16}CaN_2O_4 + C_9H_9NO_2 + 2H_2O$

Half-acid zinc salt .. $\{ C_{18}H_{16}ZnN_2O_4 + C_9H_9NO_2 + 2H_2O$

Basic lead salt .. $C_9H_7PbNO_2$.

To this series the bodies described above as obtained by means of bromine, hydrobromic and sulphuric acid, are attaching themselves quite naturally—

Dibromo-bilirubin .. $C_9H_7Br_2NO_2$

Hydrobromo-bilirubide .. C_9H_8BrNO

Hydrobromo-bilirubide-
bilirubin .. $\{ C_9H_8BrNO + C_9H_9NO_2$

Cholothalline .. $C_9H_{11}NO_3$.

39. It is impossible here to point out all the irrelevant and erroneous detail with which Prof. Maly surrounds his faulty observations. Only some points I must allude to in proof of this statement. Thus the brominated product is said to not admit of drying at a temperature above 30° to 40° , but had to be dried in an exsiccator, because it lost HBr constantly at a temperature of 100° . But my product is perfectly stable at 100° and above. The lower products

are also quite stable on drying at 100° in an air-current. I therefore cannot doubt but that Maly's product, when it came to be analysed, did yet contain hydrobromic acid. At all events it is to be regretted that he did not even determine the period at which his product ceased to evolve HBr at 100° and became stable. He was as timid when drying his preparation as he was when brominating it, and for this reason he did not attain any definite point of reaction in either process.

40. I conclude my letter to the Imperial Academy with the expression of the deepest regret concerning the circumstances which have compelled me to write it. I should not be able nor dare to molest the Academy a second time with this matter, and I therefore pray the Academy to excuse the length and serious tone of this letter, with the importance which the matter has for me, for science, and for the maintenance of the ethical rules which govern the intercourse of cultivators of science. I hope that the Academy will give to my letter no less publicity than it has given to the papers which have called it forth.

11, Pembroke Gardens, Kensington, W.,
February 24, 1876.

ON THE NEW RAYS OF CALCIUM.

A LETTER FROM MR. LOCKYER TO M. DUMAS.

I AM pretty far advanced in mapping out the solar spectrum on a scale quadruple that of Angström, and I have commenced with the region of which M. Cornu has lately published an admirable chart. My system, as I believe I have already informed you, is to determine the elements of the sun by photographing on the same plate the spectrum of the sun and the spectra of the different chemical elements. I have already done this with a great number of elements, and under varied conditions as regards pressure. One of the most important circumstances is that to which I am about to call your attention.

If we dissociate, for instance, the chloride of calcium to a slight degree, we obtain a ray of calcium—that which is in the blue, and an almost complete spectrum of undissociated chloride of calcium. In proportion as the dissociation of the chloride advances, the ray in the blue, which is the true calcium ray, becomes more brilliant, and the spectrum of the chloride gradually disappears. I admit that the experiment has been made at a low temperature. But if we employ an electric arc we obtain, in the blue, this line exceedingly well developed, and at the same time, in the violet, two new lines, which occupy the place of the two lines H in the spectrum of the sun. The remarkable point is that the ray in the blue is much broader and more brilliant than the two rays in the violet when we employ an electric arc, *e.g.*, produced by 30 Grove's elements; whilst in the sun the blue ray is very feebly represented, and the two violet rays are the broadest in the entire solar spectrum. Between the temperature which we produce and that of the sun there is then a difference which is, as regards the spectrum of calcium, what the varying temperatures which we can obtain are for the chloride of calcium.

To verify this fact I have recently carried out a series of experiments upon calcium, employing at first a small battery and a small coil, and afterwards a large battery and a larger coil, and to avoid all doubts I photographed the results. I found that with the small coil I was able to obtain a photograph which only contained the ray in the blue, without any trace of the ray in the violet; and with the largest battery and the largest coil I obtained a photograph containing the rays of the violet without any trace of the ray in the blue, and that by varying the surface of the battery I was able to get a spectrum resembling the absorption-spectrum of calcium in the sun.

These results agree so completely with those of the dissociation of a salt of calcium that I have asked myself the question if we have not here before us a dissociation of

calcium itself. Naturally we are unable to determine, for the present, whether we have a lower molecular group of calcium, or whether the calcium itself is a compound of two distinct sub-elements, if the expression may be used. It seems that we can only undertake to solve this question by photographing the rays of calcium (H_1H_2) in different stars. If we find that they present always the same relative breadth and intensity there will be a strong presumption that we have met with a decomposition of calcium, or, in other terms, we shall have shown that a spectrum of rays is a spectrum due to different orders of molecular grouping. If, on the other hand, we find that these lines vary in breadth and in intensity, it will be difficult to explain this phenomenon except we admit that calcium, instead of being an element, is really composed of two substances.

I must add that Prof. Stokes—whilst admitting that we have here a very convenient method for determining the power of dissociation existing in the sun, since we may find the extent of that power by the number of square inches of battery surface—does not believe that the evidence in favour of the dissociation of calcium is quite complete; and he believes it possible that with an increase of temperature the more refrangible lines increase in brilliance at the cost of the less refrangible, so that, if the quantity decreases gradually, we may obtain the results above described. I have, however, pointed out to him that this law does not hold good in other cases: for instance, with hydrogen an increase of temperature does not give a greater intensity to the red line C, and in case of sodium at the temperature of the sun we know that the absorption of the yellow line of sodium is more intense than that of any of the rays.—*Comptes Rendus*.

REPORT ON THE DEVELOPMENT OF THE CHEMICAL ARTS DURING THE LAST TEN YEARS.*

By Dr. A. W. HOFMANN.

(Continued from p. 144.)

THE ammonia remains dissolved in the water only under the pressure at which it was saturated, and escapes in proportion as such pressure is diminished; at the same time the liquid is cooled in a corresponding degree, as during the evaporation of pure liquid ammonia. Mort and Nicolle produce this decrease of pressure by means of the air-pump. The ammonia removed is condensed by the return stroke of the piston and along with a corresponding amount of the diluted liquid simultaneously withdrawn from the evaporator is forced through a cooler where re-absorption takes place. This arrangement requires less motive power than a pure mechanical condensation. It is to be expected that this ammonia machine should be more efficient than the ether machine, but its performance falls short of that of Carré's machine. More exact accounts are hitherto not to be had. The machine works at lower pressure than the ether machine, and like this must be carefully protected against the influx of air. The danger of explosion is removed from the machine itself and transferred to the boiler of the engine. About the end of 1870 Mort and Nicolle have patented a new ammonia machine of which the only description in our hands is the English specification. It is described as the "low-pressure ice machine," and agrees in principle with Carré's machine, the air-pump being omitted. It differs, however, from the latter machine in as far as not liquid anhydrous ammonia, but a highly concentrated aqueous ammonia is produced and evaporated. This of course requires a much lower boiler pressure, the maximum tension being about 2 atmospheres at a steam

heat of 107° C. The evaporation of course produces a much smaller reduction of temperature. The arrangement is such that the ammoniacal liquid streaming from above into the ice generator, and flowing slowly over horizontal depressions gradually loses its ammonia, and arriving at the bottom in a very reduced state of concentration is drawn out by means of a pump, and in an especial vessel re-absorbs the ammonia which has been expelled by the heat. The liquid restored to its original degree of concentration is pumped back into the ice generator. The liquid escaping below from the kettle, and which is little more than water, serves as in Carré's machine for the re-absorption of the ammonia evaporating from the ice generator, and is driven back into the boiler by a second pump. It must be mentioned as a speciality that the ammonia evaporating in the boiler passes first into a cylinder with a piston, and furnishes the power for working the pumps, whereupon the absorption takes place.

Air-pump Sulphuric Acid Machine, by Edm. Carré, of Moislains.—We have still to mention a peculiar ice machine invented by the brother of the above-mentioned F. Carré, and also belonging to the class of absorption machines. Hitherto the idea has only been carried into practical execution on the small scale for domestic use. It was shown for the first time at Paris, in 1867, and has since been exhibited at Vienna. In principle it depends upon the cooling and freezing of water by its own evaporation in a vacuum—the well-known experiment of Leslie. E. Carré arranges his apparatus as follows:—A cylindrical vessel consisting of lead alloyed with 5 per cent of antimony is half filled with concentrated sulphuric acid which can be kept in motion by means of a stirrer acting from without. With the upper empty part of the vessel is connected on the one hand an air-pump, and on the other an ascending tube fitted with a cock and slightly bent, so that a flask filled with water may be placed in its end, and an elastic band serving for a lute. All the joints are very carefully adapted so as to prevent all access of air. When the air-pump is set in action the entire air is removed from the connected apparatus, the water evaporates and is absorbed by the sulphuric acid. After some time a crust of ice is formed in the flask, which increases more and more till the whole, which fills about half the flask, is frozen. The author succeeded in forming 340 grms. of ice in forty-five minutes, 60 grms. of water having evaporated. The duration of the operation increases when the sulphuric acid grows hot and becomes diluted. By the use of $1\frac{1}{2}$ litres of concentrated sulphuric acid, 12 flasks of ice, each amounting to 340 grms., can be obtained. The last flask required two hours in freezing, and 75 grms. of water were absorbed. The sulphuric acid had then taken up one-third of its weight of water, and its sp. gr. was 1.6. The cost of a flask of ice was 10 pfennige (about one penny, English) if the sulphuric acid is not put to any further use. In this form the apparatus is exclusively destined for the *Carafe frappée*, i.e., to cool drinking water by means of ice. The writer does not think the machine fit for domestic use, since the smallest entrance of air renders it useless, and satisfactory connection-materials are not easily met with. Concentrated sulphuric acid also is an objectionable article in the household.*

(To be continued.)

ON CERTAIN CIRCUMSTANCES WHICH AFFECT THE PURITY OF WATER SUPPLIED FOR DOMESTIC PURPOSES.

By M. M. PATTISON MUIR, F.R.S.E.,
Assistant Lecturer on Chemistry, Owens College.

(Concluded from p. 146.)

III. *Influence of House Cisterns upon the Water Supply.*—There appears to be a somewhat wide-spread feeling

* "Berichte über die Entwicklung der Chemischen Industrie während des Letzten Jahrzehends."

* *Bad. Gewerbz.*, 1868, 153. *Comptes Rendus*, lxiv., 897. *Dingler Polyt. Journ.*, 77 and 417.

against the use of cisterns in dwelling-houses, which is, I suppose, chiefly due to the fact that the waste pipe from the cistern is generally in connection with the soil pipe which carries off the drainage of the house.

The hurtful sewer gases may thus readily find their way into the cistern, and so contaminate the water therein stored. On the other hand, however, it may be urged that inasmuch as the water in cisterns is frequently changed there is no great probability that the water actually used for domestic purposes will be, at any rate largely, contaminated by sewer gas. I have attempted to obtain some definite measurements of the amount of contamination present in cistern waters, in so far as this may be estimated by the chemical processes at present in our possession.

The method which I have adopted consists in measuring the amount of free and of albuminoid ammonia, and the amount of nitrogen existing as nitrates and nitrites; from these data we may deduce, at any rate comparative measurements, of the purity of various waters.

In order to prove conclusively, for my own satisfaction, that if sewer gases be absorbed by water their presence will be indicated by a marked increase in the quantities of ammonia, free and albuminoid, obtained from that water on analysis, I carried out the following preliminary experiment:—

A quantity of distilled water, free from ammonia, was placed in a porcelain basin, which was covered with porous paper, and suspended at a short distance above the liquid in a sewer which received the refuse from a very large area, chiefly occupied by dwelling-houses, in Glasgow. After ninety-six hours the free and albuminoid ammonia were estimated with the following results:—

Free ammonia = 0.60 mgm. per litre = parts per million.
Albuminoid „ = 0.54 „ „ „ „

It is thus evident that the absorption of sewer gases by water causes a marked increase in the quantities of ammonia obtained on analysis.

The method adopted for the estimation of ammonia was the well known one of Wanklyn and Chapman: the method for the estimation of nitrates was that described by Thorpe in the *Journal of the Chemical Society* for June, 1873. This method consists in evaporating the water, along with a fragment of ignited quicklime, to a small bulk, and then evolving the nitrogen, as ammonia, by the action of zinc, coated with a deposit of spongy copper, at a boiling heat.

In selecting the waters for examination I endeavoured, as far as possible, to obtain typical samples; in this endeavour I was greatly aided by the kindness of Mr. Macleod, the Sanitary Inspector for Glasgow, who obtained for me samples of waters from various houses situated in the lower parts of the town.

The results are calculated as follows:—

a wooden cistern closed at the top. No. 8. From cistern situated just under the slates in a house in a lower locality than any of the preceding. No. 9. From cistern over water-closet in a dwelling-house. No. 10. From cistern similar to above. No. 11. From the cistern same as No. 9. but after stirring up the maddy deposit at the bottom. No. 12. From cistern near the slates in a house where there had been two cases of fever and where the water was complained of.

Omitting for the present No. 11, it is found that No. 2 sample yields the highest number for free and for albuminoid ammonia, also for nitrates. Now this sample was taken from the cistern of a house in which the pipes have been recently entirely renewed, and in which the pipe leading from the water-closet to the main drain is thoroughly ventilated. The water in this cistern is, however, very rarely used; for all domestic purposes a supply is obtained directly from the main; it would therefore appear that sewer gases are slowly absorbed by water stored in such a cistern. That this absorptive action must take place slowly is evident if we look at the results obtained from the other waters. Although many of these waters were taken from badly situated cisterns, yet in none of them can the influence of sewer gases be distinctly traced. We must therefore conclude that the rapidity with which the water in the cistern has been changed has prevented any appreciable action of the gases upon these waters. There are, it is true, slight variations in the numbers obtained, but in no case do we find a notable increase as compared with water from the main pipe.

The amount of ammonia, &c., obtained from a sample of the slimy matter found at the bottom of one of the cisterns (No. 11) indicates that a great part of the ammonium salts, &c., is concentrated therein; this matter may therefore perhaps exercise a certain beneficial effect upon the water.

The general conclusions which I would draw from these results are:—

(1.) That sewer gases are absorbed by water, but that this absorption takes place slowly.

(2.) That in ordinary house cisterns the water is not contaminated to any great extent with sewer gases, probably because of the short time during which this water is allowed to remain in the cistern, and also perhaps because of the deposition of part of the impurities in the muddy substance which settles at the bottom of the cistern.

The general problem of the influence of the means of supply upon potable waters is a very wide one. I offer these measurements as a contribution towards its solution.

TABLE H.

Ammonia and Nitrates found in Various Samples of Water.

Mgms. per Litre = Parts per Million.

	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.	XII.
Free ammonia	0.005	0.085	0.023	—	0.015	0.015	0.010	0.035	0.015	0.075	0.200	0.045
Albuminoid ammonia	0.092	0.120	0.080	0.082	0.090	0.080	0.085	0.085	0.070	0.065	0.370	0.090
Nitrogen as nitrates and nitrites	0.309	0.463	—	0.321	0.360	0.200	0.258	—	0.284	0.306	0.414	—

These samples were obtained from the following situations:—No. 1. From main pipe. No. 2. From cistern in same house, little used. No. 3. From cistern in house similar to No. 2, but water generally used. No. 4. From pipe leading directly out of the bottom of cistern in well situated dwelling-house. No. 5. From cistern in smaller dwelling-house. No. 6. From small cistern supplying part of a dwelling-house only. No. 7. From public well supplied by Loch Katrine water contained in

CORRESPONDENCE.

TREATMENT OF ANTHRACEN OIL.

To the Editor of the Chemical News.

SIR,—In reply to the letter of Mr. William W. Staveley, reviewing my article on this subject in *CHEMICAL NEWS* (vol. xxxiii., p. 99), as I have now made use of the so-

called residual method for some time, I may perhaps be permitted to speak from experience as to the practical mode of working it.

Mr. Staveley has very correctly described the result of my first attempt to work the process on a large scale. I obtained a black greasy mass, full of pitch, and sufficiently uninviting to the eye. Having pressed a small quantity and submitted it to analysis, the result was so satisfactory that I was encouraged to make another trial with a second quantity of oil. For this purpose I selected a perfectly clean still, as I judged that the black colour of the deposit on the first trial was mainly due to the fact that the condensation had been conducted in a still previously used for tar distillations, and containing a quantity of pitch at the bottom. I now obtained a cake perhaps a shade darker in colour than is usually procured from the first crystallisation, but leaving little to complain of on that score.

In order to ensure success in this mode of treatment, therefore, care should be taken in the outset to commence with a clean still, and the condensation should not be pushed too far. The crude anthracen obtained in this way is difficult to filter, but not more so than when the oil is distilled, and by proper means the filtration may be much facilitated.

I was under the impression, when I stated that the solid residue contained 17 per cent anthracen, it would be fully understood by those to whom my article was addressed that the crude anthracen had previously been deprived, by filtration and pressure, of the oily matters associated with it; and I believe Mr. Staveley, although he finds fault with the obscurity of my description, perfectly comprehends my meaning. I am pleased to observe that Mr. Staveley's results on a small scale, as to quality and quantity of anthracen cake produced, are to a certain extent confirmatory of my own; and, should time permit, I hope hereafter to give further and more exact details of the process and its results.—I am, &c.,

A. McDONALD GRAHAM.

Turnchapel, Plymouth,
April 6, 1876.

VANADIUM FOR ANILINE-BLACK.

To the Editor of the Chemical News.

SIR,—In the CHEMICAL NEWS (vol. xxxiii., p. 140), in your summary of the contents of *Reimann's Farber Zeitung* (No. 11), you quote a letter from the French correspondent of that Journal in Rouen, in which he says that aniline-blacks with vanadium are there in use on a large scale. This is doubtless true. I should not have troubled you with this letter if your extract had ended with this statement, but you continue to quote some remarks made by the Editor of that Journal on this subject, in which he says "that it must be confessed that the colourists of Rouen produce far more novelties than their colleagues in all the rest of the world," and with a spirit of self-depreciation which is eminently characteristic of the English you continue—"If Germany makes such a confession, what must England say?"

Permit me to say that in this matter of the development of the use of "vanadium" for dyeing and printing cotton, woollen, and silk fabrics aniline-black, that neither French chemist nor French colourist has had anything to do with it.

The first-published suggestion of the use of vanadium for aniline-blacks was made by the late John Lightfoot, of Accrington—an Englishman: it was worked out by Robt. Pinkney, Esq., of London—another Englishman. Some of the best calico-printers in Lancashire have used vanadium instead of copper, in printing aniline-black, for three or four years past; and the various salts of vanadium, useful for this purpose, have only as yet been prepared on a commercial scale by myself.—I am, &c.,

SAML. MELLOR.

Magnesium Works, Patricroft.

[We quote the following letter from the *Textile Colourist*:—

"Sir,—Having read your article on this most curious and interesting metal, in the March number of the *Textile Colourist*, with much interest, and in view of the most important part which it is destined to play, not only in medicines and photography, but eminently in the art of dyeing and printing silk, woollen, and cotton fabrics, a few words in reference to its recent discovery in greater abundance, and its application to calico-printing, may not be without interest to your subscribers, at the same time that it records historically the progress hitherto attained.

"We are indebted to Prof. Roscoe for the discovery of vanadium in the residues left after the extraction of cobalt by the acid process as lately conducted at the works, Mettram St. Andrew's, Cheshire.

"Availing himself of the use of our furnaces and utensils which were larger than the apparatus, &c., then at his command in the old Owens College, Quay Street, Manchester, a quantity of these residues were worked up early in the year 1867, and sufficient vanadic acid was made to enable him to conduct his exhaustive researches, and to determine once and for all the atomic weight and the true constituents of the various compounds of vanadium.

"All the vanadic acid made having been used up by Dr. Roscoe in these original investigations early in the spring of the year 1870, a larger batch of the residues was put into work, and about 100 ozs. of vanadic acid was produced—the largest amount of vanadic acid there had been in the world up to that date. From this store of 100 ozs. Prof. Roscoe very liberally placed at our disposal about 30 ozs., the rest being required for his further experiments. In December of the same year it was sent up to London, to the eminent metallurgical firm of Johnson, Matthey, and Co., for sale, and it may be regarded as an evidence of the earnestness of purpose with which the late Mr. John Lightfoot, of Accrington, conducted his experiments, and watched the advent of any substance calculated to advance his discovery of the best method of working aniline-black; for there is little doubt that almost immediately afterwards he must have procured some vanadium oxide from this stock, for whilst in his original specification for printing and dyeing fabrics and yarns, and which is dated October 12th, 1870, vanadium is not mentioned, yet in the completed specification of his patent, dated April 12th, 1871, only a little over three months after the first lot of vanadium oxide was offered for sale, he states the fact that other metals and their compounds than copper can be used to form aniline-black, and amongst the rest he enumerates the oxide of vanadium; and in a small work by him on aniline-black, published May 1st, 1871, he records some interesting experiments made with various metals, and in it he states that the *best black* is obtained by the use of vanadium.

"It was without doubt because of Mr. John Lightfoot's ignorance of the marvellous power possessed by the various salts of vanadium when brought into contact with a mixture of aniline hydrochlorate, and the chlorates of sodium and potassium, to form aniline-black, that he did not introduce it into the original specification of his patent, but between the dates October 12th, 1870, and April 12th, 1871, he acquired this information when acting under the advice of his patent agent, who would inform him that the completed specification of his patent could only be an amplification of his original specification, but that no new matter could legally be introduced into it. Although he mentions that vanadium could be used, he does not claim its use in his patent.

"The high price of vanadium oxide—then 60s. per oz.—and its exceeding rarity were reasons why he would decline to apply specially for a patent for its use in the formation of aniline-black; indeed it is on record that about this time he said that by far the best and most powerful agent to use in order to produce aniline-black was vanadium, but that there was not sufficient vanadium in the world to supply the wants of one firm of calico-

printers, much less the requirements of the whole trade.

"It was about this date that Mr. Robert Pinkney, of the firm of Blackwood and Co., London, requiring a substitute for copper for their marking-ink,—independent of and absolutely in ignorance that the late Mr. John Lightfoot was at the same moment engaged experimenting on the same substance, for the same purpose,—made some original investigations upon the use of the salts of vanadium in the formation of a permanent black. He found that only a few grains of the chloride of vanadium—say from 7 to 12 grains for 1 gallon of liquid, consisting of hydrochlorate of aniline and chlorate of soda—were sufficient for the formation of aniline-black; and, acting upon the belief that if vanadium was required it would be forthcoming, he applied for a patent for the use of the salts of vanadium in the formation of aniline-black, October 16th, 1871, and since then hundreds of thousands of bottles of marking-ink, called "Jetoline," have been sold by this firm, in all of which vanadium has been used.

"At the time of the removal of the Owens College from its old premises, Quay Street, to its present palatial buildings, Dr. Roscoe very generously gave me all his stock of residues from the Mottram Mine, amounting to about 1 ton, and at the same time earnestly suggested that every effort should be made to obtain supplies from some other source, for, he argued, there was the requisite furnaces, utensils, and knowledge required to prepare the salts of vanadium, and if once a quantity of this substance, with its wonderful and quite unique properties, existed with reasonable probability of further supplies, many uses for it must sooner or later be developed.

"This advice has been adopted, and in course of time control over the only known deposit of vanadium in the world has been obtained.

"The results have abundantly vindicated the soundness of Dr. Roscoe's advice, and the number of applications opening out for this most marvellous substance has demonstrated the immense value of original researches, even upon substances which may seem to be amongst the rarest in nature.

"Unfortunately the amount of vanadium found in the ore obtained from the deposit named is only a few ounces per ton, but as the properties possessed by the compounds of this metal are of the most extraordinary and valuable character, and as the smallest quantity of it performs a great amount of work, it may possibly suffice for present wants until other and richer deposits are found. Meanwhile, the many and important uses opening out for the salts of this metal in science, manufacture, and arts, mark another step onward in that ultimate victory of man over nature, which it is his privilege and birthright to assume,

"Thanks to chemistry for having enabled us to score this additional victory.

"S. M.

"Magnesium Works, Patricroft."]

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Seances, de l'Academie des Sciences. No. 13, March 27, 1876.

Remark on the Recent Communication of Mr. Lockyer on the New Rays of Calcium.—M. C. Ste.-Claire Deville.—The curious experiment described by Mr. Lockyer in his letter to M. Dumas on the property of calcium in presenting two distinct systems of rays according to the energy of the batteries applied has a profound interest for mineralogists. In a series of lectures which I delivered at the Collège de France, and of which

I gave a summary (*Comptes Rendus*, liv., 782, 880, 949), I showed that the electro-positive elements of natural compounds do not enter indifferently into such compounds, but that some of these bodies belong in a manner virtually to a group of minerals marked out both atomically and crystallographically, and are, on the contrary, always strangers to other even closely approximating groups; but I showed that there exist most frequently between two neighbouring groups two minerals belonging respectively to each of the two groups, and characterised by one and the same basic element, playing this double part, and to which I gave the name of limitary or axial body. But of all the simple bodies calcium most frequently plays this strange part. Is this remarkable property in calcium of belonging at once to different types connected with its twofold behaviour under the influence of dissociating agents? Will the same hold good with titanium, iron, &c., which seem also to play a Protean part in the mineral kingdom?

Report on a Memoir by M. E. Bourgoïn, presented to the Academy, and entitled "Researches in the Succinic Series."—MM. Cahours and Berthelot.—M. Bourgoïn has studied the conditions under which succinic acid may be directly converted into malic acid. The examination of these conditions has led to the discovery of two new acids, the oxymaleic and dioxymaleic.

Study of Stratified Light.—M. Neyreneuff. Judging from his experiments on the action of electricity upon flames and gases, the author admits that there exist in a Geissler's tube traversed by discharges two forces which tend to impress a vibratory movement upon the gaseous mass within. He has found in studying the combustion of detonating mixtures the most important characteristics of electric stratification. The two series of phenomena may therefore be assimilated as regards their producing cause.

Photo-micrographic Researches on the Transformation of Collodion in Photographic Operations.—M. J. Girard.—The microscopic examination of collodion renders it practicable to distinguish the nature of the texture of the stratum, and to follow the reactions which take place in the production of the photographic image. When it is of good quality the layer is translucent, colourless, the cotton being perfectly dissolved. Among the most frequent modifications may be mentioned—First. Old collodion which still gives good impressions, but less rapidly than is to be desired; it contains liquid globules of modified ether. If too alcoholic it has the appearance of cellular tissue, and if water is present the fibrillæ of cotton become apparent in the form of amorphous flocks. Second. Collodion which is too thick; it is intense, but without speed of action, having the appearance of a wavy cellulose-vascular tissue. This want of regularity in the stratum affects the distinctness of the impression which it ought to receive.

Communications at a Distance by means of Water Courses.—M. Bourbouze.—This paper was deposited with the Academy in a sealed packet on November 28, 1870, and was now opened and read at the desire of the author. The experiments described show that it is possible to telegraph to considerable distances without wires; that telluric currents may be substituted for those of the batteries generally employed, provided the immersed surfaces are duly varied, and that such currents may decompose the solutions of metallic salts.

Exchanges of Ammonia between Natural Waters and the Atmosphere.—Th. Schlœsing.—This paper is not suitable for abstraction.

Sources of Carbonic Oxide: New Method of Preparing Highly Concentrated Formic Acid.—Mix formiate of soda with oxalic acid, both dry and in powder. On heating in the water-bath one obtains almost the theoretical quantity of an acid containing 99 per cent of actual formic acid.

MISCELLANEOUS.

The Radiometer.—Current literature, the faithful reflex of popular opinion, proves that the general interest in Mr. Crookes's remarkable discoveries concerning repulsion by radiation and the mechanical action of light continues unabated, notwithstanding the unusual degree of publicity accorded the successive researches of the discoverer, and the large number of recruits which have been thereby attracted to aid in the subjugation of the new territory and report to the less interested bystanders what they have seen. Thus in the last issue of the *Quarterly Journal of Science* will be found a report *in extenso*, with illustrations, of the lecture delivered by Mr. Crookes at the Royal Institution, already briefly summarised by us under the heading of "Weighing a Beam of Light." In *Fraser's Magazine*, also, there is an appreciative article on Mr. Crookes's discoveries, written in a fairly comprehensive and scientific, though essentially popular, manner. Indeed almost every periodical addressed to cultivated readers has had something to say upon the subject at one time or another; and this is true of France, Germany, and the States, no less than of our own country. It is not often that any discovery gives rise to a mass of criticism so large and varied in character as in the present instance. In the hands of a philosopher such as Herbert Spencer, these criticisms would afford fine illustrations of different kinds of bias, and of the errors resulting from each. Mr. Crookes in the hands of his critics is somewhat similarly placed to the member elect in some borough where party feeling runs high. His "friends" would push him ahead faster than he is prepared to go, whilst the other side would metaphorically "bonnet" him and hustle him out of sight with as little ceremony. According to the *Athenæum*, Professor Osborne Reynolds in a recent lecture, not merely ignored Mr. Crookes's labours, but re-christened the radiometer "Geissler's Light Mill," though the latter is in fact nothing but a Crookes's radiometer manufactured by Geissler, of Bonn. On the other hand, some of Mr. Crookes's friends write as if, before his researches, nothing whatever was known of the influence of radiation on delicately suspended bodies, and so lay the one they would befriend open to the charge of ignorance or *suppressio veri*, though his first paper, read before the Royal Society, is prefaced by a most scrupulously detailed list of his predecessors' researches in the same field, and a full acknowledgement of his obligations where such were due. Again, many of his well-wishers reason as if it were a matter of life and death to Mr. Crookes's fame to maintain that the phenomena discovered by him are due to *light* alone, and that all cause for wonder would cease if it were admitted that *heat* had anything to say to them. We noticed how very careful Mr. Crookes was to avoid committing himself to any theory when lecturing at the Royal Institution, and fully appreciating the intention and importance of this reserve, we found space even in our brief notice of the lecture, to record the fact that Mr. Crookes styled the force neither light nor heat, but a "function of the refrangibility." His views on this point are very clearly set forth in the *Quarterly Journal of Science*.

"The human eye," says Mr. Crookes, "by what we may term a physiological accident, is sensitive to but the central portion of the solar spectrum; the part at the red end possesses in the greatest degree the property of causing the sensation of warmth, and of dilating the mercury in a thermometer, and of doing other things which are conveniently classed among the effects of *heat*; the centre part affects the eye, and is therefore called *light*; whilst the part at the other end of the spectrum has the greatest energy in producing chemical action. But it must not be forgotten that any ray of the spectrum, from whatever part it is selected, will produce all these physical actions in more or less degree. A ray in the orange, for instance, if concentrated on

the bulb of a thermometer, will cause the mercury to dilate, and thus show the presence of *heat*; if concentrated on my hand I feel *warmth*; if I throw it on the face of a thermo-pile it will produce a current of electricity; if I throw it upon a sensitive photographic plate it will produce chemical action; and if I throw it upon a radiometer it will produce motion. What, then, am I to call this ray? Is it light, heat, electricity, chemical action, or motion? It is neither. All these actions are inseparable attributes of the ray of that particular wave-length, and are not evidences of separate identities. I can no more split that ray up into five or six different rays, each having different properties, than I can split up the element iron, for instance, into other elements, one possessing the specific gravity of iron, another its magnetic properties, a third its chemical properties, a fourth its conducting power for heat, and so on. A ray of light of a definite refrangibility is one and indivisible, and these different properties of the ray are mere functions of that refrangibility. There is no physical difference between heat and light; so to avoid confusion, I call the total bundle of rays, which come from a candle or the sun, *radiation*."

Perhaps fully one-half of the voluminous correspondence on Mr. Crookes's discoveries would never have seen the light if the writers had been possessed of that small modicum of modesty or self-mistrust necessary to make them doubt whether their own intellectual calibre was such that they knew more of the subject by intuition than another man, a gold medallist of the Royal Society, did after many months' laborious and indisputably skilled research. Had they asked themselves that question, the public would have been spared many folios of turgid truisms, and themselves the shame of advancing theories which had been disproved by experiment perhaps years before. A week or two ago a letter, written evidently in the highest possible spirits, appeared in the columns of a contemporary, announcing that, interesting and important as Mr. Crookes's researches were, his alleged discovery of the mechanical action of light had been anticipated by some forty years. We need hardly remark that if the writer of the letter had read Mr. Crookes's *résumé* of previous investigations, he would have found the experiment referred to duly recorded, though it affects as little the question of priority as Puck's offer to "put a girdle round about the earth in forty minutes" does the originality of the modern system of laying submarine telegraph cables. So long as poor human nature remains what it is, and what it ever has been, it is hopeless to expect that discussions on a new discovery will be conducted without animus and upon merits alone. Lord Brougham, writing in the *Edinburgh Review*, likened Dr. Young's hypothesis of the undulatory theory of light to "the unmanly and unfruitful pleasure of a boyish prurient imagination, or the gratification of a corrupted and depraved appetite." Public opinion no longer admits of such unbridled license in criticism as this, but that the snake though scotched retains his venom fangs, the tone of some criticisms of Mr. Crookes's labours clearly proves. Some allowance should undoubtedly be made for disappointed physicists. As remarked in *Fraser's Magazine*—"The general surprise was not lessened by the fact that the announcement of this mechanical action of light came from an unexpected quarter; for although Mr. Crookes, as the discoverer of thallium and as an original investigator, was well known even outside scientific circles, the public at large would have rather looked for an announcement of this character from one of the more popular exponents of science whom they had delighted to honour." Prof. Huxley has likened the progress of science to that of a nail in the wheel of a coach, since to one sitting on the coach, and regarding the nail alone, the movement appears at times to be retrograde in character, although to one standing afar off each part undoubtedly shares in the general progress of the whole. We may expand the simile, and remark that it is not the big wheels which lead and guide the coach.

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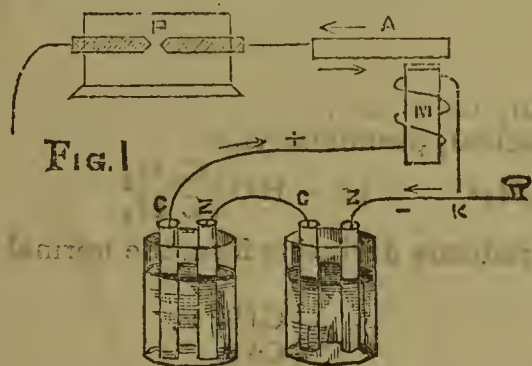
ELECTRICAL PHENOMENA.

THE ALLEGED ETHERIC FORCE. TEST EXPERIMENTS AS TO ITS IDENTITY WITH INDUCED ELECTRICITY.

By PROF. EDWIN J. HOUSTON and
PROF. ELIHU THOMSON.

SINCE the experiments of Mr. Edison are still believed by some to demonstrate the existence of a force hitherto unknown, we submit the following considerations, together with experiments, which we believe to be crucial in establishing the identity of the supposed new force with inverse currents of induced electricity. The alleged necessity for the assumption of the new force being based on its asserted lack of polarity, we propose to show how two opposite phases of the so-called new force may neutralise each other, thus conclusively establishing its polarity.

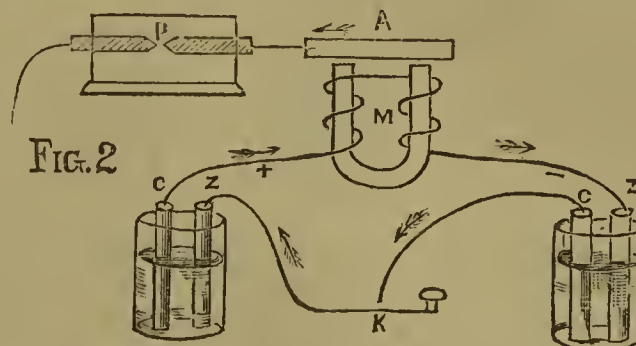
In order to show that, in Mr. Edison's experiments, inverse electrical currents must necessarily exist, notwithstanding the fact that the manifestations occur only at the *opening* or breaking of the circuit, we will discuss his typical experiment in detail. In Fig. 1, we have the



well-known arrangement for the production of the alleged new force. On the completion of the circuit, the battery current flows as shown by the arrows, and *M* becomes a magnet. On breaking the connection as at *K*, the so-called etheric force is manifested at the points *P*, in the dark box. It is evident that the above embraces all the essentials of Mr. Edison's experiments. When a battery current flowing through a considerable length of wire, is interrupted by breaking contact as at *K*, a bright spark of appreciable length is seen at the break (*K*). This spark is due to the extra current, and indicates a great increase of electrical tension in the wire, the discharge occurring through an appreciable air-space at *K*. It will be seen that the wire around the magnet is at the moment of breaking contact, charged with electricity of considerable tension (extra current), positive or negative, according to the direction of the battery current. In Fig. 1, since the magnet wire is connected with the positive pole *c* of the battery, the charge in the wire will be positive, and a negative charge will be accumulated on the general conducting surface of the battery, which thus acts in part to condense the negative charge. This state of tension at once disappears on the discharge of the extra current. The extra current is not produced until the circuit is broken, and its discharge takes place when the wires have been appreciably separated, as shown by the spark. At every break, therefore, the wire surrounding the core of the magnet accumulates a static charge of considerable tension, which is rapidly discharged. This charge, acting by induction on the core of the magnet, induces in it, and in all metallic masses in connection therewith, a flow or charge in one direction, while the tension in the wire is increasing, followed instantaneously by a flow or charge

in the reverse direction for the re-establishment of electrical equilibrium in the cores of the magnet, *consequent on the discharge of the wire itself*, the wire and the cores of the magnets bearing the same relations to each other as the inner and the outer coatings of a Leyden jar. Here, then, we have all that is necessary for the production of the so-called etheric effects, apparent non-polarity included.

In order to prevent the possibility of a charge of any tension remaining in the coils of wire on the interruption of the current, we arranged the following experiment:—A battery of eight cells was divided into two sets of four cells each, as shown in Fig. 2. The sounder magnet *M*,



used in this experiment, was connected as shown, *i.e.*, one end of the coils with the positive pole of the left hand battery, and the other end to the negative pole of the right hand one. An interrupter placed midway between the remaining poles of each battery furnished the necessary breaks, as at *K*. Under these conditions we could obtain no appreciable spark in the dark box at *P*. In this experiment the magnet is placed so as to occupy the exact middle of the circuit, one-half the wire in the coils being influenced by that part of the extra current which produces a positive charge, and the other half by that which produces a negative one. When thus arranged the inductive effects of the extra current being equal and opposite, neutralise each other, and hence no inductive spark appears in the dark box *P*. In this experiment, thorough insulation of the batteries, key, and connecting wires, is necessary, in order to secure an equal division of the effective circuit.

The absolute necessity for the equality of the two divisions of the circuit and of the neighbouring conducting surfaces, in the above experiment, is shown by connecting any part of the circuit with a conducting surface, as, for instance, a mass of metal, or even the body of the experimenter, when sparks at once make their appearance at *P*. The mere approach of the person, *without* contact with any conducting surface near to any part of the circuit, or to either of the batteries, is followed by a similar result. In this connection it is evident that any inequality in the metallic surroundings of the halves of the circuit is sufficient to cause irregularity in the results. So necessary is the *equal* division of the conducting surfaces, that even the use of an ordinary telegraphic key at *K* is sufficient to introduce unequal metallic surfaces into the circuit, and so derange the experiment, and we would therefore suggest that the breaks be made by the conducting wires themselves. If the battery be unequally divided, sparks are seen in abundance in the dark box.

To test the question of the polarity of the alleged new force, the following experiment was devised:—The battery terminals were connected respectively with one end of the coils of the magnets *M* and *M'* which were exact counterparts of each other. The circuit was completed through the interrupter *K*, connected with the two remaining ends of the coils. Wires *a* and *a'* were provided for connecting the cores of the magnets with the dark box *P*, at pleasure. When the wire *a* was connected with *P*, sparks were seen in the dark box, in breaking the contact at *K*; similar sparks were seen where the wire from *a'* was alone connected. When *both a* and *a'* were connected with the dark box *no spark could be obtained*.

In the foregoing experiment it is evident that the

polarity of the extra current produced in M is the opposite of that in M', representing, as they do, the positive and negative poles continued from the battery. Under these circumstances the induced charges in each core being opposite, neutralise each other, and no spark is seen. Since, however, contact of *a* or *a'* with M or M' singly gave all the so-called etheric manifestations, and that when

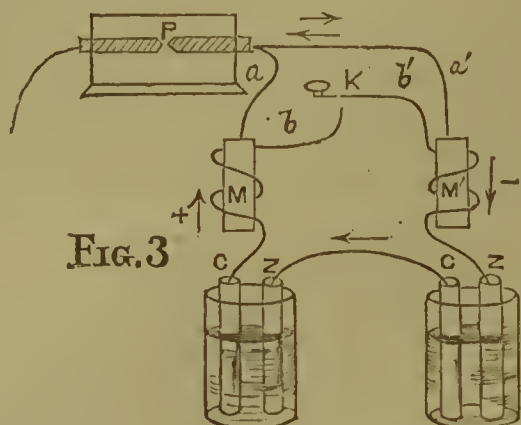


FIG. 3

both were connected no spark was obtained, it is clear that in this experiment is presented unquestioned evidence of that polarity which has apparently been wanting, and which want has thus far furnished the only grounds for the assumption of the discovery of a new force.

That the non-appearance of the spark at *p* was due to an exact neutralisation of the two opposite phases of the "etheric force," is shown by bringing any conducting surface, as the finger, into contact with any part of the circuit, as at *b* or *b'*, when sparks at once appeared at *p*.

We found that it was not necessary to employ cores surrounded by coils of wire to produce the so-called etheric force. We note the following experiment:—A hollow cylinder of non-conducting material as a test-tube was covered on the outside with a sheet of metal. A metallic bar was placed inside the tube, and from it a wire was led to the dark box. On connecting the exterior metal surface with almost any portion of a long battery circuit, which was interrupted, sparks were seen in the dark box at every break. These sparks possess all the properties claimed for the "etheric" sparks. In this case no person at all familiar with electrical induction would for a moment question the true origin of the sparks seen in the dark box.

Metallic coatings are not necessary to produce the effects just described. In the following experiment we

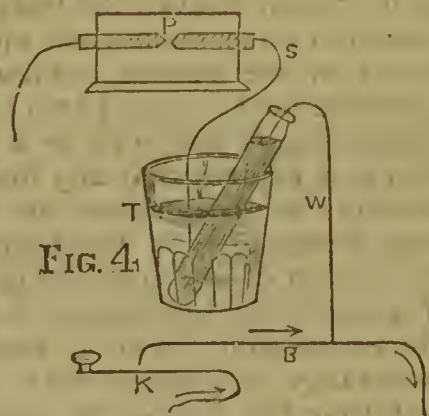


FIG. 4

replaced them by liquid surfaces:—In a tumbler partly filled with slightly acidulated water, a test-tube is placed, also filled with acidulated water. The wire *w*, connected with the battery wire *B*, interrupted at *K*, is inserted in the test-tube. The wire *s*, in connection with the dark box *p*, dips into the liquid in the tumbler. On interruption of the circuit, sparks appear at the dark box *p*. Comment is unnecessary.

It may be interesting to state that the foregoing experiments were thought out in accordance with the known laws of electricity, and the results fully confirmed our expectations. It is hoped that the foregoing experiments will have established still more decidedly the fact that all the manifestations classed as "etheric" are due solely to inverse currents of induced electricity.

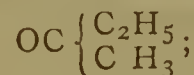
NOTE ON THE FORMULÆ OF ALCOHOLS.

By WILLIAM ODLING, M.A., F.R.S.

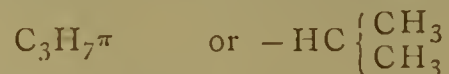
THE accompanying table presents the formulæ of thirty-three alcohols. Of these, eight only are expressed by four-line formulæ, the remainder by three-line and two-line formulæ. Moreover, in order to manifest the relationship of the several formulæ to one another, the majority of them are written more cumbrously than would suffice equally well to exhibit the constitution of the several alcohols individually. Thus the formulæ of the two primary butylic alcohols, or propyl-methol and pseudopropyl-methol, would commonly be written $(\text{HO})\text{H}_2\text{C}.\text{C}_3\text{H}_7$ and $(\text{HO})\text{H}_2\text{C}.\text{CH}(\text{CH}_3)_2$, corresponding to the formulæ for the butyric and isobutyric acids, $\text{HO}_2\text{C}.\text{C}_3\text{H}_7$ and $\text{HO}_2\text{C}.\text{CH}(\text{CH}_3)_2$, respectively. The formula for secondary butylic alcohol, or ethyl-methyl pseudol, would be written—



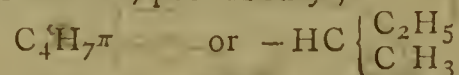
corresponding to that of ethyl-methyl ketone—



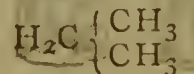
while the formula for tertiary butylic alcohol, or trimethyl kathol, would be written $(\text{HO})\text{C}.\text{CH}_3)_3$. Of the thirty-three alcohols represented, seven are referable in some way to meso-paraffins. In the formulation of these seven alcohols it has been preferred to denote the differently constituted radicals by index letters rather than to use more complex formulæ. In four of these seven formulæ, however, there occurs only the radical pseudopropyl,—



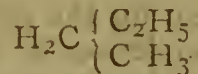
(more commonly called isopropyl), and in a fifth only the homologous radical, pseudobutyl,—



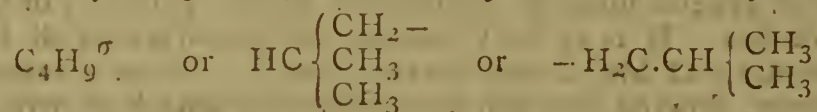
the secondary radicals derivable from the normal paraffins, propane,—



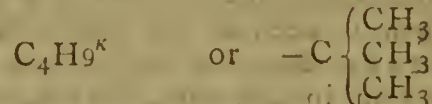
and butane,—



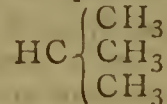
respectively. In the other two formulæ there occur respectively the primary and tertiary radicals, isobutyl,—



and katabutyl,—



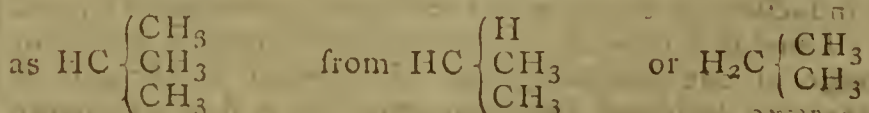
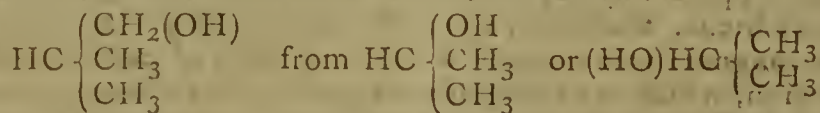
derivable both from the iso-paraffin,—



or isobutane.

It is to be noticed that the formulæ for all the several alcohols are constructed on the type of the formulæ of the several paraffins, normal, iso-, neo-, and meso- respectively, from which the alcohols are formed by substitution of hydroxyl, OH, for hydrogen.

The homologous iso-primary alcohols are derived from a typical iso-primary alcohol; and this typical iso-primary alcohol is derived from the normal secondary alcohol, as is the iso-paraffin from the normal paraffin:—



PARAFFIN ALCOHOLS.

$\text{CH}_4\text{O}.$
Methyllic, and Normal primary Alcohols.

$\text{H}_3\text{C}(\text{OH})$ 66° $\text{H}_2\text{C} \left\{ \begin{array}{l} \text{OH} \\ \text{CH}_3 \end{array} \right.$ 78° $\text{H}_2\text{C} \left\{ \begin{array}{l} \text{CH}_2(\text{OH}) \\ \text{CH}_3 \end{array} \right.$ 98° $\text{H}_2\text{C} \left\{ \begin{array}{l} \text{C}_2\text{H}_4(\text{OH}) \\ \text{C}_2\text{H}_5 \end{array} \right.$ 116° $\text{H}_2\text{C} \left\{ \begin{array}{l} \text{C}_3\text{H}_6(\text{OH}) \\ \text{C}_3\text{H}_7 \end{array} \right.$ 137° $\text{H}_2\text{C} \left\{ \begin{array}{l} \text{C}_4\text{H}_8(\text{OH}) \\ \text{C}_4\text{H}_9 \end{array} \right.$ 157°

$\text{C}_3\text{H}_8\text{O}.$

$\text{C}_4\text{H}_{10}\text{O}.$

$\text{C}_5\text{H}_{12}\text{O}.$

$\text{C}_6\text{H}_{14}\text{O}.$

$\text{C}_5\text{H}_{12}\text{O}.$

(Continued.)

$\text{C}_6\text{H}_{14}\text{O}.$

Normal secondary and Iso-primary Alcohols.

$\text{HC} \left\{ \begin{array}{l} \text{OH} \\ \text{C}_2\text{H}_5 \end{array} \right.$ 83°

$\text{HC} \left\{ \begin{array}{l} \text{C}_2\text{H}_4(\text{OH}) \\ \text{C}_2\text{H}_5 \end{array} \right.$ 109°

$\text{HC} \left\{ \begin{array}{l} \text{C}_2\text{H}_4(\text{OH}) \\ \text{C}_2\text{H}_5 \end{array} \right.$ 130°

$\text{HC} \left\{ \begin{array}{l} \text{C}_3\text{H}_6(\text{OH}) \\ \text{C}_2\text{H}_5 \end{array} \right.$ 150°

$\text{HC} \left\{ \begin{array}{l} \text{OH} \\ \text{C}_3\text{H}_7 \end{array} \right.$ 106°

$\text{HC} \left\{ \begin{array}{l} \text{C}_2\text{H}_2(\text{OH}) \\ \text{C}_3\text{H}_7 \end{array} \right.$ 120—30°

$\text{HC} \left\{ \begin{array}{l} \text{OH} \\ \text{C}_2\text{H}_5 \end{array} \right.$ 97°

$\text{HC} \left\{ \begin{array}{l} \text{C}_2\text{H}_2(\text{OH}) \\ \text{C}_2\text{H}_5 \end{array} \right.$ 125°

$\text{HC} \left\{ \begin{array}{l} \text{C}_2\text{H}_4(\text{OH}) \\ \text{C}_2\text{H}_5 \end{array} \right.$ (?)

Iso-secondary and Meso-primary.

$\text{HC} \left\{ \begin{array}{l} \text{OH} \\ \text{C}_4\text{H}_9 \end{array} \right.$ (?)

$\text{HC} \left\{ \begin{array}{l} \text{OH} \\ \text{C}_4\text{H}_9 \end{array} \right.$ (?)

$\text{HC} \left\{ \begin{array}{l} \text{OH} \\ \text{C}_3\text{H}_7 \end{array} \right.$ 119°

$\text{HC} \left\{ \begin{array}{l} \text{C}_2\text{H}_2(\text{OH}) \\ \text{C}_3\text{H}_7 \end{array} \right.$ (?)

$\text{HC} \left\{ \begin{array}{l} \text{OH} \\ \text{C}_4\text{H}_9 \end{array} \right.$ 137°

$\text{HC} \left\{ \begin{array}{l} \text{OH} \\ \text{C}_3\text{H}_7 \end{array} \right.$ (?)

$\text{HC} \left\{ \begin{array}{l} \text{OH} \\ \text{C}_2\text{H}_5 \end{array} \right.$ 117°

$\text{HC} \left\{ \begin{array}{l} \text{C}_2\text{H}_2(\text{OH}) \\ \text{C}_2\text{H}_5 \end{array} \right.$ (?)

Neo-secondary. $\text{HC} \left\{ \begin{array}{l} \text{OH} \\ \text{C}_4\text{H}_9 \end{array} \right.$ (?)

$\text{HC} \left\{ \begin{array}{l} \text{OH} \\ \text{C}_3\text{H}_7 \end{array} \right.$ 135°

Iso-tertiary and Neo-Primary Alcohols.

Meso-Tertiary. $\text{C} \left\{ \begin{array}{l} \text{OH} \\ \text{C}_3\text{H}_7 \end{array} \right.$ 112°

$\text{C} \left\{ \begin{array}{l} \text{OH} \\ \text{C}_2\text{H}_5 \end{array} \right.$ 82°

$\text{C} \left\{ \begin{array}{l} \text{C}_2\text{H}_2(\text{OH}) \\ \text{C}_2\text{H}_5 \end{array} \right.$ (?)

$\text{C} \left\{ \begin{array}{l} \text{C}_2\text{H}_4(\text{OH}) \\ \text{C}_2\text{H}_5 \end{array} \right.$ (?)

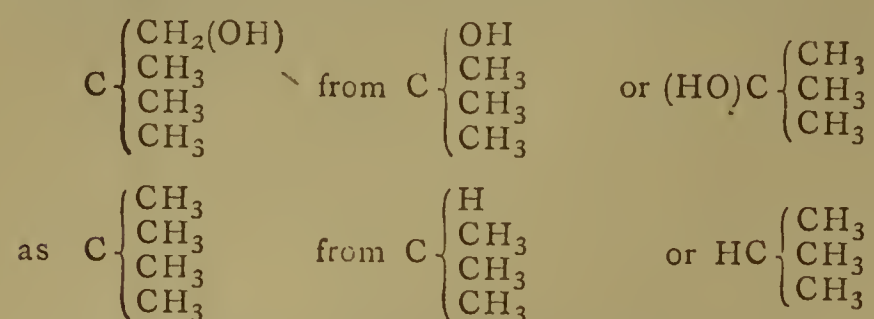
$\text{C} \left\{ \begin{array}{l} \text{OH} \\ \text{C}_2\text{H}_5 \end{array} \right.$ 98°

$\text{C} \left\{ \begin{array}{l} \text{C}_2\text{H}_2(\text{OH}) \\ \text{C}_2\text{H}_5 \end{array} \right.$ (?)

$\text{C} \left\{ \begin{array}{l} \text{OH} \\ \text{C}_3\text{H}_7 \end{array} \right.$ 115°

$\text{C} \left\{ \begin{array}{l} \text{OH} \\ \text{C}_2\text{H}_5 \end{array} \right.$ 120°

Similarly, the homologous neo-primary alcohols are derived from a typical neo-primary alcohol; and this typical neo-primary alcohol is derived from the iso-tertiary alcohol, as is the neo-paraffin from the iso-paraffin:—



Except for the use in a perfectly definite sense, and in a few instances only, of the index letters π , σ , and κ , as above referred to, the whole of the thirty-three alcohols are expressed by comparatively simple formulæ, which manifest their constitution fully, in symbols intelligible, without any new convention, to everyone.

NEW PROCESS FOR PREPARING SULPHO-CONJUGATED ACIDS.

By M. CH. GIRARD.

THE preparation of sulpho-conjugated acids requires in most cases the use of fuming sulphuric acid, or of a mixture of such with common sulphuric acid concentrated to 66° B. The presence of the fuming or Nordhausen acid is necessary to obtain disulpho-conjugated compounds, and those of a higher degree. We may replace with advantage—both as regards cost, facility of working, and amount of yield—the Nordhausen acid with anhydrous bisulphate of soda, either alone or mixed with variable quantities of the ordinary sulphuric acid of commerce. One of the chief advantages of the anhydrous bisulphate of soda lies in its mode of decomposition during the reaction, which has the effect of gradually setting at liberty anhydrous sulphuric acid. The author has prepared, by heating under pressure, and at temperatures varying according to the nature of the substance to be obtained, a mixture of anhydrous bisulphate of soda, concentrated sulphuric acid of commerce, and carbides of the aromatic series, such as benzin, toluen, xylen, and their homologues, naphthalin, anthracen, phenol, cresylol, quinons, and anthraquinons; with alkaloids such as aniline, diphenylamin, and their homologues, the operation may be performed at the ordinary pressure.

To obtain the disulpho-conjugated acids of benzin he heats under pressure for four hours to 200° to 250° a mixture of—

Crystallisable benzin	10 kilos.
Anhydrous sulphate of soda	15 "
Commercial sulphuric acid	25 "

The excess of the salt of soda may be removed either by exhausting with 3 or 4 litres of alcohol, or by letting the mass drain exposed to the air; the sulpho-conjugated acids being deliquescent, flow away, whilst the salt of soda remains in the funnels in a crystalline state. The excess of sulphuric acid is easily removed by neutralising with lime, filtering, and evaporating. To prepare the salt of soda he employs either the sulphate of soda collected or the carbonate of soda, filtering to separate the calcareous precipitate. The solution, evaporated to dryness, yields the soda salt, which latter, on heating to 240° with double its weight of potash, yields resorcin. To obtain the sulpho-conjugated acids of anthracen or of anthraquinon he heats under pressure between 260° and 270° for five to six hours, at 66° B.—

Anthraquinon	10 kilos.
Anhydrous bisulphate of soda	12 "
Commercial sulphuric acid	40 "

The treatment to remove the excess of the salt of soda is the same as that described above; or the mixture may be directly neutralised with lime, the salt of lime decomposed by a salt of soda, sulphate or carbonate, and the sulphate of soda may be separated by crystallisation from the soda salts of the sulpho-conjugated acid.

The disulpho-anthraquinonate of soda, heated with double its weight of potash, soda, or a mixture of the two alkalies, is transformed into alizarin.—*Bull. Soc. Chim. de Paris.*

REPORT

ON THE

DEVELOPMENT OF THE CHEMICAL ARTS DURING THE LAST TEN YEARS.*

By Dr. A. W. HOFMANN.

(Continued from p. 167.)

SUCH machines have been recently made by Eigel and Lesemeister, of Cologne. The duty of a machine of the kind described must, on the supposition that the sulphuric acid expended is recovered by means of concentration, be estimated at a very high rate. From a calculation which certainly was only approximate it would appear that 17 kilos. of ice are produced per 1 kilo. coal used in concentrating the acid. If, in the continuous action of the apparatus, the concentrated acid running off could completely exchange its heat with the dilute acid to be introduced, the effect would be greater by one-third. This result considerably exceeds that of the ammonia machine. The manufacture of ice on this principle would offer certain advantages if the apparatus were differently arranged, since in its present form it is not suitable for lump ice. Perhaps instead of pure water a saline solution might be evaporated, which would be cooled down far below zero, and into which, as in other machines, vessels containing water might be plunged, and the latter might thus be indirectly frozen. The air-pump would require to be put in action only once in order to exhaust the air of the internal space. To open it would be needless, since the sulphuric acid can be introduced, and removed by means of pumps.

III. Production of Cold by Expansion.

If a gas is compressed the mechanical power applied is converted into heat and the temperature rises. If equal volumes of different gases at a similar initial pressure are compressed to the same extent, a gas of lower specific heat increases in temperature more than one of higher specific heat, and that in a potentiated manner since its particles, in the first place, assume a higher temperature by an equal increment of heat, and secondly, since the hotter gas possesses a greater tension and opposes more resistance to compression, whence more heat is evolved. Different gases of equal initial temperature and tension, when compressed to an equal volume, not only attain unequal temperatures, but unequal pressure. The following table shows in what proportion atmospheric air of mean tension increases in temperature if compressed at an initial temperature of 20° C.

Pressure in atmospheres..	1	2	3	4
Temperature	20	85	130	163

If a hot compressed gas is allowed to re-expand, always under full pressure, the heat is transformed into mechanical power, and a fall of temperature ensues in the same measure as the rise during its compression. If a hot and compressed gas is cooled down and then expanded, it falls below the initial temperature, and very great degrees of cold can be attained. Thus air at 2, 3, 4 atmospheres, cooled down to 30° C., and allowed to expand to 1 atmo-

* "Berichte über die Entwicklung der Chemischen Industrie während des Letzten Jahrzehends."

sphere yields respectively the temperatures of -25° , -53° , -70° C. It is pre-supposed that the air, like steam in an engine, works outwardly; if it rushes into an empty space the temperature of the whole mass experiences no change, since the heat lost by the initial expansion is reproduced by the impact of the molecules against the sides of the vessel. If the air drives before it a pressure smaller than corresponds to its own tension, e.g., if, having been strongly condensed in a receiver, it escapes into the open air its fall in temperature is less than as stated above. On these principles depends the application of air to the production of cold and the preparation of ice.

(To be continued)

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, April 20th, 1876.

Professor ANDREWS, F.R.S., in the Chair.

AFTER the minutes of the preceding meeting had been read and confirmed, and the presents announced, the names of Messrs. J. Davidson, D. H. Richards, and W. J. Hannah were read for the first time. Mr Cornelius O'Sullivan and Dr. Rudolph Messel were balloted for and duly elected Fellows of the Society after their names had been read the third time.

The first paper, "*On the Manufacture of Sulphuric Anhydride*," by Dr. R. MESSEL and Dr. W. SQUIRE, was read by the latter. The speaker, after giving a sketch of the history of the manufacture of sulphuric acid, described their process of preparing the anhydride. The vapour of ordinary sulphuric acid is passed through a white-hot platinum tube, whereby it is almost completely decomposed into water, oxygen, and sulphurous anhydride: the mixed gases, after passing through a leaden worm to condense the great portion of the water, are completely dehydrated in a lean tower filled with coke, over which a stream of concentrated sulphuric acid is allowed to trickle. The dry mixture of oxygen and sulphurous anhydride is now passed through platinum tubes heated to low redness, and containing fragments of platinised pumice, when the gases re-combine to form sulphuric anhydride, which is condensed in a series of Woulffe's bottles.

The CHAIRMAN thanked the authors, and, in allusion to a remark of theirs on the difficulty of condensing sulphuric anhydride when mixed with air, said that, in the case of a mixture of equal volumes of air and carbonic anhydride, the latter did not condense even at a most enormous pressure, but on lowering the temperature to 0° C. the carbonic anhydride was condensed.

Dr. ARMSTRONG reminded that the authors had spoken of the Nordhausen acid as a solution of sulphuric anhydride in sulphuric acid, that it was in reality a definite compound, which yielded definite salts and also a corresponding chloride. It might perhaps be called pyrosulphuric acid.

In reply to an observation by Mr. Spiller, Dr. SQUIRE said the Nordhausen acid, as he had examined had a sp. gr. considerably below 1.0.

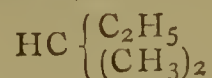
The adjourned discussion of Dr. Armstrong's paper, "*On Systematic Nomenclature*," was then proceeded with.

The AUTHOR said he might perhaps be allowed to make a few additional remarks. At the last meeting he had scarcely been able to do justice to Dr. Odling's paper, chiefly from want of diagrams to illustrate it. These he had now prepared, and from inspection of them it would be evident that in the first series there would be several isomeric, iso-, neo-, meso-paraffins, and for

these Dr. Odling's system did not provide names. He considered Schorlemmer's arrangement of the paraffins unsatisfactory and unscientific, and in its place he would propose to divide them into three groups, represented by the general formulæ $\text{CH}_2(\text{C}_n\text{H}_{2n+1})_2$; $\text{CH}(\text{C}_n\text{H}_{2n+1})_3$; and $\text{C}(\text{C}_n\text{H}_{2n+1})_4$ respectively, the various members of which could be distinguished by the letters of the Greek alphabet in the manner before explained. He desired to see some system of nomenclature which would not only serve to distinguish isomeric compounds, but would assimilate the different series as much as possible. The one he had proposed served to indicate the position which the isomeric compounds occupied with reference to their physical properties.

Dr. ODLING said that he had, in the first place, to express both his appreciation of the friendly tone of Dr. Armstrong's criticism, which he would endeavour to imitate in his reply, and also his obligation to him for bringing the subject forward. He objected himself to Dr. Armstrong's proposal, that it dissociated strictly analogous alcohols by assigning to them a difference of prefix, whilst it associated with one another by an identity of prefix alcohols of the most diverse character, normal and iso- primary, secondary, and tertiary. It moreover dissociated the alcohol from the acid and paraffin into which it was oxidisable and reducible, by a difference of prefix, and accorded a common prefix to those which were not so related to one another. He considered the arrangement of isomeric bodies according to their boiling-points to be inapplicable in the case of isomerides, including (say) aldehyds, ketones, olefine alcohols, olefine ethers, olefine oxides, &c., without prior classification of the isomerides according to their several characters, and similarly with regard to normal and iso- primary, secondary, and tertiary alcohols. He objected, further, that any seeming consistency in Dr. Armstrong's seriation of alcohols was dependent on this seriation being conducted according to a special artificial system which it was assumed would coincide with the seriation by boiling-points; but which he (Dr. Odling) contended would not accord, unless, for example, the relationship between similar primary butyl and propyl derivatives were reversed in the case of secondary butyl and propyl derivatives, and unless, whilst some alcohols differed from their progenitors in boiling-point by about 20° C., other alcohols—derived in precisely the same way—differed from their progenitors in boiling-point by about 40° C. For his own part he considered that the $\alpha\beta\gamma$ system of notation was applicable only as a temporary expedient in the case of compounds in the course of being assimilated into the body of investigated and systematised substances. He believed Dr. Armstrong's formulæ for the alcohols to be confusing, in respect of the number and variety of indices employed; to be cumbrous out of proportion to the information afforded with regard to any particular alcohol; and to fail in indicating the close mutual relationship of some, and the great mutual alienation of other isomeric alcohols. He further objected to Dr. Armstrong's proposal with regard to the nomenclature of isomeric acids. To designate these acids as primary, secondary, and tertiary, was to use the above epithets in an entirely different sense from that in which they are applied to alcohols. In this last sense all acids are primary, and their varieties are dependent on the variety of paraffin from which they are derived; moreover, inconvenience would result from referring the alcohols to a mono-carbon alcohol, and the acids to a dicarbon acid. As regards his own proposals, Dr. Odling contended that the reference of the known paraffins to four types was perfectly well established, and that any familiar mode of designating the different isomeric paraffins should indicate to which of these classes the paraffin belonged, and that any prefix such as "iso" applied to the members of some one class of these paraffins should be confined exclusively to them, and to their derived alcohols, aldehyds, acids, &c. He recognised fully that any paraffin of a more complex class might be formulated

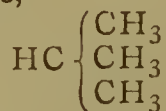
on the type of a paraffin of a simpler class; that hydride of amyl, for instance,—



might be written on the type of ethane, $\text{H}_3\text{C}.\text{CH}_3$, or on that of propane,—



or on that of isobutane,—



and that the paraffin sometimes called di-isopropyl, although belonging to the class of meso-paraffins, might very well be formulated on the type of the iso-class. He further considered that in the description of the monad radicles derivable from the paraffins, it was advisable to give to the secondary radicles derived from the normal paraffins a different prefix from that assigned to the primary radicles derived from the iso-paraffins, and also to accord a distinctive prefix to the tertiary radicles of the iso-paraffins, the prefixes which he suggested being "pseudo" and "kata" for the secondary and tertiary radicles respectively. With regard to the designation of the isomeric alcohols, he thought it was advisable to indicate whether they were derived from normal or iso-paraffins, and whether they were primary, secondary, or tertiary derivatives. He further thought it would be advisable to give to the secondary and tertiary alcohols substantive names, such as the name ketone given to the secondary aldehyds, the names he suggested being pseudol and kathol respectively, whilst the primary alcohols might be designated as methols; these terms being used in the same manner as, and included in, the common designation of carbinol. Lastly, whilst admitting that every variety of formulation was admissible to illustrate special relations, he considered that particular mode of formulation to be most generally advantageous in which a set of alcohols, glycols, olefines, aldehyds, ketones, acids, nitriles, &c., should be expressed by similarly constructed formulæ, which would show at a glance the relations of the several bodies to one another, and especially their common relation to the paraffin from which they are derivable.

Dr. ARMSTRONG said that at that late period of the evening there was no time to reply to Dr. Odling's remarks, but he might say there was less difference between their views than might at first sight appear. He might observe, however, that he desired to use the Greek letters as prefixes merely to show the order in which isomeric bodies would be arranged, having regard to their physical properties.

The CHAIRMAN then adjourned the meeting until Thursday, May 4th, when the following papers will be read:—"On Glycerin-Phosphoric Acid and on Fermentation," by Dr. THUDICHUM and Mr. KINGZETT. "Note on the Occurrence of Benzene in 'Rosin' Light Oils," by Mr. W. SMITH. "On the Action of Water and Various Saline Solutions on Copper," by Mr. T. CARNELLY. "On some New Reactions of Biliverdin," by Dr. THUDICHUM. And "On Fermentation," by Dr. THUDICHUM and Mr. C. T. KINGZETT.

CORRESPONDENCE.

THE NECESSITY FOR ORGANISATION AMONG CHEMISTS.

To the Editor of the Chemical News.

SIR,—I have taken great interest in the discussion opened up by Dr. C. R. Alder Wright, upon the necessity for organisation among chemists. I am also greatly surprised at the little response it has called forth.

The wretched position to which professional chemists are drifting, and the low opinion held of analytical chemistry, is well illustrated by the advertisement to which you refer in your article in the CHEMICAL NEWS (vol. xxxiii., p. 89).

I observed a few months ago, in a mining paper, an advertisement, emanating from a spelter works in Glamorganshire, for a chemist (?) "competent to make the analysis of zinc ores," and who could keep accounts—salary £2 a week. From enquiry and experience I believe I shall not be far out when I state that of chemists in works about 50 per cent receive salaries under £100, and 80 per cent under £150 per annum.

It is perfectly true, as Dr. Wright observes, most of these have merely risen from "bottle-washers," and who have learnt to determine certain substances by mere rule-of-thumb, and are perhaps more adept, from constant experience, at that one thing than even a skilled chemist of more extensive knowledge; but if given any different substance or element to determine, are totally at sea, and entirely incapable of working by induction.

It is a very general thing, in many commercial laboratories, for youths to be employed entirely at one certain thing; for instance, at an "Assay Office" with which I am acquainted one boy is kept to do all the weighing, another all the H_2S precipitations, so that in a single determination the manipulation will have passed through three or four different hands, and the results are then returned as gospel: if they should happen to turn out correct it is by a happy dispensation of Providence,—if not it cannot be helped; and this is done by the holding high positions as commercial analysts.

But then, Sir, the rate of fees quoted by these people will not admit of doing honest work. Will you pay a man (of any position above that of bottle-washer) himself to do an analysis of an iron ore, determining Fe, Mn, S, P, Ca, Si, Al, and Mg, for £3 10s. (which is the advertised price of many firms),—to do it in duplicate, and be able (as advised by Fresenius) to swear to his results? and yet how many brokers and manufacturers will quibble over £5 for an analysis, seemingly forgetting that every £1 saved by employing an inferior chemist may entail a loss of £100 on themselves. Take the case of copper ore. I imagine very few commercial assay or analysts bother to determine the copper within 0.25 per cent over or under the actual quantity present, and yet this would represent 4s. per ton; and as this ore is often bought in parcels of 1000 tons, would represent a loss or gain in the parcel of £200.

I am sure that if a guild or similar institution were established all our best chemists would become members. There seems many varieties of opinion as to how, and in what subjects, the examination should be held. They should certainly be such, and of such a nature, as would be required to see if a candidate is capable of practical chemical work, and to perform correct trustworthy qualitative and quantitative analysis. I mainly do not see the necessity (as suggested by one of your correspondents) for Latin, German, and French to form any of the subjects. A man may be possessed of great skill and experience, and yet not know a word of any but his own language. The subjects should be confined strictly to practical and theoretical chemistry, and go into subjects of general education. It is not for a university degree.

I would beg to suggest the candidate for membership of the guild should fulfil the following conditions:—

1. That he shall provide satisfactory evidence to the examining body, or other constituted authority, that he has received his chemical education under a person of professional status, or in some recognised institution.
2. That he is engaged in practical chemical work, and is following chemistry as a profession.
3. That he can pass a practical examination in analysis, qualitative and quantitative, and in theoretical chemistry, equivalent to the papers given to the

students of the School of Mines who are competing for the Associateship.

Of course the latter would be the main thing necessary for a candidate to pass, and should be of a sufficient stiffness to prove that he had received a thorough chemical education, and was not a "bottle-washer" chemist. This third section might also be divided into organic and inorganic, either of which might be taken by the candidate, as, considering now the wide range of chemistry, one of these is quite sufficient for a man to make his speciality, and it would be unfair if he intended to confine himself to inorganic (or metallurgical) chemistry, as his sole profession, to make him pass a strict examination in organic, and *vice versa*. We have a parallel in the medical profession, as in physician and surgeon.

The diploma granted might specify which it was granted for. It may be urged against this that it would cause complications, but even if it did it would soon cease to exist. We have a parallel among several of the allied professions; for instance, we have civil, mechanical, and mining engineers: besides it would show who was the most proper man to consult on any special subject, so that a client would not go to, say, Dr. Percy to consult him regarding "previous sewage contamination" in a sample of water, nor to Mr. Wanklyn to consult as to the commercial value of an argentiferous copper ore. Besides, if he could, the candidate may pass in both inorganic and organic; but what I wish to urge is the unfairness of mixing the two.

From the general tone of your correspondents' letters they seem to insinuate that this guild is to be formed to support commercial analysts, but I think it should certainly embrace works' chemists, who require quite as high a degree of skill as our commercial friends, considering that they generally have to work against them, and the operations often occurring in manufactures involving chemical questions.

The diploma of the guild would also at once place a chemist seeking for an appointment in a more favourable position than now, when he has to contest with "bottle-washers" and students fresh from the class-rooms, besides enabling him to command a salary at least equal to that of a collier in full work, instead of the wretched salaries many receive—not being more than that paid to a yard foreman or inferior clerk.

I can assure you, Sir, that many skilled artizans in the various Birmingham trades can earn far higher salaries than the income of many of our professional chemists. To quote a case:—I am acquainted with a hammerman at a Steel Works in Glamorganshire who received a standing salary of £400 per annum.

I perfectly agree with you, Sir, that those persons rejoicing in a sufficient private income had far better devote themselves to original research than degrade themselves and the profession by doing work at wretchedly low fees. What would be thought of a barrister who pleaded causes at half-a-crown a head? I imagine he would be very soon ejected by his profession; and the cases are exactly similar.

I shall be glad to hear that a movement is being made by the metropolitan chemists towards the formation of this guild, and can assure them that they will meet with the hearty co-operation of their provincial brethren. Could they arrange a meeting, and let a report of the minutes be sent to every F.C.S.? Subscriptions might be invited to defray expenses of same. But unless something is done the profession of analytical chemist will degenerate to such an extent as not to be worth following, especially as it is becoming combined with that of clerk, foreman, and messenger. Perhaps we shall soon see the public analyst also performing the office of parish beadle.

In conclusion I would beg to observe, to brokers, manufacturers, &c., in the words of a celebrated physician, that "advice that costs nothing is worth nothing."—I am, &c.,

C. H. ALLDRED, F.C.S.

Burry Port, S. Wales, April 10, 1876.

ORGANISATION FOR ANALYTICAL CHEMISTS

To the Editor of the Chemical News.

SIR,—I cannot allow this subject to pass from the notice of your readers without publicly recording my vote in its favour. It is beyond doubt that such a step is not only desirable, but absolutely necessary, to secure the position due to the profession. Medical men, lawyers, pharmacists, and even druggists, are very properly *obliged* to show due qualification before they can practise or commence business, but anyone can call himself an analytical chemist without possessing the meanest claim. I say it is time analytical chemists established a *locus standi* against such intrusion. During my practice I have heard, in Law Courts and elsewhere, more ridiculous statements, if possible, than those recorded by Mr. A. H. Allen (CHEM. NEWS, vol. xxxiii., p. 94). Undoubtedly there will be some difficulty at the commencement of such an organisation, but the difficulties will increase with time; therefore action cannot be taken too soon.

I look upon the general suggestion of Mr. C. H. Piesse (CHEM. NEWS, vol. xxxiii., p. 116) with general favour. A board consisting of our *recognised leading Professors* would give general satisfaction, in first of all passing those for members who have *unquestionable* qualifications, and then considering the merits—by examination or otherwise—of all the analytical chemists in practice, making it compulsory afterwards for certain qualifications to be attained before being allowed to practise as an analytical chemist.

Promising my share towards preliminary expenses to form such an organisation—I am, &c.,

E. W. T. JONES.

Public Analysts' Laboratory, 10, Victoria St., Wolverhampton,
April 4, 1876.

PS.—I think Mr. Allen will find himself and all public analysts exempt from jury service by being public officers.

BOILING-POINTS AND VAPOUR VOLUME.

To the Editor of the Chemical News.

SIR,—1. Can anyone kindly tell me how it is that of bodies reputed to have the same type and the same vapour volume, the denser should have the lower boiling-points?

	At. weight.	Boils.
Meth-alcohol	32	66°
Alcohol	46	78°
Meth-mercaptan	48	21°
Mercaptan	62	36°

2. Also, do I understand B. Aronheim correctly that phenyl-butylen ($C_{10}H_{12}$) has for atomic weight 132, and vapour density $132 = 1$ vol., the normal boiling-point quite precluding any idea of its being a polymer? (CHEM. NEWS, vol. xxvii., p. 46.)

3. Can anyone refer me to a satisfactory determination of the atomic weight of mercury? I have followed with much distrust the usual 200 and (2 vols.), but how often do we see that metal evincing the equivalence of 2H?

Silver acetamide $AcHAgN$

Mercury „ $AcHgN$, &c.

If it be settled that copper may have two allotropic forms with a certain equivalence of mono- and di weight, might not the same allotropism explain the anomalies of Hg as normally 100 and 1 vol.?—I am, &c.,

S. E. P.

Fall of a Meteorite.—The *Wolverhampton Evening Express* of the 22nd inst. records the fall of a meteorite near Wellington, on the afternoon of the 20th inst. It weighed about 8 lbs., and had penetrated to a depth of 18 inches, passing through 4 inches of soil and 14 inches of solid clay down to the gravel.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Seances, de l'Academie des Sciences. No. 14, April 3, 1876.

Detection of Vinic Alcohol in Mixtures, and especially in Presence of Wood-Spirit.—MM. A. Riche and C. Bardy.—The method of the authors depends on the following principles:—Aldehyds turn the red colour of the salts of rosaniline to a violet. Methylal and acetal possess the same property. This colour resists the action of sulphurous acid, which readily discharges magenta. If the liquid does not mark 80° on the alcoholometer—the case with all commercial alcohols—4 c.c. are poured into the body of a small retort, and 6 c.c. of ordinary sulphuric acid are then carefully added. After having warmed the apparatus for an instant 10 c.c. of water are added, the apparatus is stoppered and heated, and 7 to 8 c.c. are allowed to distil over into a graduated test-tube in which have been put 10 c.c. of water. Into the test-tube are further poured 5 c.c. of sulphuric acid at 21° B., and 10 c.c. solution of permanganate of potash at 4° B. After from three to five minutes the liquid will have become decidedly brown, and 4 c.c. of hyposulphite of soda at 33° B. are added, and then the same measure of a solution of magenta at 2 centigrms. per litre. If the liquid in question marks less than 80° it is diluted with water down to 5°, and 30 c.c. are taken and distilled with 10 c.c. of sulphuric acid, and 12 c.c. are collected and mixed successively with 4 c.c. of acid and the other reagents in the proportions given above. Under these circumstances wood-spirit gives a yellowish white liquid, whilst if it is accompanied by vinic alcohol the liquid takes a violet colouration, the more intense in proportion to the quantity of such alcohol. The operation requires only a few minutes, and the test-tube is marked so as to indicate the quantities of the reagents to be introduced. Aceton, formic acid, isopropyl alcohol, do not give a colouration under these circumstances. But the case is different with the propyl, butylic, and amylic alcohols. This is not of practical importance, because these alcohols are not found in commerce in a separate state, and only occur in vinic alcohol. In this case, however, the authors let down the alcohol to 5°, and treat successively with 5 c.c. of acid, 5 c.c. of permanganate, 2 c.c. of hyposulphite, and 4 c.c. of magenta. The methylic, butylic, and amylic alcohols yield a sulphur-yellow solution, the propylic gives a greenish grey tint, whilst vinic alcohol produces a purple colouration.

Justus Liebig's Annalen der Chemie,
Band 180, Heft 3.

Researches carried on in the Laboratory of Prof. v. Gorup-Besanez, of Erlangen.—These consist of papers on certain compounds of tellurium, by Dr. F. Becker; on certain constituents of Angelica root, by Dr. Carl Brimmer; and on the preparation of glycol, by Otto Lietzenmayer. Dr. Becker obtains pure tellurium by the following process:—Into a combustion-tube 80 centimetres in length he introduced a plug of asbestos so as to remain at the distance of 20 centimetres from the end. Upon this followed coarsely powdered crude tellurium (containing 75 to 80 per cent of pure tellurium), and in front of that another asbestos plug. The back end of the tube was then connected with a gas apparatus, fitted up so as to furnish a current of perfectly dry hydrogen. To the mouth of the tube was fitted a descending tube, and the tellurium was then heated to redness in the combustion-furnace. The combustion-tube must be heated very strongly from its posterior end to beyond the second asbestos plug, and the current of hydrogen must be uninterrupted. The sublimation then goes on readily and quickly. The

residue between the two asbestos plugs containing metallic tellurides was heated in dry chlorine gas. Chloride of tellurium was deposited in the cool parts of the apparatus as a yellowish white powder. Thick layers of pure tellurium, when fused, appear yellowish brown and translucent. A small amount of telluride of hydrogen is formed on subliming tellurium in hydrogen gas. Telluric acid is most readily formed by dissolving tellurous acid in nitric acid, and adding peroxide of lead in slight excess. The precipitates obtained by passing sulphide of hydrogen into solutions of tellurous and telluric acids are not definite compounds, but merely mixtures of tellurium and sulphur. Attempts to prepare a carbide, a cyanide, and a nitride of tellurium were not successful.

Communications from the Laboratory of the University of Halle.—These consist of a paper on the action of sulphuretted hydrogen upon the alkaloids, by Ernst Schmidt; an account of β -naphthoe acid, by Paul Vieth; and a paper on dipseudo-propyl-keton and methylpseudo-propyl-keton, by R. Münch.

Remarks on the "Mercuric Oxyrhodanid" described by A. Fleischer.—J. Philipp.—The author, having described the same body some years ago, differs from the views of Fleischer as to its constitution.

On Gentisin.—H. Hlasiwetz and J. Habermann.—If gentisin is treated with sodium amalgam an orange solution is first obtained, which passes quickly into deep green, brownish red, and then becomes almost colourless. On the addition of dilute sulphuric acid a cherry-red amorphous body is thrown down, which, when washed, becomes a cochineal-red, dissolves in ammonia with a red colour, and is re-precipitated by acids. Its formula appears to be $C_{13}H_{13}O_4$, differing from that of gentisin merely by CO.

Milky Juice of Cynanchum Acutum.—A. Butleroff.—Already noticed.

Bulletin de la Societe Chimique de Paris,
No. 6, March 30, 1876.

Note on Sulpho-phenyl-urea.—M. P. de Clermont.—The author causes hydrochlorate of phenylamin to act upon sulphocyanide of ammonium in equivalent proportions at the temperature of 100° C. in the water-bath, and obtains thus sulpho-phenyl-urea. If this substance is heated to 180° in a closed vessel it is decomposed, yielding a mixture of sulphocyanic acid, ammonia, phenylamin, hydrosulphate of ammonia, and diphenyl-sulpho-carbamide.

Determination of Nitrogen in Organic Compounds.—M. A. Dupré.—The author proposes a modification of the well known process of Dumas. His method cannot be understood without the accompanying illustration.

Secondary Monamines formed by the Action of Liquid Toluydin upon the Hydrochlorate of Aniline.—Ch. Girard and E. Willm.—The authors obtain phenyl-ortho-cresylamin in hard, brilliant, crystalline needles, which, if treated with a drop of sulphuric and nitric acid, produce a fine blue colouration. Solid dicresylamin gives a straw-yellow colouration in contact with nitric acid. Among the other products are diphenylamin, and a body which gives with nitric acid a blue colouration inclining to violet, and which is liquid phenyl-toluydin, phenylpara-cresylamin, or perhaps ortho-para-dicresylamin.

On Sulpho-naphthalide.—M. P. T. Clève.—The author finds the composition of this body to be—

Carbon	75.48
Hydrogen	4.40
Sulphur	10.06
Oxygen	10.06

100.00

answering to the formula $C_{20}H_{14}SO_2$. Its composition is the same as that of sulpho-naphthalin.

Action of PCl_3 on β -Naphthol.—P. T. Clève and H. Juhlin-Dannfelt.—The result of the reaction is β -mono-

chlorated naphthalin, a crystalline mass having the consistence and appearance of stearin. It consists of—

Carbon	73.84
Hydrogen	4.31
Chlorine	21.85
	100.00

Action of Compound Ammonias upon the Sulphocyanates of the Acid Radicals.—M. P. Miquel.—The author has obtained by this action phenyl-acetyl-sulphurea, phenyl-benzoyl-sulphurea, and benzyl-benzoyl-sulphurea.

Composition of the Black Matter obtained on Calcining Ferrocyanide of Potassium.—M. A. Terreil.—Already noticed.

MISCELLANEOUS.

Sanitary Conference.—A Sanitary Conference, convened by the British Medical and Social Science Associations, will be held on the 11th and 12th of May, 1876, at the rooms of the Society of Arts, John Street, Adelphi, commencing at 11 A.M. on each day, under the presidency of the Right Hon. the Lord Aberdare. The following are the points to which it is proposed specially to direct the attention of the Conference:—

1. Do the existing boundaries of urban and rural districts furnish such a division of the country as enables authorities to exercise their powers and fulfil their obligations, for sanitary and other purposes of local government, in the most effective manner?

2. What preliminary inquiries, if any, are necessary to determine the question? and if, ultimately, districts should be reconstructed—

a. What principles should govern their reconstitution?

b. How can this reconstitution be best carried into effect?

3. Should there be more than one authority within the limits of any one of the boundaries so reconstituted, or should area and authority coincide; and should all authorities have the same obligations, be governed by the same sanctions, and be invested with the same powers? Should new authorities be constituted, and is it desirable to have any intermediate representative local board between sanitary authorities and the local government board?

4. Are the powers already granted to local sanitary authorities in any respect inadequate to fulfil their intention: and should all powers and purposes of local government be vested in, and carried out by, one and the same authority.

5. What, if any, alteration should be made in the incidence of taxation for sanitary and other purposes, so as to insure that payments should, as nearly as possible, coincide in amount with direct benefits?

6. What executive officers are essential to good local administration, and how should they be appointed, regulated, and paid?

7. What alterations, if any, in the central executive organisation, are needed to augment the efficiency of local administration?

PATENTS.

ABRIDGMENTS OF PROVISIONAL AND COMPLETE SPECIFICATIONS.

Improvements in the treatment of ores and minerals for the purpose of obtaining useful products therefrom. B. Tanner, F.C.S., Dublin, Ireland. January 25, 1875.—No. 279. This invention consists in the treatment of ores and minerals which contain zinc, lead, copper, silver, gold, iron, and sulphur, and of which the "bluestone" found in the Isle of Anglesea may be taken as an example. The chief advantage

obtained is the separation of the zinc and lead from the ore or mineral. The ore or mineral is first reduced to a state of fine division, and treated with hydrochloric or sulphuric acid, or with other chlorides or sulphates capable of bringing the zinc and lead into a soluble condition. The solution is then submitted to a series of decompositions and precipitations for the separation of the metals contained therein.

Improvements in the manufacture of blacking. J. C. Coombe, Barnsbury, Middlesex. January 25, 1875.—No. 287. The inventor takes a certain quantity of the ordinary starch, and dissolves same in hot or boiling water as is usually done, and, whilst hot, introduces into the starch oil, wax, or any suitable greasy matter, and stirs the mixture, and same is allowed to cool. When the mixture is cold a small quantity of iodine dissolved in spirits of wine is added, and the result of the addition of the iodine is that the mixture is converted into a bluish black colour. To a gallon (more or less) of the above mixture is added about 8 ounces (more or less) of a solution of perchloride or other persalts of iron, and also a small quantity of gallic or tannic acids singly or mixed, and, following this, about 2 drachms of oil of cloves with 8 ounces of glycerine (more or less) is added and thoroughly stirred. In making superior blacking suitable carbon blacks are added.

Improvements in purifying gas for inflating and rendering ascensive balloons and other aerial machines, and in the apparatus employed therein. J. Simmons, Regent Street, London, and J. M. Morris, Hanover Street, London, W. January 25, 1875.—No. 289. An improved method of purifying hydrogen gas by filtering it through a mixture of lime and water is described; also an improved construction of apparatus combining within itself a purifier and gasometer; also an improved means of emptying the retorts when necessary.

An improved carbonaceous material suitable for filtering, deodorising, and other analogous purposes, and in the apparatus employed in its manufacture. K. Weare and S. Isherwood, both of Manchester. January 28, 1875.—No. 326. The features of novelty in this invention consist in the production of an improved carbonaceous material by mixing clay with sawdust, clinkers, and refuse from furnaces, together with tanners' waste or the nuts used by tanners, and charring the mixture in an apparatus consisting of three, five, seven, or more cylinders revolving in or over a furnace, and arranged in the case, say of three, with two small upper cylinders and one large lower cylinder. The material is fed into the top cylinder, and after being partially carbonised passes through suitable hoppers to the lower one, all the extremities of these cylinders being fitted with covers to obtain ready access to the interior.

An improvement in purifying iron, and in apparatus used therein. W. Baker, Willenhall, Stafford. January 28, 1875.—No. 327. The inventor employs a vessel or trough placed between the furnace and the moulds, or other receiver for the molten metal, and forms the vessel or trough preferably oblong, and a few inches deep on one side, and shelving up to the top on the other side. He closes the ends of the vessel or trough to retain the metal to be acted upon, and forms an opening in the top of each end, one for the admission and the other for the discharge of the molten metal. He forces air through tuyeres placed along that side of the vessel or trough to which the bottom shelves up, and inclines the tuyeres towards the surface of the metal with their nozzles nearly touching the metal, so that the air will be forced into and through the metal. He carries up the sides of the vessel or trough and covers the top with a perforated plate. The metal flows through the vessel or trough, and is purified by the action of the injected air.

Improvements in apparatus for the manufacture of chlorine. H. Deacon, Appleton House, Widnes, Lancaster. January 28, 1875.—No. 332. This invention consists in the construction and use in the manufacture of chlorine of a vessel containing gratings so supporting the chemically active porous material that it may be easily withdrawn and replaced and form on its upper and under surfaces angles with the horizon parallel to each other, and such vessel being provided with suitable openings for the introduction and withdrawal of the chemically active material, and for the entrance and exit of the gases which pass through the withdrawable porous material.

An improved coating for iron and other metals. W. R. Lake, Southampton Buildings, London. (A communication from D. R. Brownlow and G. W. Francis, both of Middletown, Connecticut, U.S.A.) January 29, 1875.—No. 340. This invention relates to an adhering coating for iron and other metals, composed of the ordinary slag from an iron furnace, or its equivalent, and borax or other equivalent flux, by means of which the metal is perfectly protected from the action of the air or of moisture, and so that the metal can be subjected to a high degree of heat without injury to itself or the said coating.

Improved processes and apparatus for manufacturing ice, cooling buildings, and other cooling or refrigerating purposes. C. P. N. Weatherby, Southampton Buildings, London. January 29, 1875.—No. 351. My invention relates, first, to the production of cold by a peculiar method and apparatus by which atmospheric air is compressed, cooled, and expanded more effectually than heretofore. It is desirable in making ice according to the second part of my invention to reduce the air passing through the ice-box or refrigerator to as low a degree as possible. In carrying into practice this part of my invention I use apparatus or machinery comprising a steam cylinder, various force-pumps, water-jet pumps, expansion-power cylinders worked by compressed air, an ice-chamber containing near the centre a water-vessel [in which the water is frozen, and a graduated vessel*] to supply water to the said ice-box. I modify my invention by combining the cooling effect produced by the alternate compression, cooling, and evaporation of a liquid having a low boiling-point, or of a condensable

* The words "in which the water is frozen, and a graduated vessel" are found in the copy of the abridgment delivered by the applicant, but do not appear in the original abridgment.

gas, and the cooling effect produced by the alternate compression, cooling, and expansion of atmospheric air.

Improvements in filters or apparatus intended chiefly to clear rain-water during its flow from roofs to reservoirs. C. Lambert, Sunk Island, Hull, York. January 30, 1875.—No. 360. The apparatus consists of an outer casing attached to the wall of the building, within which is an inner vessel into which the water is led. The upper part of the inner casing has holes through which the water passes into a filtering medium held between perforated plates through which it passes to a reservoir.

NOTES AND QUERIES.

* * Our Notes and Queries column was opened for the purpose of giving and obtaining information likely to be of use to our readers generally. We cannot undertake to let this column be the means of transmitting merely private information, or such trade notices as should legitimately come in the advertising columns.

Nitrous Compounds.—Can any of your readers inform me of the best method of getting rid of nitrous compounds in commercial sulphuric acid?—A SUBSCRIBER.

Sawdust Manure.—Sawdust that has been used in stables for the bedding of horses (in lieu of straw) is found to have a less beneficial effect upon land than manure of straw. Could any reader inform me—(1) What is the probable cause of this? (2) What would be a likely remedy? (3) Would keeping the manure for a lengthened time make it serviceable?—F. H. T. A.

Potassium Flame.—Text-books and teachers generally aver that a potash salt can be detected in presence of a soda salt by looking at the flame through a blue glass, when it appears violet or reddish violet. Recently I obtained a reddish violet flame from common salt, crystallised carbonate of soda, and Howard's pure carbonate of soda, and also from sodium ignited on water. Would some one kindly explain this?—F. H. T. A.

MEETINGS FOR THE WEEK.

SATURDAY, 29th.—Physical, 3. "On an Improvement in the Helio-stat," by Sir John Conroy, Bart. "On the Supposed New (Etheric) Force," by S. P. Thompson.

MONDAY, May 1st.—Medical, 8.
Royal Institution, 2. (Annual Meeting).
Society of Arts, 8. Cantor Lectures. "Wool Dyeing," by George Jarman.

TUESDAY, 2nd.—Civil Engineers, 8.
Royal Institution, 3. "Comparative Geology and former Physical Geographies of India, Australia, and South Africa," by Prof. P. M. Duncan, F.R.S.
Zoological, 8.30.

WEDNESDAY, 3rd.—Society of Arts, 8. "Preparation of China Clay," by J. H. Collins.
Microscopical, 8.

THURSDAY, 4th.—Royal, 8.30.
Chemical, 8.
Royal Institution, 3. "On Voltaic Electricity," by Prof. Tyndall, D.C.L., LL.D., F.R.S.
Royal Society Club, 6.30.

FRIDAY, 5th.—Royal Institution, 8. Weekly Meeting. 9. "On Methods of Chemical Decomposition Illustrated by Water," by Prof. Gladstone.
Geologist's Association, 8.
Society of Arts, 8. (Indian Section). "Irrigation Works in India," by W. T. Thornton.

SATURDAY, 6th.—Royal Institution, 3. "On Crustacea," by Henry Woodward.

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THE CHEMICAL NEWS.

VOL. XXXII. No. 858.

THE CHEMISTRY OF DIABETES MELLITUS.

By CHARLES T. KINGZETT, F.C.S. London and Berlin.

IN the present article the writer proposes to briefly indicate what is known regarding the chemistry of the disease called "Diabetes Mellitus," and to consider the results obtained recently by Pavy in his researches.

In a work on Diabetes Mellitus, published by Dr. Rollo in 1798, is contained a most valuable mass of information concerning this disease, besides the description of many experiments with urine by Mr. Cruickshank, which, crude as they are in some respects, constitute indications of work which has been more elaborately wrought out since by other observers.

Rollo alleged that "this disease consists in an increased morbid action of the stomach with too great a secretion, and an alteration in the quality of the gastric fluid, producing saccharine matter by a decomposition of the vegetable substances taken in with the food, which remains unchanged." (Rollo on "Diabetes Mellitus," p. 387.)

This view of the disease he maintained against Dr. Baillie and others, who regarded it rather as consisting in a morbid state of the kidneys. But Dr. Rollo argued that the kidneys, from their structure, are incapable of forming sugar, while they simply act as separating agents, allowing that they might become morbid through the increased activity and sympathy they are subjected to in the disease. On this basis he explained the fact which he had recognised, that the serum of the blood contains, in cases of diabetes, less sugar than the urine. In consonance with these views sugar-forming foods were avoided in treating cases of this disease, while such oxidising agents as potassic chlorate and nitric acid were recommended, as bodies supposed to be capable of oxidising the sugar existing in the circulation.

Subsequently, Bernard and Hensen discovered in the liver a kind of dextrin of formula $C_6H_{10}O_5$, and they concluded that it was produced through a decomposition of albumen exerted through that organ. These views finally developed into a theory called "the glycogenetic function of the liver," which function was supposed to consist in the first place of the production of dextrin (glycogen) by the liver, and the transformation of this into sugar, which, passing into the blood by way of the hepatic vein, was supposed to become oxidised in the circulation during health. A lack of oxidising power was thus conjectured to constitute the disease, Diabetes Mellitus, so that the sugar is voided in its unchanged state. The above theory of Bernard and Hensen derived weight from the fact that dead liver is capable of quickly converting glycogen into sugar, and was upheld for many years by physiologists. Pavy—one of its chief admirers—nevertheless was the first to reject it as erroneous, and on the grounds that, according to him, no sugar is made in the liver in health. As some sugar could be found in hepatic blood, Pavy's experiments were criticised, among others, by Thudichum, who claimed to show that "they admitted of such variation as to prove either his or Bernard's doctrine" ("Chem. Phys.," Thudichum, p. 8).

Before considering Pavy's more recent researches let us now briefly notice some experiments which were conducted by Dr. A. Dupré, and published in the *Practitioner*. These experiments were made with the view of testing the prevailing theory of diabetes, and consisted in the administration of known amounts of fruit sugar (in the form of honey) to a diabetic patient, and in the estimation of the amount of the sugar passed in the urine. The quantitative determinations were effected in

the ordinary chemical way, and were supplemented by optical determinations, by which he showed that all the fruit-sugar contained in the honey administered was oxidised in the system.

Mr. Abernethy had observed that sugar (of an uncertain quality) as food increased that voided in the urine, but this must be accepted as meaning *much* sugar given as food, for, as we have seen, up to a certain amount, that which is administered is oxidised in the circulation.

From Dupré's experiments we can almost conclude that diabetes is not to be explained as due to a lack of oxidising power in the blood. But Dr. Dupré went further than this: he thought his results pointed to the conclusion "that the sugar found in the urine has not previously existed ready formed in the blood, but has been formed only in its passage through the kidneys."

The first of these conclusions is, we think, supported by Bernard's own observation, that an artificial diabetes (glycosuria of Pavy) can be developed in dogs or rabbits by irritation with a needle of the fourth ventricle of the brain, whereby, after some time, sugar is developed in the blood and in the urine.

The second conclusion may derive some slight support from Lehmann and Dupré's observations, that the percentage of sugar found in the blood is extremely small, and generally amounting to 0.04 per cent only, a quantity not easily comparable with that which is excreted in the urine. But while this conclusion derives such a measure of support, it must be observed that it is in opposition to all our knowledge of the functions of the kidneys. Moreover, there are cases on record in which the sugar present in diabetic blood exceeded this amount: thus Dr. Thudichum found in one case as much as 2 per cent of sugar in the blood.

Now thus far we have only regarded the production of sugar as a factor in the disease, while Dupré observed that the administration of sugar as food caused a perceptible decrease in the amount of urea excreted, a fact also previously observed by F. Hoppe (*Arch. f. Path. Anat.*, x., pp. 144 to 169). Moreover, there are other evidences of a perversion of chemical agencies, consisting in the alleged appearance of lactic acid in the saliva, and of acetone in the stomach and the urine, the latter statement having been quite recently re-asserted. But the statement of the presence of acetone in the stomach, &c., must be accepted with caution, as the tests which have been adopted for its detection are also tests for *alcohol*.

With these data before us let us now consider the more recent researches of Pavy, "On the Production of Glycosuria by the Effect of Oxygenated Blood on the Liver" (*Proc. Roy. Soc.*, vol. xxiii., p. 539, and vol. xxiv., p. 51). simply premising that in a previous communication he had shown that "division of certain parts of the sympathetic system occasioned the presence of sugar in the urine."

Here Pavy first condemns, on the basis of experiment, Schiff's hypothesis, in which the escape of sugar from the liver, and thence the production of glycosuria, are supposed to be caused by the development of a ferment in the blood flowing to the liver, as a result of the hyperæmia which succeeds those operations on the nervous system which give rise to artificial diabetes.

Pavy next treats of the influence of arterial blood upon the liver, and demonstrates that sugar is developed in the circulation. In three experiments upon dogs defibrinated arterial blood was injected in the mesenteric vein, successful results being obtained in all instances where the amount of sugar in the urine was from 10 to 15 grains per fluid ounce. Having next shown that defibrinated *venous* blood failed to give these results, he explains the production of glycosuria through influences on the nervous system as due to a vaso-motor paralysis affecting the vessels of the chylipoietic viscera, by which means the blood reaches the portal system without having been dearterialised.

Pavy then supports his conclusions by causing animals to breathe oxygen, and he states that in *several* experi-

ments upon dogs saccharine urine was developed, while in the others no success was attained. In the case of frogs many experiments gave negative results, while some gave distinctly affirmative results. He also finds that artificial respiration with *air* is sufficient to cause glycosuria, an observation previously made—as indeed Pavy points out—by Tieffenbach.

Now Dr. Richardson had already alleged a similar result to be effected by the inhalation of carbonic oxide, and Pavy follows this with a parallel in the use of puff-ball smoke, which has been stated to owe its influence to the presence of carbonic oxide. This, however, is very doubtful, for the physiological effects of pure carbonic oxide and puff-ball smoke are vastly different in many respects.

Having thus shown that “the passage of oxygenated blood through the liver excites the transformation of its amyloid substance into sugar,” he proceeds to attempt an explanation, and confesses that while he looks upon the production of sugar as only one part of the event occurring, he *thinks* the effect is due to the action of the oxidised blood as a ferment, and not to the direct influence of oxygen; and seeing that carbonic oxide combines with hæmoglobin (hematocrystalline), he reasons by analogy to the same conclusion as regards the way in which it produces glycosuria.

Pavy observed that the decoction of the liver, which is ordinarily milky from the presence of amyloid substance, became, under the influences described, nearly or quite clear, showing that this substance had disappeared, and passing on from this he adduces evidence from the observations of Dr. M. Foster, Bernard, and himself, that in several forms of life (Entozoa, larvæ of flies, solidified lung of pneumonia) amyloid substance accumulates in certain animal structures which have this feature in common with the liver, namely, a limited supply of oxygen.

While we desire to express our admiration of Pavy's elaborate researches, perhaps we may be pardoned for venturing a few criticisms on his conclusions.

Pavy is credited with having shown that temporary glycosuria can be induced by *impeding* respiration. Now, unless it can be shown that in such an instance blood reaches the liver in an arterial condition, such a result would be directly opposed to the experiments he has more recently performed and irreconcilable with the conclusions drawn therefrom. Moreover, Pavy's *explanation* of glycosuria is, we think, rendered somewhat doubtful by the fact that carbonic oxide acts like oxygen; for if oxygen gives rise to a ferment so also must carbonic oxide, and the two cannot be identical, hence the production of glycosuria must be brought about in a different way in each instance, and, moreover, such cases of artificial diabetes must differ very much from cases of natural diabetes.

We would further suggest that Pavy's conclusions can be experimentally criticised as follows:—If glycosuria be due to the transformation of amyloid substance into sugar, through the agency of a ferment present in oxidised blood, then if this dextrin-like body be isolated (as it can be), and treated with arterial blood out of the body, it should give rise to the formation of sugar. The same is to be said of carbonic oxide. The results of such experiments the writer hopes to be shortly in the position to publish.

This much seems to be certain, that glycosuria is not the result of the direct action of oxygen, for otherwise it would be difficult to explain how carbonic oxide should cause identical results. Moreover, it is not easy to see how a body of the formula $C_6H_{10}O_5$ can become converted into sugar, $C_6H_{12}O_6$, by an act of oxidation. Such a conversion can only come about by fixing a molecule of water, and this can be done through the agency of ferments, or an acid, or an alkali. Pavy concludes his last paper with these words:—“I consider that another link has been added to the chain of evidence against the glycogenic theory, which I have never wavered in regarding as untenable since the communication of my former researches to the Royal Society.”

Here Pavy takes, we think, a perfectly justifiable position so far as regards that part of the glycogenic theory which states that in health the amyloid substance in the liver is transformed into sugar and thrown into the circulation, where it becomes oxidised; but until evidence touching the points we have raised is forthcoming it is difficult to accept Pavy's explanation of glycosuria, while, as regards the natural diabetes, his researches leave us in our old position, in which there is “neither a plausible theory nor a rational treatment of diabetes.”

In concluding this contribution to the subject of diabetes we would make a few general comments.

Organic diseases affecting the brain and spinal cord, external injuries to the brain, and certain influences on the sympathetic nervous system, are known often to precede diabetes, and perhaps to lead to it, and these observations, supplemented by our knowledge of Bernard's famous experiments, and the skilful ones of Pavy, would seem to indicate that diabetes is a factor concerned in the chemical functions of the blood as governed solely by the nervous system. It becomes, therefore, especially valuable to investigate the brain and other parts of the nervous system in cases of death of diabetic subjects.

Finally, we would make a few remarks on a certain treatment of diabetes recommended and practised by Dr. Day, of Geelong. He has applied peroxide of hydrogen in cases of diabetes, as well as gout and rheumatism, believing all these diseases to be results of imperfect oxidation of the blood. He states that this treatment is efficacious through the oxidation of sugar effected, although he finds it to be successful only to the extent of giving relief and stopping the constant excessive passage of urine, while it fails to eradicate the causes of the diseases. Now I am not at all prepared to say that in the blood sugar cannot be oxidised by peroxide of hydrogen, but it is certainly impossible in the case of urine, for in an experiment the writer conducted it was found that the amount of sugar present in a sample of diabetic urine was the same after as it was before treatment with a great excess of this reagent. But there is this difference, that when blood is treated with peroxide of hydrogen nascent oxygen is immediately set free, but not so with urine. But notwithstanding this it is at least evident that Dr. Day's explanation of the good results attending the use of peroxide of hydrogen is faulty, for assuming for the moment that peroxide of hydrogen can oxidise the sugar present in diabetic blood, the quantity administered is not, nor can be in any sense, equivalent to the amount of sugar.

FOOTNOTE TO THE PRECEDING PAPER.

In the above article the writer has spoken of “Diabetes Mellitus” as if it were a perfectly defined disease which reveals itself always according to a fixed state of things. But this has been done only for the sake of convenience in comparing it with “glycosuria.” The natural disease seems to exist in many forms: these forms may or may not be related to the same original causes.—C. T. K.

Pathological Laboratory,
68, Earl's Court Road, Kensington, W.
April, 1876.

ON SOME REACTIONS OF IODINE AND PALLADIUM CHLORIDE WITH POTASSIUM FERROCYANIDE.

By SERGIUS KERN, St. Petersburg.

IODINE in the form of an alcoholic solution was added to an aqueous solution of potassium ferrocyanide (K_4FeCy_6); it dissolved in the liquor, which retained its original yellowish colour. Palladium chloride was next added, in order to separate the iodide of palladium in case the iodine occurred in the liquor in free state, but no precipitate of PdI_2 was obtained; the solution meanwhile turned brownish, and very quickly assumed a green tint. This

reaction shows the unfitness of palladium chloride in quantitative analysis for the separation of iodine from bromine and chlorine, because, if potassium ferrocyanide is present in the solution, faulty results may be obtained, as in this case iodine is not precipitated in the form of palladium iodide. The resulting greenish solution, on being boiled, yielded a solution of a beautiful dark green colour, which is palladium ferrocyanide (PdFeCy_6).

It was remarked during the experiments that iodine in lumps easily dissolves in a boiling solution of potassium ferrocyanide, the colour of which remains yellow.

In the green solution of palladium ferrocyanide obtained as mentioned above, the iodine dissolved in potassium ferrocyanide was not detected by starch; thus iodine must be supposed to be chemically combined with potassium ferrocyanide, and is, as I suppose, a molecular combination.

In the formation of palladium ferrocyanide iodine has no action, because the same green solution of this salt is obtained by adding palladium chloride to a solution of potassium ferrocyanide free from iodine.

Potassium hydroxide in the form of an aqueous solution, without being heated, rapidly dissolves the palladium ferrocyanide, yielding a yellow solution, containing free K_4FeCy_6 .

It must be mentioned that bromine easily dissolves in a solution of potassium ferrocyanide, forming a molecular combination.

In a solution of potassium ferricyanide (K_3FeCy_6) iodine also dissolves, yielding a reddish yellow liquor: in this solution palladium chloride does not give the palladium iodide, and does not yield the green palladium ferrocyanide; the liquor remains reddish yellow; potassium hydroxide colours this solution red.

These experiments prove that palladium salts must be very carefully used in analyses for the detection of iodine, because in presence of potassium ferrocyanide or potassium ferricyanide the iodine is not detected, and cannot be separated from bromine or chlorine by palladium chloride. So as gold salts give, with ferrocyanide of potassium, also a green colouration, the K_4FeCy_6 as a reagent may give faulty results, as it was remarked that palladium salts give the same colouration.

I conclude my paper by observing that if palladium salts are used as a reagent for iodine, the preliminary analysis must be very carefully executed, in order to be quite convinced of the absence of double ferrocyanates of potassium and other cyanides. As I showed in my paper in the CHEMICAL NEWS (vol. xxxii., page 242) that in presence of alkaline sulpho-cyanides (KCNS , NH_4CNS , &c.) iodine is not precipitated by palladium nitrate or chloride.

A SIMPLE METHOD OF "NESSLERISING."

By OTTO HEHNER.

THE modification of Wanklyn's method of water analysis, or rather of the process of "Nesslerising," proposed by Mr. H. B. Cornwall, and described in the CHEM. NEWS (vol. xxxiii., p. 135), is far too lengthy and objectionable to be likely to supersede the process described by Wanklyn in his book on "Water."

I have, for some years past, been in the habit of employing a modification of the process of Nesslerising, much easier, I believe, shorter, surer, and less troublesome than the modification alluded to.

Two glass cylinders, capable of holding about 110 c.c., are graduated from below upwards, from 5 to 5 c.c., up to 100 c.c. The divisions must be equal throughout the whole length of the cylinder, ensuring perfect uniformity in the shape of the vessel. A glass tap is fused at about the division 30.

Cylinder A is filled to the 100 division with the distillate, and 2 c.c. of Nessler's reagent are added. The liquid therefore reaches to 102. Into the second cylinder as

many c.c. of standard ammonia solution are run as will be deemed requisite to give a colouration equal to that yielded by the distillate, pure distilled water is used to fill up to the 100 mark, and 2 c.c. of Nessler, thus making the level of the liquid also in this cylinder B to reach 102.

The usual time is allowed to elapse before the tints are compared. Supposing the liquids to be of different depths of colour, the glass tap of the cylinder which contains the darker liquid is opened, and some of the liquid is allowed to run out quite slowly, while the observer looks down the tubes in the direction of the axis, towards a sheet of white paper. The colours will get more and more of the same shade and tint, till at last no more difference can be observed. The glass tap is now closed, and the division is read off. It is easy to calculate now the quantity of ammonia actually present in the distillate.

Supposing cylinder A, containing the distillate, appear the darker, and into B 5 c.c. of standard ammonia have been run, the liquid in both having been made to reach 102 as described, and, after equalisation of colours, the height of liquid in A was 76. We have then—

$$76 : 5 = 102 : x; x = 6.71 \text{ c.c.}$$

Supposing, on the other hand, that too great a quantity of standard solution has been put into B, and the level of the liquid in that cylinder, after equalisation, be 76: then we have—

$$102 : 5 = 76 : x; x = 3.72 \text{ c.c.}$$

If the distillate amount altogether to 163 c.c., then the whole of it contains as much ammonia as corresponds to—

$$3.72 \times 1.63 \text{ c.c.} = 6.06 \text{ c.c.}$$

One single experiment thus gives accurately the quantity of ammonia. There is no difficulty whatever in noticing the right point, and, since the observer looks straight down the tube (not, as Frankland recommends, obliquely), the sinking of the level, whilst the tap is open, cannot be noticed at all, and no inconvenience can arise from the different height of the columns of liquid in the two cylinders.

Of course, after the right number has thus been found, a supplementary experiment may be made, adding the correct volume of standard ammonia to the pure distilled water in B; but in all cases the figures will agree within 1-10th c.c.

These graduated cylinders with glass taps may be procured, I believe, from Messrs. Townson and Mercer, Bishopsgate Street, London. Care should be taken to have them perfectly cylindrical, in order to ensure equal value of the different divisions.

The same method as described may be used in all other chlorimetric estimations, as of lead and iron in water, of copper, &c.

AN ESTIMATION OF THE FREE AND ALBUMINOID AMMONIA YIELDED BY THE STAGNANT WATERS OF THE DUBLIN STREETS, AS COMPARED WITH THE QUANTITIES OF THOSE SUBSTANCES OBTAINED FROM THE LIFFEY WATER, AFTER RECEIVING THE SEWAGE.*

By LANCELOT STUDDERT, LL.D., Ex-S. T.C.D.

DURING the Session of 1874-5 of the Royal College of Science, Stephen's Green, several examinations were made there of Dublin well-waters: and also some determinations like those that follow for the river water. As a sequel to those analyses, the suggestion of the Professor of Chemistry in that college, Mr. Galloway, induced me to undertake a series of estimations of the ammonia yielded by the surface-water of some streets and squares

* Read before the Royal Irish Academy, January 10, 1876.

in Dublin, taking as the standard of comparison the water of the Liffey, near where the sewage is discharged into the river.

The results of my examination, conducted during November and December last, in the College of Science Laboratory, are now laid before the Royal Irish Academy.

Altogether twenty-nine of these street-waters were examined; the samples dealt with were collected in my presence at the times and places stated in the Table appended to this paper; the mud, also, left from some of these pools was examined for ammonia, which reached two parts in the hundred, calculated after allowing for moisture expelled at 212° F. The river water was collected at intervals during the two months from four different places, namely at Eden Quay, Aston's Quay, Burgh Quay, and Sir John Rogerson's Quay, four hours after high water at Dublin bar.

The method employed for determining the quantity of ammonia yielded by these waters and muds is that devised by Messrs. Wanklyn and Chapman. This process is almost universally allowed to be the best yet made known for ascertaining the character of the nitrogenous matter in

waters; its quantitative results are accurate, and they are obtained with rapidity. It may be well to state for the information of any unacquainted with the Wanklyn and Chapman process, that under the term "*free ammonia*," those chemists include ammonia, not only present *as such*, or in combination with acids, but also the ammonia that, after adding a saturated soda carbonate solution, is evolved by distillation from urea, or other easily decomposable nitrogenous organic bodies. The term *free ammonia* is therefore not strictly correct; but, taking it in this special sense, it would be difficult to substitute any other term more convenient or less open to objection.

The Table of results gives the figures for the Liffey standard at the head of the list. The quantities of the free and the albuminoid ammonia obtained from the several street and Liffey waters are calculated as grains in the gallon, and also as milligrms. in the litre of each water, respectively examined.

The average of free ammonia from the four samples of the river is 0.0982, or under $\frac{1}{10}$ of a grain in the gallon; the average of albuminoid ammonia from the same is 0.0779, or under $\frac{1}{12}$ of a grain in the gallon.

Date of Water Collection.	Place of Water Collection.	Free Ammonia.		Albuminoid Ammonia.	
		Grains per Gallon.	Milligrms. per Litre.	Grains per Gallon.	Milligrms. per Litre.
1875.	LIFFEY STANDARD.				
November 11	Eden Quay	0.0840	1.20	0.0980	1.40
" 29	Aston's Quay	0.0812	1.16	0.0910	1.30
December 15	Burgh Quay	0.1750	2.50	0.0875	1.25
" 17	Sir J. Rogerson's Quay	0.0525	0.75	0.0350	0.50
Total		0.3927	Total	0.3115	
Average		0.0982	Average	0.0779	
November 1	Stephen's Green, N.	0.1260	1.80	0.2380	3.40
" 2	" E.	0.0700	1.00	0.4200	6.00
" 3	" S.	0.0700	1.00	0.3780	5.40
" 5	" W.	0.3150	4.50	0.4900	7.00
" 15	Stephen's Street, Lower	12.6000	180.00	5.8000	80.00
" 17	Cross Kevin Street	2.8000	40.00	1.4000	20.00
" 18	Patrick Street	4.9000	70.00	1.7500	25.00
" 19	Townsend Street	4.5500	65.00	4.9000	70.00
" 20	Peter Place, at corner on Adelaide Road	5.6000	80.00	10.1500	145.00
" 30	Baggot Street, Lower	0.7000	10.00	0.8000	12.00
December 1	Duke Lane	1.1900	17.00	1.1900	17.00
" 1	Lemon St. (late Little Grafton St.)	9.1000	130.00	2.0300	29.00
" 2	Leeson Street, Lower	1.5400	22.00	0.9100	13.00
" 2	Leeson Lane	10.5000	150.00	2.3800	34.00
" 7	Creighton Street	1.5400	22.00	0.9800	14.00
" 7	Sandwith Street	1.8200	26.00	1.4000	20.00
" 7	Boyne Street	4.2000	60.00	2.1100	31.00
" 8	Abbey Street, Mid.	3.7800	54.00	1.8200	26.00
" 10	Lee's Lane, Aston's Quay	98.0000	1400.00	11.2000	160.00
" 10	Sir J. Rogerson's Quay, Gutter ..	70.0000	1000.00	7.0000	100.00
" 10	Moss Street	105.0000	1500.00	10.0000	145.00
" 10	Poolbeg Street	105.0000	1500.00	7.0000	100.00
" 14	Peterson Lane	9.8000	140.00	0.9800	14.00
" 15	Frederick Lane, S.	7.0000	100.00	1.8200	26.00
" 17	New Street	13.5000	150.00	3.5000	50.00
" 20	Fitzwilliam Square, W.	0.4900	7.00	0.3500	5.00
" 20	" E.	17.5000	250.00	1.7500	25.00
" 20	Merrion Square, N.	0.3500	5.00	0.5250	7.50
" 20	" S.	0.4200	6.00	0.2800	4.00
Total		492.8610	Total	83.6460	

Average 17 grs. of free ammonia per gal. of water (surface). Average 3 grs. of albuminoid ammonia per gal. of water (surface).

Mud dried at 212° F. Percentage.

	Free Ammonia.	Albuminoid Ammonia.
Peter Place Corner	1.2857	0.6163 = 2 per cent
Lower Stephen Street	0.3780	0.1001
Boyne Street	0.4861	0.3640

It may be interesting to note that the examination of the river water referred to as having been made by other workers in the College of Science Laboratory, in 1874, gave a result equal to my average in 1875; thus showing a remarkable constancy in the state of the Liffey.

It may also be remarked in passing that my average for free ammonia is less, but for albuminoid ammonia is greater, than the average Messrs. Wanklyn and Chapman reported as that of the Thames at London Bridge, in June, 1867: that river, the tide being at two hours flood, yielding free ammonia = 0.1232 of a grain per gallon; and albuminoid ammonia = 0.0245 of a grain per gallon.

The average of free ammonia obtained from the 29 street-waters is 17 grains to the gallon, that is, over 170 times the like average from the river. The average of albuminoid ammonia from the street-water is 3 grains to the gallon, or 38 times the Liffey average.

It will be seen by the Table, that from three out of the four river samples, the quantity of free ammonia was under that yielded by any of the street-waters, except at Stephen's Green, East and South.

The maximum of free ammonia from the river was at Burgh Quay, and only reached 0.175, or less than $\frac{1}{5}$ of a grain to the gallon; whilst the maximum of free ammonia from the street-waters, namely, at Moss Street and Poolbeg Street, was 105 grains to the gallon, that is, exactly 600 times greater than the river maximum.

The least impure of the 29 street-waters yielded nearly three times more albuminoid ammonia than the most impure sample of the river water; for instance, the surface-water at Merrion Square South being the best of the street-waters, yielded 0.280 of a grain against that obtained from the river at its worst, namely, 0.098 of a grain, or nearly 3 to 1.

But the bad pre-eminence of the water in Moss Street and at Peter Place (corner in Adelaide Road), and in Lee's Lane, off Aston's Quay, namely, 10 and 10.15 and 11.2 grains of albuminoid ammonia from one gallon of each water, respectively, is more than 100 times greater than the Liffey maximum.

Messrs. Wanklyn and Chapman conclude from a wide induction of experiments that "the disintegrating *animal refuse* in the river [Thames] would be pretty fairly measured by *ten times* the albuminoid ammonia which it yields." In this way, the average of such refuse in the Liffey is 0.779, or just $\frac{3}{4}$ of a grain in the gallon; whilst the average of such refuse in the street-waters is 29 grains to the gallon.

That much of this enormous amount of animal matter thus in our midst must, if not rapidly removed, take forms that will vapourise, seems all but certain, since the conditions for spontaneous decomposition may be said to be always present; there are the moisture and heat required for this chemical change, and then there occurs at intervals the drying up of these stagnant pools.

My examination of these street-waters found, as might be expected, sulphuretted hydrogen, with other sulphides and very offensive volatile substances.

What the effect must be on the people's health who dwell in an atmosphere contaminated by exhalations such as these, it is not for me to determine; this paper simply records the facts of the case, leaving conclusions to those physicians who make such researches their peculiar study. But without knowing the least of the little that is known, even to the medical faculty, about either the chemical or the germ-theory as to the propagation of disease, yet one of the unlearned, like myself, having but ordinary sagacity, might correctly conclude that the continued presence of so much dirt in the streets would go far to account for the high death-rate (33 to the 1000 yearly) lately recorded for Dublin, a city whose situation, other things being equal, might mark it out as one of the healthiest in the Empire. The *London Times*, reviewing "Ireland at the close of 1875," laid this to our charge—that "dirt reigns, and slays its thousands in Dublin and elsewhere."

Whatever is to be done with our street sewage, whether it is still to defile the natural purity of the river, or to be applied to improve the land, or only to be thrown away, with great cost, into the sea; whatever be the destination of this noxious mass, whether it is to be good, bad, or indifferent, it certainly appears, from the results now laid before the Academy, that better scavenging and a level surface for the streets is at once required.

The Professor of Hygiene and Public Health in University College, London (Dr. Corfield), in reference to this subject, in the "Manual of Public Health," edited by Hart, states that:—"If the streets, roads, and ways of a town or district are allowed to become or to remain so out of repair as to become receptacles for filth, or to afford, by their inequalities, depressions in which foul water accumulates, it is in vain to look for beneficial results from other sanitary measures."

REPORT

ON THE

DEVELOPMENT OF THE CHEMICAL ARTS DURING THE LAST TEN YEARS.*

By Dr. A. W. HOFMANN.

Progress in the Artificial Production of Cold and Ice.

By Dr. H. MEIDINGER.

(Continued from p. 177.)

ACCURATELY regarding the various stages traversed by the air, the arrangement of an air ice machine would be, in principle, as follows:—The air is condensed in an especial cylinder up to a certain pressure, at which it is then forced into the cooling apparatus. Whilst it here parts with its excess of heat, its volume, at the same pressure, becomes reduced in the proportion—

$$\frac{273+t}{273+T}.$$

Hence it passes into a second cylinder where expansion takes place; the processes taking place here in the reversed order from what ensues in the compression cylinder, and the effect agrees exactly with that of an expansion steam-engine. The air here becomes very cold and is forced by the return of the piston into the freezing chamber where the ice boxes stand. After passing through this apparatus it arrives anew in the compression-cylinder to repeat the same circuit. The expansion-cylinder here corresponds to the evaporation-receiver in other machines. The distinction, however, must be noted that but a small quantity of air is kept in circulation, whilst in other systems a large stock of the matter inducing the cold is present in the state of a liquid. It will be seen that the course of the conversions is exactly the same as in a "caloric engine," but in a reversed direction, and the performance of the one and the other may be calculated by the aid of the same formulæ. The writer has carried out such a calculation,† from which, it appears, that when the air, at an initial temperature of 20° C., is compressed to 3 atmospheres and then cooled down to 30°; the theoretical yield is 5 kilos. of ice per 1 kilo. coal consumed, whilst at 2 atmospheres the yield is 6 kilos. The production is in general terms inversely as the condensation of the air or the difference of temperature thereby produced. But, on the other hand, the dimensions of the cylinders for a given yield must be so much the larger the smaller the condensation which is to be applied, as appears at once on a close examination of the procedure. The actual performance of the machine may perhaps be considered equal to one-half of the theoretical yield. Hence it appears that the air machine is far

* "Berichte über die Entwicklung der Chemischen Industrie während des Letzten Jahrzehends."

† *Bad. Gewerbezt.*, 1869, Appendix Number.

inferior in its performance to the ammonia machine.* The reasons are the same which have been already advanced in the comparison of the ammonia and the ether machine. The efficacy of the machine may, however, be considerably increased, if, as we shall further explain below, the air is at once cooled during compression, so that it cannot become heated, in which case the cost of compression is much reduced. An advantage of the air machine as compared with other systems is that no offensive or combustible substances are brought into play, and that there can occur no waste of a costly material.

An air machine is mentioned for the first time in 1863.† It was patented in England in April, 1862, by A. C. Kirk, of Bathgate. It consists of upright cylinders, the lower part of each being connected with the upper part of the other by a channel, fitted with a valve opening upwards. The pistons have valves opening downwards. The lower covers of the cylinders are kept cold by a stream of water, whilst the upper give off cold to salt water. According to the somewhat obscure description the action is as follows:—The piston of the cylinder *a* on descending compresses the air below it, and expands that above it, the compressed air being forced into the upper part of the cylinder *b*. On the ascent of the piston *a* the expanded air passes through the valve of the piston from the upper into the lower part of the cylinder, whilst the piston receives above at first compressed air from the lower part of the cylinder *b*, which, when the latter is emptied, begins to expand and to be cooled. The same processes take place in the cylinder *b*. Consequently one and the same quantity of air is always employed, which circulates from one cylinder to the other. It is asserted that 1 horse-power yields, in twenty-four hours, 106 kilos. of ice, the yield of the ether machine being 110.5 kilos., = 2 kilos. ice per kilo. of coal. In Young's paraffin works at Bathgate there was at that time a machine which turned out in twenty-four hours 2 tons or 2032 kilos. of ice. The result is somewhat small; the cooling surfaces of the cylinders are certainly not large enough to take up heat and cold quickly and completely. Indeed a series of theoretical objections might be urged against the construction of the machine, which is very simple. In 1864 it was announced that this machine was still at work in Young's establishment, producing a ton of ice with the consumption of a ton of coal, worth (then) four shillings. It was also declared that its efficacy was equal to that of the ether machine.‡ This would be a far smaller yield.

(To be continued)

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Friday, April 28th, 1876.

Professor ABEL, F.R.S., President, in the Chair.

At this meeting (which was a special one) Professor ANDREWS, F.R.S., delivered a lecture "On Certain Methods

of Physico-Chemical Research." After a few preliminary remarks, in which he observed that the address must necessarily be of a somewhat discursive nature, he exhibited and described the apparatus employed by him many years ago for determining the heat developed during the combination of gases. It consisted of a thin copper cylinder, into which the mixture of hydrogen with excess of oxygen was introduced, closed by a screw top, with an apparatus attached for igniting a fine platinum wire in the interior of the vessel by means of an electric current. This was placed in a calorimeter, and the whole in a copper box, where it could be rotated so as to equalise the temperature of the water. After the rotation the temperature of the calorimeter was taken, the explosion effected, the apparatus again rotated, and the temperature read a second time, giving, after the necessary corrections had been made, the heat developed by the union of the known quantity of hydrogen with water in the proportion to form water. For the combustion in oxygen of solids, such as sulphur or carbon, a somewhat similar but larger apparatus was employed. The only experiments yet made to ascertain the heat developed by the direct combination of chlorine, bromine, and iodine had been made by him. He had at first experienced some difficulty in the case of potassium, as no glass vessels would resist the heat developed by its union with chlorine. This difficulty had been overcome by using a brass vessel, for he had found that dry chlorine was without action on both copper and brass. He also stated that in determining the heat developed by the mixing of liquids the only method of obtaining accurate results was to float a thin glass or platinum capsule containing one of the liquids on the surface of the other, and then by means of a fine pair of forceps pour out the contents of the capsule. Thomsen's results, obtained by another method, although not absolutely correct, were more accurate than those of Favre and Silbermann. Although frictional electricity was competent to decompose potassium iodide, yet it had been found that on passing the current by means of platinum electrodes through acidulated water no trace of gas was evolved: the wires became polarised, however, from which it might be inferred that the water was actually decomposed. It had seemed to him that the non-appearance of gas at the poles was due to its being dissolved in the large bulk of the liquid used, and he had consequently devised a simple arrangement by which this inconvenience might be avoided. Long platinum wires to serve as electrodes are fused into the ends of a couple of thermometer tubes, which are then filled with acidulated water by the simple expedient of boiling them for some time in the liquid. They are then inverted in a vessel of the same acidulated water, and a current of electricity from an electrical kite passed through by means of the platinum wires. In this way it was found that the water was quickly decomposed, and owing to the great tension of the electricity fifty or sixty of these couples could be arranged in series without any sensible diminution in the rate of decomposition, whilst it was well known that two or three, when introduced into the circuit of an ordinary battery, greatly enfeebled if it did not entirely stop the current. If one of these tubes filled with oxygen were placed over a solution of potassium iodide, and the silent discharge passed, the whole of the gas would be absorbed in about a minute. He desired particularly to draw attention to these tubes as affording a facile mode of experimenting on the action of the electric current on gases and liquids. He also exhibited the tubes employed by himself and Prof. Tait in their experiments to demonstrate the diminution in volume which oxygen undergoes when converted into ozone. They consist of a wide tube filled with oxygen, and furnished with platinum wires, which is connected with a small U-tube containing concentrated sulphuric acid to serve as a gauge. After the oxygen had been measured at a constant temperature the point of the gauge was sealed, the discharge passed, and the apparatus brought to the original temperature. On now breaking off the sealed point of the gauge the

* In consequence of the low specific heat of the air, relatively large quantities must be employed, whence the cylinders and the resistance of friction to be overcome are very large.

† *Pract. Mech. Journ.*, 1863, 113. *Dingl. Pol. Journ.*, clxx., 241. Wagner, *Jahresbericht*, 1863, 568. However, a patent for an air ice machine was granted in England to one Nesmond, of Bellac, in France, as early as 1852. It compressed air to 20 atmospheres by means of a hand air-pump in a vessel like a boiler placed in cold water. After cooling the air passed into a second vessel where were the substances to be cooled or the air to be frozen, and escaped thence into the open air. It was asserted that a man could force the air into the compression-vessel in eight minutes, and thus produce 8 to 10 lbs. ice per hour. The action of the apparatus was therefore intermittent and not economical, and indeed the whole arrangement left much to be desired in point of convenience.

‡ *Mech. Mag.*, 1864, 245. *Dingl. Pol. Journ.*, lxxiv.

diminution of volume could readily be observed; and if again sealed and heated to 300°C. , and the volume again observed with similar precautions, it was found to be what it was originally, the ozone having been reconverted into oxygen. An apparatus on the same principle, but on a larger scale, adapted for class experiment, was shown in action. It consists merely of a Siemens's induction tube filled with oxygen, and connected with a narrow tube, the end of which dips into concentrated sulphuric acid, so as to serve as a gauge to measure any alteration in volume. As soon as the temperature of the apparatus has become constant, and the column of sulphuric acid in the gauge-tube is stationary, the silent discharge from an induction coil is passed. The first effect, from the elevation of temperature, is to expand the gas and cause the column in the gauge to sink. After a time, however, the column begins to rise, showing the contraction in volume of the oxygen produced by the conversion of a portion of it into ozone. It seemed unlikely that this property of the silent discharge of altering the density of oxygen should be confined to that element, and he had tried its effect on nitrogen and chlorine, but with negative results. He had hoped for something different in the case of chlorine, for it was a curious fact that although platinum might be left for years in contact with chlorine without producing any effect on the metal, yet it was immediately attacked when an electric current was passed. The Lecturer then passed on to his apparatus for determining the latent heat of vapours, pointing out the great advantage of having it of small size, since the experimenter was thereby enabled to work with small quantities of material, for they all knew how extremely difficult it was to obtain liquids which were perfectly pure in quantity. He was in the habit of passing the vapour of the substance directly from the vessel in which it was dried over calcium chloride, so as to avoid any chance introduction of a trace of water, a matter of the utmost importance when the high specific heat of water is considered: 1 per cent of water in a liquid would often cause an error of 10 per cent in the latent heat. The determination of the latent heat of vapour was a very large field for chemists to work in, as at present scarcely anything had been done in it. After some remarks on the construction of graduating engines, in which he recommended the use of a very short screw, it being impossible to obtain a long one which was perfect, he proceeded to describe the construction and methods of working with his apparatus for observations on the behaviour of gases under great pressures. The thermometer-tubes employed in these experiments are made of a special kind of glass, and joined at one extremity to a wider tube, which is cut off and ground at the end. A slight swelling is made in the thermometer-tube towards the lower end to serve as a shoulder, on to which and for some distance down the tube shoemaker's thread is wound and covered with cobbler's wax, so that when firmly pressed into the perforated gun-metal cover of the pressure apparatus it forms a perfectly tight joint. The lower end of the tube dips into a glass vessel containing mercury, and the upper one, after the gas has been introduced, is carefully sealed. The accurately graduated and calibrated tubes are filled by passing a current of the pure dry gas, carbonic anhydride for instance, through them for some hours. The end is then carefully sealed, so as to cause the bore to be as perfectly conical as possible. This is a matter of considerable difficulty, but may generally be effected by keeping the tube vertical, and rotating it slowly before the blowpipe flame. The glass tube containing the mercury, and into which the lower end of this pressure-tube is plunged, is introduced into the gun-metal apparatus for communicating the pressure, and which is filled with water, the whole being screwed up tightly. The desired pressure is obtained by means of screws at the bottom of the apparatus, which being screwed into the water diminish the capacity of the vessel, the pressure thus produced being transmitted to the gas in the pressure-tubes through the mercury. In order to make these screws quite tight

under the enormous pressures employed, sometimes reaching 500 atmospheres, they pass through washers consisting of a pile of discs of perforated leather, which have been saturated with grease by soaking them *in vacuo* in melted lard. In this manner the apparatus was still perfectly tight even after the lapse of two or three years. He might mention that he had an apparatus made of iron, in which the pressure was communicated to the gas entirely by means of mercury, but he never succeeded in getting it to remain perfectly tight for any length of time. The graduated portions of the thermometer-tubes projecting from the apparatus were surrounded by a suitable arrangement for keeping them at a constant temperature by means of a current of water, or steam, or the vapour of some other liquid. When employing steam he had at times encountered some difficulty from drops of water condensing on the graduated tubes, and thus interfering with the readings; these, however, could readily be removed by pouring in boiling water, which washed the tubes thoroughly. He had found great difficulty in employing the vapours of liquids other than water, owing to the impossibility of obtaining them in any quantity in a pure state so that they would boil at a constant temperature. He also described the means by which he had ascertained that mercury did not absorb either air or carbonic anhydride in the slightest degree, and mentioned that although under very high pressures the capacity of the tubes was slightly altered, such alteration was not permanent, and then proceeded to give a brief abstract of the results which he had just laid before the Royal Society at the Bakerian Lecture on the natural gaseous states of matter, and the way in which they differ from a theoretically perfect gas: the product of the pressure into the volume being invariably less than unity, whilst with a perfect gas it would be unity. It is thus shown that gases condense more than they would if Boyle's law were correct. After some remarks on the air-manometer, which he said was an almost perfect instrument up to 200 atmospheres, the Lecturer concluded amidst great applause.

The PRESIDENT then in a short speech expressed the thanks which they owed to Prof. Andrews for his most interesting lecture.

The Lecturer afterwards exhibited the striking experiment of the action of heat on liquid sulphurous anhydride in causing it to pass into that curious "intermediate state" in which it is neither liquid nor gaseous.

PHYSICAL SOCIETY.

April 29th, 1876.

Prof. GLADSTONE, F.R.S., Vice-President, in the Chair.

THE following gentlemen were elected Members of the Society:—Prof. F. Fuller, M.A., and Capt. E. H. White.

The SECRETARY read a communication from Sir John Conroy, Bart., "*On a Simple Form of Heliostat*." The defect of Fahrenheit's heliostat, in which the beam of sunlight is reflected by a mirror moved by clockwork in a direction parallel to the axis of the earth, and then in the required direction by a fixed mirror, consists in the great loss of light. The author substitutes two silvered mirrors for the looking-glasses usually employed, and he has shown that the loss of light with this arrangement is less than when the light is once reflected from a looking-glass.

Mr. S. P. THOMPSON, B.A., B.Sc., then made a second communication "*On the so-called 'Etheric Force,'*" and described some experiments which he has recently made in the Physical Laboratory at South Kensington on the subject. The name was given by Mr. Edison—the inventor of the motograph—to the sparks obtained when a conductor is presented to the core of an electro-magnet, the coils of which are traversed by an intermittent current. The results of the experiments conducted as originally described not proving satisfactory, various other arrange-

ments were tried, and it was found that if the secondary current from an induction coil be used instead of a current direct from the battery the effects are much more marked. When the induced spark was thus diverted, either wholly or partially, into a short coil which was insulated very perfectly from the core inside, a spark about half an inch in length, which has a decided effect on the nerves, could be drawn off from the core, and this was sufficient to illuminate a small vacuum tube; the spark, however, does not exhibit the usual signs of polarity. It was shown by observing the illumination thus produced with a rotating mirror, that the discharge is in reality a reciprocating one, each spark returning on its path after a minute interval of time. Under certain conditions it is also possible to charge an electroscope either positively or negatively by means of the spark, and Mr. Thompson has shown that the spark ignites a jet of gas, but fails to deflagrate metallic wire or ignite gunpowder. From the above and other experiments, which will be exhibited on a future occasion, the author concludes that the cause of the phenomena is obvious, and that the hypothesis of a new force is unnecessary.

Prof. McLEOD referred to a paper on the same subject, which appeared in the *CHEMICAL NEWS* (vol. xxxiii., p. 173), by Professors Houston and Thomson.

Mr. DAVID ROSS, B.A., enquired the tension of the Leyden jar arrangement used in the experiments, but Mr. Thompson pointed out that it would be very difficult of determination on account of the rapid change of the spark from positive to negative.

NOTICES OF BOOKS.

The Journal of the Iron and Steel Institute. No. 2, 1875
London: E. and F. N. Spon.

THIS issue contains an account of proceedings at the meeting of the Institute at Manchester, in September last.

Of the papers read one only seems to come fairly within our cognizance, namely, an essay by Mr. I. L. Bell, F.R.S., on the use of caustic lime in the blast-furnace. The author shows that of the total heat generated 22 per cent was absorbed by the expulsion of carbonic acid from the limestone, and the decomposition of this compound of oxygen and carbon. Of this loss 16 per cent is due to the use of limestone. Hence he argues that:—"An expenditure of 16 per cent of the heating power of the fuel, which is rendered necessary by the presence of one of the constituent parts of our flux affords, *prima facie*, a strong reason why we should seek to relieve the furnace of a duty represented by about $4\frac{1}{2}$ cwts. of coke, particularly as half this weight of inexpensive small coal sufficed for the purposes of the lime-kiln."

In certain experiments undertaken with furnaces 48 feet high he failed to find any tangible economy in fuel effected by the substitution of burnt lime for the raw limestone. In the same furnaces, however, he found an advantage in the increased make and superior quality of the iron when burnt lime was employed. With furnaces 80 feet in height this improvement was no longer to be traced. In the experiments undertaken with these furnaces the composition of the cinder in each case was found almost absolutely identical, and no change in the removal of silicon or sulphur from the metal was effected by the use of burnt lime.

Hence he concluded that in what he called an "imperfect furnace," *i.e.*, a furnace less than 80 feet high, there was certainly an advantage in using limestone in a calcined state. But in complete and perfect furnaces, where the economy of coke is carried as far as the chemical nature of the operation permits, there is nothing to be gained by a change.

Etudes sur le Phylloxera et sur les Sulpho-Carbonates.
Par M. DUMAS.

THIS treatise composes the seventh volume of the Fifth Series of the *Annales de Chimie et de Physique*, and embodies the labours of the special commission issued to investigate this minute but formidable enemy of the vineyards. The following are the conclusions arrived at:—As regards the phylloxera of the roots it is found that the sulpho-carbonate of potassium, of which more than 20,000 kilos. have been already used, is a rapid insecticide, the only one which certainly destroys the phylloxeras fixed upon the roots, and which affords at the same time an efficient nourishment to the vine. The sulpho-carbonate of sodium offers similar advantages as an insecticide only. The sulpho-carbonate of barium being an anhydrous salt, and sparingly soluble is recommended by its resistance to the action of oxygen and to that of carbonic acid, which renders it a poison less rapid, but of an effect more durable.

As to the winter-eggs the heavy oil of gas-tar, and especially the so-called oil of anthracen, seems to be the most suitable agent for anointing the branches and for destroying the winter-eggs. The application of gas-tar to the branches and of sulpho-carbonates to the roots is best performed in the months of February and March.

That the investigation has been carried out on truly scientific principles the name of M. Dumas is a sufficient guarantee, and the signal success which has been attained is a fresh instance of the practical value of seemingly abstract research, and of the efficiency of scientific method.

CORRESPONDENCE.

ON A NEW REACTION OF TARTARIC ACID.

To the Editor of the Chemical News.

I HAVE lately noticed the following reaction, which, besides presenting one or two rather interesting peculiarities, may, as far as I can judge at present, be proposed as a test for tartaric acid. To a very dilute solution of ferrous sulphate or chloride, a small quantity of a solution of tartaric acid or a tartrate is added, followed by a few drops of chlorine water or hydric peroxide, and lastly, excess of caustic potash or soda, when a fine violet colour is obtained.

I have tried the same experiment, using citric, succinic, malic, oxalic, or acetic acids, or sugar, in place of tartaric acid, but without getting a similar result. If a ferric salt be used instead of a ferrous salt, the colour is not obtained. The violet compound formed appears to be potassic or sodic ferrate. It is destroyed at once by sulphurous acid, and is slowly discharged by boiling.

I have tried to obtain the higher oxides of manganese and chromium in the same way, but without success. I intend to follow up the investigation more fully.—I am, &c.,

H. J. H. FENTON.

Christ's College, Cambridge, April 25, 1876.

ORGANISATION AMONG CHEMISTS.

To the Editor of the Chemical News.

SIR,—I am glad to find that chemists seem to be waking up to the fact that they ought to have a position as members of an honourable profession. There is no doubt that it is a splendid science, and I regret much at the low tone that is taken by some of your correspondents.

Connected as chemistry is so closely with physics, medicine, sanitary matters, not to speak of the arts and manufactures, it seems to me a pity that anyone should

suggest that it is sufficient qualification to pass in inorganic chemistry alone: this can be learnt in a year, and I am quite sure that no one can learn chemistry in that time so as to be any credit either to himself or the profession.

There is no doubt that at present chemists are held in very low estimation, and there can be no wonder that in this great commercial country some are found to grumble at the very poor remuneration that is attached to it. This, however, is perfectly intelligible, as so few men study and qualify for it properly.

I must beg to differ from one of your correspondents, who says that it is "those who have a sufficient private income who degrade the profession by the acceptance of wretchedly low fees." It seems to me that very few men with a "sufficient private income" take up chemistry as a profession, and I wish there were more. With regard to "organisation," I think this might be more properly referred to the highest authorities in our profession, and I am glad to say I have good reason to believe that the Council of the Chemical Society is taking some steps towards distinguishing between those who are properly qualified and those who are not.—I am, &c.,

THETA.

May 2, 1876.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Seances, de l'Academie des Sciences. No. 15, April 10, 1876.

Experimental Criticism on the Formation of Sugar in the Blood, and on the Function of Physiological Glycæmia.—M. Claude Bernard.—The author maintains that the experiments, on the faith of which earlier authors have thought themselves entitled to deny or to affirm the presence of sugar in the blood of diabetic patients, have really no scientific value. The experiments of Tiedemann, Gmelin, and Magendie are exact as crude facts, but the interpretation which connects them with an amylaceous or saccharine diet is erroneous.

Analytical Solution of the Problem of Distribution in a Magnet.—M. J. Jamin.—A strictly mathematical paper, unfit for abstraction.

Vegetation of Maize begun in an Atmosphere free from Carbonic Acid.—M. Boussingault.—The author's experiment shows that a seed placed in a barren soil supporting a barren atmosphere forms at first, on germinating, a fertile atmosphere, that is to say, an atmosphere containing carbon, in which, under the influence of light, the leaves develop chlorophyll, and subsequently amylaceous and saccharine matters.

Verbal Observations on the foregoing Communication.—M. Pasteur.—We may comprehend that special cells, animal or vegetable, other than those of chlorophyll may behave towards electricity as the cells of green matter behave with the solar radiations, and that the carbonic acid is decomposed and is carbon assimilated, because the electric vibrations are transformed into chemical force.

Seventeenth Note on the Electric Conductibility of Bodies Moderately Conductive.—M. Th. du Moncel.—Not suitable for abstraction.

Solar Spots, and on the Physical Constitution of the Sun.—M. G. Planté.—M. Planté concludes from his experiments that the sun may be considered as a hollow electrified globe, full of gases and vapours, and covered with a liquid covering of molten incandescent matter; the wrinkles, or luculi of his surface, result from undulation

in this liquefied stratum; the spots are produced by masses of gas and electrified vapours proceeding from the interior of the orb, penetrating the liquid layer, and giving to the edges of the cavities forms which characterise the passage of positive electricity. The *faculae* seem to be a brilliant phase in the evolution of the gaseous masses when they approach the surface before their eruption. The protuberances are formed by the gases themselves issuing from the interior of the sun at a higher temperature, and consequently more luminous than those which form the atmosphere of his surface.

Influence of the Asparagin contained in Saccharine Juices (Canes and Beet-roots) upon the Saccharimetric Assay: Destruction of the Rotatory Power of Asparagin, and Method for its Determination.—MM. P. Champion and H. Pellet.—The presence of asparagin in saccharine juices makes the percentage of sugar apparently too high, the error amounting in certain cases to 0.7 per cent. An addition of acetic acid destroys this disturbing influence.

Theory of Trial Contact.—M. Bouty.—A mathematical paper, not suited for abstraction.

Use of the Magneto-Electric Machines of M. Gramme for Lighting the Large Halls of Railway Stations.—M. A. Sartiaux.—Under the circumstances in question the electric light is not merely more intense but more economical than gas.

Simple Apparatus for the Analysis of Gaseous Mixtures by means of Absorbent Liquids.—M. F. M. Raoult.—This paper requires the accompanying illustrations.

Exchanges of Ammonia between Natural Waters and the Atmosphere.—M. Th. Schloësing.—The quantity of ammonia condensed in each gramme of water increases as the temperature falls, in spite of the gradual impoverishment of the air.

Products of the Reduction of Anethol, and on the Probable Constitution of this Latter Body.—M. F. Landolph.—The author holds that the formula of anethol ought to be doubled.

PATENTS.

ABRIDGMENTS OF PROVISIONAL AND COMPLETE SPECIFICATIONS.

Improvements in the manufacture of manures. Major-General H. Y. D. Scott, C.B., Ealing, Middlesex. February 1, 1875.—No. 373. The object of this invention is the production from faecal matters of manures sufficiently concentrated to command a ready market without the creation of a nuisance in the process. Instead of employing deodorants of a bulky and (as respects fertilising properties) inert nature, I make use in some of the closets of coal soot which contains considerable quantities of ammoniacal salts, and in others I apply as a deodorant lime, which is subsequently neutralised with phosphoric acid in a soluble form combined or uncombined with a base. The combined matters are afterwards stirred in a pug-mill, and then dried and reduced to a powder.

Improvements in the treatment of farinaceous substances for the manufacture of starch and fermented and distilled liquors. J. H. Johnson, Lincoln's Inn Fields, Middlesex. (A communication from W. Adamson, Philadelphia, Pennsylvania, U.S.A.) February 2, 1875.—No. 381. The essential features of this invention consist in preparing farinaceous substances for conversion into starch, beer, or alcohol by subjecting them to the action of heated hydrocarbons or hydrocarbon vapour under heat and pressure.

Improvements in the process of and apparatus for treating and purifying sewage and polluted waters, which improvements are applicable for collecting the soap, grease, and oils for the manufacture of gas, and for removing incrustation in steam-boilers. G. Rydill, Quality Court, Chancery Lane, Middlesex. February 3, 1875.—No. 399. This invention of improvements is for the purification of sewage and polluted waters, preventing the pollution of rivers and streams, which improvements are applicable for collecting the soap, grease, and oils, which prevents insoluble soaps and slimy scum from mixing with the sewage waters in the sewers, which may be used for the manufacture of gas and other useful purposes. As the polluted waters are precipitated with an alkali, such waters after being purified may be used for steam-boiler purposes for removing incrustation.

Improvements in the treatment of saccharine solutions. J. H. Johnson, Lincoln's Inn Fields, Middlesex. (A communication from L.

Marot, Paris.) February 3, 1875.—No. 408. The essential feature of this invention consists in raising the saccharine solutions to a boiling temperature, and then adding thereto or bringing the same in admixture in another vessel with a solution of lime or baryta, and then adding an alkali, either soda, potash, or ammonia, the same being by preference free from carbonates.

Improvements in furnaces used in the manufacture of glass. J. Sheffield, Glasgow, Lanark, North Britain. February 4, 1875.—No. 411. The features of novelty which constitute this invention are—(1) The water tank at the sides of and below the fire-bars. (2) The employment of air, and heating it by tortuous passages in the sides of the "cupola." (3) Constructing tank-furnaces with an air space passing up through the "cave," so as to divide the tank, thus enabling two kinds of glass to be simultaneously treated in the same furnace.

A new and useful process in the manufacture of imitation braids, trimmings, leather, wood, and other articles. H. Loewenberg, Charlottenburg, near Berlin, Germany. February 4, 1875.—No. 420. This invention relates to the production of flexible or elastic casts in imitation of leather, straw, or woven goods, wood, ornaments, &c. A mould is first made of the object to be imitated by pouring over the same a compound, consisting by preference of a solution of glue with glycerin, which is removed after it has solidified. Into this mould is then poured a liquid compound, consisting of a solution of glue with glycerin, oil, or soap, and any desired colouring matter. Before this solidifies the greater portion is poured out again, leaving a thin layer in the mould, which is removed after it has solidified. The surface of the mould is treated with chromic acid or acetate of iron to prevent the compound adhering to it.

MEETINGS FOR THE WEEK.

- MONDAY, May 8th.—Royal Geographical, 8.30.
Royal Institution, 2. (General Monthly Meeting).
- TUESDAY, 9th.—Civil Engineers, 8.)
Royal Institution, 3. "Comparative Geology and former Physical Geographies of India, Australia, and South Africa," by Prof. P. M. Duncan, F.R.S.
Society of Arts, 11 a.m. Sewage Conference. 8. "The Commerce of the Gaboon; its History and Future Prospects," by R. B. N. Walker.
Photographic, 8.
Anthropological Institute, 8.
- WEDNESDAY, 10th.—Society of Arts, 11 a.m. Sewage Conference. 8. "New Method of Propulsion for Street Cars," by E. H. Leveaux.
Geological, 8.
- THURSDAY, 11th.—Royal, 8.30.
Royal Institution, 3. "On Voltaic Electricity," by Prof. Tyndall, D.C.L., LL.D., F.R.S.
Royal Society Club, 6.30.
Society of Arts, 11 a.m. Sewage Conference.
- FRIDAY, 12th.—Royal Institution, 9. "Resistance of Ships," by W. Froude.
Society of Arts, 11 a.m. Sewage Conference. 8. (Chemical Section). "Salt Cake, with special reference to the Hargreaves-Robinson Process," by John Morrison.
Astronomical, 8.
Quekett Microscopical Club, 8.
Anthropological, 8.
- ATURDAY, 13th.—Royal Institution, 3. "Chaucer," by F. J. Furnivall.
Physical, 3.

Chemical Technology, or Chemistry in its Applications to the Arts and Manufactures. By THOMAS RICHARDSON and HENRY WATTS. Second Edition, illustrated with numerous Wood Engravings.

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GLASGOW CORPORATION GAS.

RESIDUAL PRODUCT WORKS TO LET, AND RESIDUAL PRODUCTS FOR SALE.

The Glasgow Corporation Gas Commissioners are prepared to receive OFFERS for a LEASE of their RESIDUAL PRODUCT WORKS, at Dawsholm, near Maryhill, and also for the PURCHASE of the TAR and AMMONIACAL LIQUOR produced at their GAS-Works there. The Lease to be for such Term of Years from 1st July next as may be agreed on. Offerers must offer a Fixed Rent of £1400 per Annum, and also a further Sum in respect of each Ton of Coal Carbonised at the Dawsholm Gas-Work. On application at the Gas Office (Manager's Department), 42, Virginia Street, intending Offerers will receive Orders for inspecting the Works. Conditions of Let may be seen in the hands of the Subscriber, from whom also Forms of Offer may be obtained, which will be received by him up till 1st June next. The Commissioners do not undertake to accept the highest or any Offer.

J. D. MARWICK,
Clerk to the Commissioners.

City Chambers,
Glasgow, April 27, 1876.

CANTERBURY GAS AND WATER COMPANY.

Offers are invited for the Purchase of the SULPHATE OF AMMONIA made at the Gas-Works during one year ending May 1, 1877. The estimated quantity is 40 tons. Tenders stating a price per ton, to be removed from the Works at the Contractor's expense, to be delivered at the Company's Offices before 10 a.m., on Friday, the 12th of May instant.

The Directors reserve the right to reject all or any of the Tenders.

JAMES BURCH, Secretary.

Castle Street, Canterbury,
May 1, 1876.

UPSET PRICE REDUCED TO £10,000 TO ENSURE COMPETITION.

FOR SALE.

CHEMICAL WORKS AT WHITECROOK, DALMUIR, NEAR GLASGOW.

To be Sold (under the Direction of the High Court of Justice) by Public Roup, within the Faculty Hall, Saint George's Place, Glasgow, on Wednesday, May 10, 1876, at 2 o'clock in the afternoon.

The Whitecrook Chemical Works of the British Seaweed Company (Limited), situated upon the Forth and Clyde Canal, with the ground connected therewith, and whole Engines, Machinery, Boilers, Retorts, Pans, Vitriol Chambers, Chimneys, and whole other Movable Plant, and Buildings on the ground, including Apparatus of the most approved construction for the manufacture on a large scale of Sulphuric Acid, Carbonate, Chlorate, Muriate, and Sulphate of Potash, Caustic Soda, Refined Brimstone, Iodine, Bromine, Iodide and Bromide of Potassium, &c. The Works have unusual facilities for water carriage, and are near to the Dalmuir Station of the North British Railway Company, by which there is direct communication to all parts of the United Kingdom. The ground connected with the Works extends to 8 acres and 10 and 7-10th poles (more or less) subject to a feu duty of £149 13s. Entry immediately.

For further particulars apply to H. J. C. F. Woodhouse, Public Accountant, 14, Warwick Court, Holborn, London; to J. N. Cuthbertson, Chemical Broker, 29, Bath Street, Glasgow, Official Liquidators of the British Seaweed Company (Limited); to Bannatynes, Kirkwood, and McJannet, Writers, 145, West George Street, Glasgow; to William A. Crump and Son, Solicitors, 10, Philpot Lane, London, E.C.; to Chauntrell, Pollock, and Mason, Solicitors, 63, Lincoln's Inn Fields, London, W.C.; or to Balfour and Paterson, Writers, 138, Hope Street, Glasgow, the latter of whom will exhibit the Titles and Articles of Roup.

Messrs. Mawson and Swan require the services of a Gentleman accustomed to the Chemical and Philosophical Apparatus Business.—11 and 13, Mosley Street, Newcastle-on Tyne.

ANALYTICAL CHEMIST OR MANAGER.

Wanted, appointment as above. Ten years manufacturing experience. England or abroad. Good linguist.—Address, R. J. A., 56, Bevington Road, Westbourne Park, W.

Wanted, a Chemist thoroughly understanding Tar Products—State salary, with references, to 321, CHEMICAL NEWS Office, Boy Court, Ludgate Hill, London, E.C.

FOR SULPHURIC ACID MANUFACTURERS.

Two Platinum Stills, that have been very little used, and were made in London, are for Sale.—For particulars address, Platinum, CHEMICAL NEWS Office, Boy Court, Ludgate Hill, London, E.C.

On Sale, a superior Double-Roll Bone Mill, ready for delivery. Whole and Ground Coprolite; and Maker of all kinds of Machinery for Manure Works.—For particulars, J. I. Headly, Eagle Foundry and Coprolite Mills, Cambridge.

THE CHEMICAL NEWS.

VOL. XXXII. No. 859.

NEW RESEARCHES ON GALLIUM.

By M. LECOQ DE BOISBAUDRAN.

I HAVE just reduced to the metallic state about 10 centigrams. of gallium, which I believe is very sensibly pure. As I have stated, the first specimen of gallium presented to the Academy owed its solidity to the presence of a small quantity of foreign metals. Pure gallium melts at about 29.5° , and is therefore liquefied when held in the fingers; it remains very readily in super-fusion, which explains how a globule remained in the liquid state at temperatures falling at times as low as zero. Gallium obtained by the electrolysis of an ammoniacal solution is identical with that prepared by means of a potassic solution. When once solidified the metal is hard and resistant, even at temperatures little below its melting-point. Nevertheless, it is sectile, and possesses a certain degree of malleability. Melted gallium adheres readily to glass, on which it forms a fine mirror, whiter than that produced by mercury. If heated to bright redness in presence of air gallium is only oxidised very superficially, and is not volatilised. In the cold it is not sensibly attacked by nitric acid, but with the aid of heat it dissolves with the evolution of red fumes. The specific gravity of the metal, determined approximately on a specimen weighing 64 milligrams., is 4.7 at 15° C., with respect to water at the same temperature. The mean sp. gr. of aluminium and indium is 4.8 at 0° . The specific gravity agrees, therefore, with theoretical prevision, but its extreme fusibility is a fact totally unexpected. For the other properties of gallium I must refer to my sealed packet, dated March 6, and to my former communications. With the permission of the Academy I will shortly describe certain new reactions of the compounds of gallium, and point out the method to be followed for its extraction from ores."

M. Wurtz laid before the Academy three specimens of gallium, one of them in a state of super-fusion, and demanded, in the name of M. Lecoq de Boisbaudran, that the sealed packet in question should be opened. This having been accordingly done by the Perpetual Secretary, it was found to contain the following paper:—

"The specimen of gallium which I had the honour of presenting to the Academy had been obtained by the electrolysis of an ammoniacal solution of sulphate of gallium. The metal thus prepared was solid, and even rather hard. Its solution in hydrochloric acid gave the rays of gallium with brilliance, and those of zinc much more feebly. The metal, therefore, was gallium containing, according to the indications of the spectrum, small quantities of zinc and insignificant traces of other metals.

"The solubility of oxide of gallium in ammonia not being great, I sought for some other solvent which might enable me to obtain concentrated solutions suitable for electrolysis. Caustic potassa dissolves a large quantity of oxide of gallium: this solution readily undergoes electrolysis, but the metal obtained by this process is liquid, and not solid like that obtained from an ammoniacal solution.

"The following observations have been made on about 1 milligram, of liquid gallium:—

- (1.) A very small globule, exposed to the open air for more than three weeks, lost neither its liquidity nor its metallic lustre.
- (2.) The metal is deposited upon a negative electrode of platinum as a dead, whitish grey coating formed of numerous minute globules. It dissolves in the cold in dilute hydrochloric acid with brisk disengagement of hydrogen.

(3.) The hydrochloric solution of the metal yields a fine spectrum of gallium; along with the zinc rays more feebly. The latter are less marked than with the solid gallium from the ammoniacal solution.

(4.) The residue from the limited evaporation of the hydrochloric solution of the liquid metal is neither coloured by iodide of potassium, nor by ammonia, nor by hydrosulphate of ammonia. The dry residue from the evaporation was nevertheless sufficient to be distinctly visible. Mercury, therefore, is not present.

(5.) Liquid gallium, deposited by electrolysis upon a small piece of platinum, was heated to redness, or almost to redness. It adhered and doubtless became alloyed with the platinum, and resisted the action of hydrochloric acid, but it was attacked by weak *aqua regia* along with a little of the platinum; the solution showed the rays of gallium. A slight whitish pellicle, insoluble in *aqua regia*, was detached from the platinum; this was, perhaps, oxide of gallium rendered insoluble by ignition.

"At the time of those experiments I still had a portion of the gallium which I had presented to the Academy, and which had been returned to me. I made use of it to satisfy myself anew of the hardness of this gallium, and of the nature of its spectrum, which I found as before was composed of brilliant gallium rays, feeble zinc rays, and insignificant traces of other metals.

"We cannot ascribe the liquidity of the gallium obtained by the electrolysis of a potassic solution to the presence of a small quantity of potassium reduced by the voltaic current, since the alkaline metal would have been quickly oxidised, both during the washings and by contact with moist air. I think, therefore, that pure gallium is really liquid, and if I obtained it at first in the solid state it was probably by reason of its being alloyed with small quantities of other metals, especially zinc. Solid gallium is, in fact, less pure than liquid gallium. The solidity of gallium seems to be caused by relatively inconsiderable quantities of foreign metals.

"We may still suppose that by the electrolysis of an ammoniacal solution there is deposited, not pure gallium, but a compound of the metal with the elements of ammonia—a hydride, amidide, or nitride.

I eagerly await the time—I hope not far distant—when, having at my disposal several centigrams. of purified gallium, I shall be able to determine its physical properties, which promise to be interesting."—*Comptes Rendus*.

REPORT

ON THE

DEVELOPMENT OF THE CHEMICAL ARTS DURING THE LAST TEN YEARS.*

By Dr. A. W. HOFMANN.

Progress in the Artificial Production of Cold and Ice.

By Dr. H. MEIDINGER.

(Continued from p. 188.)

IN 1869 the design and description of an ice machine constructed by F. Windhausen, of Brunswick,† were made public. It has only one cylinder, with a piston of the diameter of the height of the stroke. On one side of the piston there occurs compression and on the other expansion. The compressed air is forced through a cooler with a large surface, which serves at the same time as a

* "Berichte über die Entwicklung der Chemischen Industrie Während des Letzten Jahrzehends."

† Windhausen, *Mech. Mag.*, 1869, 387. *Dingl. Pol. Journ.*, cxcv., 115. Wagner, *Fahresberichte*, 1870, 542.

reservoir for the compressed air. Whilst on one side of the piston the air is compressed it expands on the other. On the return of the piston the cold air is forced into the ice-chest, from which, when deprived of its cold, it is immediately drawn on the other side of the piston. That side of the long cylinder in which condensation takes place is surrounded with water as a cooling agent, whilst the other end is packed with a bad conductor of heat. The broad piston renders it impossible for an equalisation of temperature to take place within the cylinder. For this purpose there requires an especial external arrangement for the admission and for the cutting off of the air which enters the expansion end. If the object is not to make ice but to cool spaces, the expanded cold air is forced directly into these, whilst the fresh external air is drawn into the compression end of the piston. As regards the performance of the machine nothing has transpired.

In the summer of 1871 the author saw at Berlin a powerful ice machine destined for New Orleans, constructed and experimentally set up by Windhausen. The construction was different from that above described, the compression and expansion cylinders being distinct, according to the scheme which we considered as most correct in principle, and took for the foundation of our preliminary investigation. It yielded air at -40°C ., which was filled with abundant snow-flakes. As the compression cylinder constantly drew in fresh air, hygroscopic water was deposited in the cooler, where on account of the contracted space it could no longer remain in the state of vapour. From there the air, saturated with water, passed into the expansion-cylinder, and in consequence of the cold produced by the expansion the greater part of the existing vapour was necessarily deposited as snow. This circumstance very much interfered with the working of the machine, as the friction of the piston was considerably increased by the snow, which also choked up the escape-pipes. The latent heat set at liberty acted also in opposition to the fall of the temperature even though the sum total of the negative heat units was not lessened. The piston was appropriately lubricated with glycerin. The author could ascertain nothing as to the experimental measurements made to determine the actual effect of this machine.

Since the beginning of 1873 Nehrlich and Co., of Frankfurt-on-the-Main, make the Windhausen machine with two cylinders of one size only, with especial regard to the demand in breweries. It requires a 40-horse power engine, and is guaranteed to yield hourly 2500 cubic metres of air at temperatures of from -30° to -50° . If we assume that these temperatures refer to initial temperatures of from 10° to 30° the total reduction of temperature amounts to 60° , whence the amount of the negative heat units may be calculated as 50,000, corresponding at most to 400 kilos. of ice. If the production of ice were the object in view the same quantity of air might be made to circulate. A steam-engine of 40-horse power consumes hourly 80 kilos. of coal; consequently 1 kilo. of coal would give 5 kilos. of ice—a very favourable result. Such a machine, including the engine, cost in 1873 66,000 marks (£3300).

L. Mignot,* of Paris, in 1870, constructed also an air ice machine with distinct compression and expansion cylinders. It is distinguished from that of Windhausen by the arrangement that a small pump injects water into the compression-cylinder, and that the air in the condenser sweeps over open water. This arrangement is, without doubt, advantageous. The labour of compression is much reduced when the temperature of the air is kept low. The water cannot have an injurious effect in the air since the cooled and compressed air is in any case saturated with moisture. It may therefore be expected that the compression of the air will be effected at a less cost, and its complete refrigeration will require a smaller condenser.

Particulars concerning this machine have not transpired. The more recent Windhausen machines are also provided with an injection apparatus.

(To be continued)

THE DETECTION OF ADULTERATIONS IN COFFEE.*

By Prof. G. C. WITTSTEIN.

BEFORE coffee is brought to us as a beverage it is not unfrequently subjected to different adulterations, one of which is even carried so far that of the original bean nothing but the mere name remains. H. Ludwig, for instance, relates that in commerce coffee-beans are found which are made of dough moulded after the true bean in the most perfect and deceptive manner. Such beans, however, unlike the genuine, always possess sharp edges, and may be very easily ground to a grayish yellow powder. On boiling with water they are converted into a pasty mass, which on addition of iodine assumes a deep blue colour. The detection of this imitation is therefore never attended with difficulty, and it may only escape notice if the proportions in which the artificial product is mingled with the natural bean are comparatively small.

The attempts, however, of imparting to the poorer classes of coffee the colour and general appearance of the more excellent varieties, are far more frequent. The methods of arriving at this end, whether through colouring matters or otherwise, are quite various. (See *Polyt. Journ.*, 1874, No. 213, 172.) I am informed, from sources of unquestionable authority, that one of these methods consists in placing the coffee together with a sufficient quantity of shot or lead granules in an empty barrel, rolling the latter about for some time until enough lead has been rubbed on the beans to give them the glossy appearance usually considered characteristic of the better qualities of coffee. This adulteration is not easily detected by the naked eye; usually a good lens will suffice. When absolute certainty, however, is desired, it will be found convenient to adopt the following process:—Digest the suspected beans with dilute nitric acid (1.10 sp. gr.); decant the liquid after one hour, dilute with three times its volume of water, and then precipitate the lead with sulphuretted hydrogen as usual.

Another substance for colouring coffee is a greenish powder, one hundred parts of which, according to the analysis of Lohr, are composed of 15 parts Prussian blue, 35 parts chromate of lead, 35 parts of a mixture of clay and gypsum, 15 parts water. Use is, therefore, here made of a mixture similar to that which the Chinese have for some time employed for the colouration of green tea, except that in the latter mixture turmeric is substituted as the yellow constituent in place of the harmful plumbic chromate. The different ingredients of the coloured mixture—Prussian blue often replaced by indigo (Warrington), turmeric, and gypsum—may be quite distinctly seen adhering to the tea-leaves under the lower powers of the microscope, or even by a good lens, as I have often had occasion to convince myself.

The microscope alone, therefore, is frequently sufficient to discover whether a similar mixture adheres to the coffee beans or not. For more accurate examination, however, it is better to put a considerable quantity of the beans in a suitable vessel, and to pour distilled water upon them. After two hours or so the beans are removed, and the turbid liquid allowed to settle. In presence of gypsum the supernatant clear liquid will become densely turbid on addition of baric chloride and ammoniac oxalate. Prussian blue may be detected in the sediment by giving rise to a brown colouration on addition of KHO. If this change of colour should not occur, the mixture will not

* L. Mignot, *Mech. Mag.*, Dec., 1870, 404. *Dingl. Pol. Journ.*, cxcix., 362. Wagner, *Jahresbericht*, 1871, 696.

* *Dingler's Polyt. Journ.*, No. 215, 84 (translated for *American Chemist* by Carl Barus).

contain Prussian blue, but probably indigo. The colour of the latter may be destroyed by nitric acid. The potassic hydrate will likewise decompose the plumbic chromate to a greater or less extent, causing it to dissolve partially or wholly in the alkali. Turmeric, if present, would merely turn dark brown. If, upon slightly moistening the sediment with sulphide of ammonium, a black colour is produced, no further doubt need be entertained as to the presence of chrome-yellow in the mixture.

By the process of roasting, coffee is put into a condition which renders adulteration almost impossible so long as the beans remain unground. Ground coffee, however, is one of the easiest substances to adulterate. Who does not know of the numerous essences, &c., which have come into commerce from time to time, and even at present are constantly recommended under various names, true and false? Be their name, however, what it may, the general appearance and properties of the substances are always the same; they occur as a more or less dark brown, coarsely pulverulent mass, of a peculiar empyreumatic odour, and of a similar bitter aromatic taste. They are most commonly perhaps called "chicory coffee," from the fact that the root of the *Chicorium intybus* is the principal ingredient in their manufacture. When a sufficient quantity of the latter is not at hand, red beets, carrots, and other roots resembling the turnip in properties, are substituted for it, and in Southern Europe figs are roasted, by which process a peculiar product known as fig-coffee is obtained.

As all these preparations undergo the same treatment as pure coffee, namely, of roasting to a deep brown colour, it is not surprising that in many particulars they should resemble the genuine article very closely. They cannot, however, be used as a true substitute for the latter, as all are without the most important constituent of pure coffee, namely, caffeine. Compared amongst themselves they resemble each other so closely that it would be next to impossible to discover from what substances any one may have been produced. The process of roasting has not only totally destroyed or so changed the former properties of the material that any attempt to recognise them would be hopeless, but has furthermore introduced a great number of new products the nature of which is but little known, all of which, however, are characterised by an empyreumatic oil and extractive matter. Even if the raw material possessed no bitter properties (as is the case with turnips and figs) the roasted product will always exhibit it in very marked degree; and it is known that to the originally bitter qualities of coffee and chicory, a new product, similar in properties, to which Reichenbach gave the name "assamar," is added.

It is therefore evident that bitterness of taste is no criterion for distinguishing these artificial preparations from coffee. As there is, however, an unquestionable difference between the two, it must be sought in the different behaviour of the empyreumatic oils. The nature of these oils, however, at the present day is so little understood that we are only able to separate them by difference of taste and smell, and, indeed, this means of distinction would be fully sufficient if it were only necessary to distinguish between ground chicory and ground coffee. When, however, as is usually the case, the material to be examined is a mixture of the two in which the adulteration occurs in a comparatively small quantity, we are altogether at a loss. It being obvious, therefore, that in general taste and smell lend us but little aid, it will be necessary to investigate special modes for the examination of the mixtures in question.

The following is a very convenient manner of proceeding:—The suspected coffee is poured out on water. If pure, the particles will float, and remain in a state of suspension for hours, whereas chicory will sink immediately. This process, however, is not always altogether reliable in its results; for Denault relates a case where a specimen of coffee, roasted and ground by himself, sank within a quarter of an hour, while specimens of other

pure coffees which he examined floated as long as ten hours. Chevalier mentions a similar experience. To me, however, this test has always given satisfactory results.

J. Horsley, some time ago, proposed the following process for the detection of chicory in coffee:—If to a much diluted decoction of chicory, a solution of bichromate of potash be added, no sensible reaction will take place. If, however, we subject to this same reagent a decoction of pure coffee, its colour will immediately darken, and become brown similar to porter. This is, therefore, an easy method of distinguishing between the two, provided they are separate. In mixtures the determination of the impurity becomes much more difficult. In this case a dilute decoction is made of a weighed quantity of the suspected mixture. It is then to be heated to boiling and treated with the solution of bichromate of potash. A few decigrammes of copper sulphate are next added, and the solution is again to be boiled, whereupon a dark brown flocculent precipitate will be formed. The depth of its colour depends on the quantity of coffee in the mixture; and we have thus, by comparing this precipitate with a similar one of the same quantity of pure coffee, an approximate method of examining our mixture quantitatively.*

From its importance I had this method examined by Mr. Rotmanner; it did not, however, give us any satisfactory results. Dilute infusions of chicory and coffee indeed gave, with the bichromate, the reactions which Mr. Horsley has indicated, namely, no sensible reaction with chicory, but a deep brown colouration with the coffee infusion. When to an infusion of the mixture bichromate of potash and copper sulphate were added, the precipitate above mentioned was also formed. But the precipitate from pure coffee was exactly like it. Hence, though by this process coffee may be detected in chicory, it is absolutely worthless for the detection of chicory in coffee.

These unsatisfactory results influenced me to investigate the subject somewhat further. Decoctions of each with 8 parts water were made, which afterwards were further diluted to 12 parts.

If to 30 drops of the coffee decoction in a test-tube 2 drops of conc. hydrochloric acid be added, and then, after a few seconds boiling, the liquid be treated with 15 drops of a solution of 1 part red prussiate of potash and 8 parts of water, and again boiled as before, the liquid will first turn green, finally blackish green. Upon now adding to the mixture 6 drops KHO, the liquid, after 1 to 2 minutes further ebullition, will become brown, and shortly after, with the deposition of a dirty yellow precipitate, clear, pale yellow. If the chicory decoction is subjected to the same treatment, the last liquid will be brown and turbid, and only after long standing will a precipitate be deposited while the supernatant fluid retains its brown colour. By testing in the same manner a mixture of 6 drops chicory and 24 of coffee, the brown turbidness will also be obtained. It is thus easily possible to discover adulterations of chicory in coffee.

The quantity of coffee actually dissolved in a decoction is perhaps always over-rated. A good, by no means weak, infusion, left upon evaporation a residue of 1 per cent, and a very strong infusion scarcely 2 per cent. This residue has the appearance of a dark brown shining varnish, which is scarcely hygroscopic,—at least remains dry after two days' exposure. If, however, the coffee contains one of the artificial preparations above referred to, the residue will become sticky to the fingers within two hours, and will after twenty-four hours be decidedly moist. This simple process may likewise be used as a test upon suspected coffee.

Even the impurity in coffee, chicory, is subject to various adulterations,—among others, with bog-turf. On

* This reaction is not due, as might be supposed, to the colouring matter of the coffee, but to the tannic acid present therein, for an infusion of green coffee behaves in the same manner.

this matter I have no personal experience. According to Prof. Th. Schwartz, of Ghent, however, this fraud is practised to a considerable extent, especially in Flanders.

RESEARCHES ON THE SOLID CARBON COMPOUNDS IN METEORITES.*

By J. LAWRENCE SMITH, Louisville, Ky.

IN the study of meteorites it is well known that, of all the simple and compound substances met with in these bodies, the carbon has received the least study and investigation. This has arisen principally from the limited amount of material at the command of the chemist,—a fact to be regretted, since if any one element more than another demands attention and excites wonder at the part it plays, either as an element or in its endless combinations with other substances, that element is carbon.

In its elementary condition we see it in crystals of exceeding hardness and brilliancy in the diamond, and also in irregular, nearly opaque masses that are not to be confounded with the diamond. Again, we have carbon in a soft, black, unctuous state, either in lustrous flaky crystals or in fine-grained masses. It also occurs in the harsh and gritty form of coke, sometimes changed to an unctuous body approaching graphite in aspect, yet different physically as well as in some of its chemical relations. Deposits of anthracite furnish carbon in yet another form. Besides these, the results of decomposition of what are known as organic compounds give quite a list of different forms of carbon, made either by the incomplete combustion of hydrocarbons, or by passing through red-hot tubes the vapours of hydrocarbons, chloride of carbon, sulphide of carbon, &c., or by the decompositions of such substances as carbonic acid, carbides of boron, of iron, of manganese, &c.

These various forms of carbon have certain chemical differences, more or less marked, which differences have attracted the attention of chemists; although no one has studied them with much care or success except M. Berthelot, the investigations being difficult on account of the want of proper methods. M. Berthelot obtained his results by taking advantage of the singularly slow oxidising action of a mixture of nitric acid and chlorate of potash on carbon, first pointed out by Sir B. C. Brodie, in 1860,† in experiments on graphite, by which he produced for the first time what is known as graphitic oxide. He operated by this means on very many specimens of carbon, from the diamond to lampblack, embracing a large variety of artificially prepared carbons, and discovered certainly six or eight more or less distinct chemical characteristics of these different carbons.‡ The physical differences of some of them are well known; among them none is more remarkable than that of their specific heats. Other bodies known as elements—as silicon, boron, oxygen, &c.—take upon themselves different conditions called allotropic conditions,§—a term applied to the isomeric conditions of simple bodies,—but carbon differs from these, not only in exhibiting a most wonderful variety of allotropic conditions, but also in the phenomena coming under the head of isomerism, polymerism, and metamerism; so much so that we are disposed to take this body away from the rank of a mere element, and call it a protean body, that gives rise to substances of endless form and variety by combining with a very limited number of elements.

Additional interest attaches to carbon from the fact of its being regarded as belonging pre-eminently to the

organic kingdom. In fact, some of the best observers and investigators assume that there is no such thing as mineral carbon among the rocks of our globe, and that wherever found—whether as diamond, graphite, or coal—it is a product derived from organic matter, in which it had first performed its part in the economy of nature.

A still more exciting interest has been felt in carbon since the new department of celestial chemistry has received the attention of scientists. And here we are not left for our knowledge of celestial carbon to the attenuated form of it which can be detected only by astronomical instruments; for masses of matter from other spheres reach our globe from time to time, bringing with them specimens of solid carbon for our investigation, and, at the same time, perplexing our minds with questions as to its mineral or organic origin, and as to the existence or not of life on other planets, and in other systems of planets.

Like the footprints of former life on the rock strata of our globe, these indications in what we call meteorites, however slight they may be, are not to be disregarded. While I do not wish to arrogate to myself any undue merit in the study of this subject, I must say that I believe that my methods published in 1855 set forth more prominently than it had been done before, the proper method of research for arriving at correct conclusions. It is clear that to attain positive results, the astronomer, physicist, mineralogist, and chemist must not run counter to one another in the use of the facts severally studied by them, and in all that I have done in this direction it has been my effort to keep this in view.

In the present memoir it is my object to develop new facts, and consider some points in connection with the carbon of meteorites.

The Carbonaceous Meteorites.

Certain well-known meteorites, from among those whose fall has been observed, have been called, from their aspect and from their containing a small amount of carbon, *carbonaceous* meteorites, although the small amount of carbon contained in them is not sufficient to account for their colour. Perhaps the term *melanotic* meteorite would be a more appropriate one to distinguish them from the stony and iron meteorites. There are but four of them yet known, viz., that which fell at Alais in 1806, that at Kold-Bokeveldt in 1838, that at Kaba in 1857, and that at Orgueil in 1864. They contain, respectively, about 3.0, 2.0, 0.6, and 6 per cent of carbonaceous matter.

I would here remark that the Alais, Kold-Bokeveldt, and Orgueil are more closely allied to each other than to the Kaba meteorite. The predominating mineral constituents are about as follows:—

	Alais, by Berz.	K.-Bok., by Harris.	Kaba, by Wöhler.	Orgueil, by Pisani.
Silica	31.22	30.80	34.24	26.08
Magnesia	22.21	22.20	22.39	17.00
Iron protoxide ..	29.03	29.94	26.20	29.60

If we now contrast these mineral constituents with those predominating in well-known meteoric stones, a most striking fact presents itself, one not commonly realised by those engaged in the study of these bodies. It is seen on comparing the above with the following tables:—

	Chas- signy.	Chateau Renard.	Harrison City.	Con- cord.	Dan- ville.	Sears- mont.
Silica	35.30	38.13	47.30	47.30	50.08	40.61
Magnesia	31.76	17.67	24.53	24.53	20.14	36.34
Iron protoxide	26.70	29.44	28.03	28.03	19.85	19.21

From these tabular statements it will be seen that, deducting the small amount of carbon contained in the black meteorites, the mass of mineral matter constituting them is about the same, and corresponds thus with the so-called common type of meteoric stones; and hence the mineral matter to which these constituents belong must be the

* Communicated by the Author.

† *Annalen der Chemie und Pharm.*, April, p. 6.

‡ The full detail of his researches is to be found in the *Annales de Chimie et de Physique*, 4th Series, xix., 392, 1870, and xxx., 419, 1873.

§ Notwithstanding the recent experiments of M. Weber, showing that, under certain conditions, carbon, silicon, and boron are not exceptions to the law of Dulong and Petit, they still occupy a singular position in regard to specific heat.

same in the two classes of meteorites, viz., olivines and pyroxenes, differing only in the more or less compact form of these minerals.

In the writings of some of the most astute observers of these bodies, we find little stress laid on these facts. Thus M. Meunier, in a paper on the origin of meteorites, published in the *Cosmos* of December, 1869, expresses his amazement that I should speak of the circumscribed uniformity of the composition of meteorites as evidence of a circumscribed cosmical origin of these bodies, both with reference to the sphere or spheres whence they come, as well as their rock structure. He takes so opposite a view as to say—"So far from the meteorites showing such a resemblance, we can establish between meteoric iron, olivine meteorites, aluminous meteorites, and carbonaceous meteorites, differences as great as between the most different terrestrial rocks." An assertion which would include all the ranges of rocks and sedimentary deposits from the basalt and granite to the cretaceous and tertiary deposits.

Let anyone look at the above table, and say whether or not he sees so vast a difference in the mineral constituents of the different meteorites there enumerated; and yet they represent the two extremes of these bodies so far as their external properties are concerned. It is well known that three or four minerals represent the great mass of the constituents of every meteorite in various proportions, viz., nickeliferous iron, olivine, pyroxene, and anorthite, especially the first three; and the purely iron meteorites must be recognised as magnified masses of the metallic particles to be found in every stony meteorite, not excepting even the carbonaceous meteorites.*

My object, however, in this paper is not to discuss at length the general internal resemblances of these bodies, as I may have occasion to do it more fully at another time. I wish simply to note, that black and pulverulent as are the carbonaceous meteorites, they are not removed by their mineral constituents from the so-called common meteorites. I now pass on to show that even in their carbonaceous constituent they are strongly linked even to the iron meteorites.

Graphite Carbon in the Iron Meteorites.—Ever since the internal structure of this class of meteorites has been examined by sections through the centre of these compact metallic masses, nodular concretions have been noted in their interior, the most common of which consist of *troilite*, a protosulphide of iron, and filling ovoidal cavities. Sometimes these troilite concretions have a thin coating of a lighter-coloured mineral known as *Schreibersite*, and this last is also found alone in concretionary masses which are usually angular or lamellar.

Less frequent concretions than either of the above, and even more remarkable, consist of carbon of the character of *graphite*: these, like the troilite, usually fill irregular ovoidal cavities, and are more or less contaminated with the latter mineral.

The most important of the meteoric irons containing these nodules, that have come under my immediate observation, are the Toluca, the Cranbourne, the De-Kalb, and the Sevier: the last two have received my special study, the latter furnishing much the larger part of the material in my hands.

Character of the Graphite Nodules.—These concretions differ more or less in appearance, while their general character is the same. In this communication I call special attention to a large nodule taken from the very centre of the Sevier iron, the largest that has come under my observation, and perhaps the largest known. It was detached from the iron entire and perfect in every respect. Its greatest length is 60 m.m. Its dimensions in the other direction vary from 20 to 35 m.m. The weight before it was cut was 92 grms. Its form is that of an irregular

dumb-bell, flattened on one side, and slightly nodular on the surface. Its colour is plumbago-black, except at small places on the surface, where there is a little bronze-coloured troilite. Its texture is remarkably close and compact, and it is cut readily by the saw, except when the tool encounters particles of enclosed troilite. Its structure and powder is not unlike that of the close-textured graphite of Borrowdale, in Cumberland, England, and quite unlike the scaly graphite such as that from Ceylon, or that found in certain cast-irons.

Examined from the circumference to the centre, this nodule presents the following appearances:—About one-fifth of the circumference of the section is made up of troilite with a thickness of 1 m.m. The remainder of the section has all the aspect of graphite, except in a few spots. In the nodule there is a small mass of troilite not unlike in form the entire nodule: it is 10 m.m. long by about 5 m.m. wide; it is not continuous from its circumference to its centre, but the centre portion is cut off completely from the exterior portion by a thin belt of graphite, $\frac{1}{2}$ to $\frac{3}{4}$ m.m. in thickness. Again, on other parts of the surface small particles of troilite are to be seen.

The specific gravity of this graphite is 2.26 m.m., as determined on a piece in which no troilite was visible to the eye, and after it was immersed in water and placed under the receiver of an air-pump to abstract the air from its pores.

(To be continued.)

EXHIBITION OF SCIENTIFIC APPARATUS AT SOUTH KENSINGTON.

AVAILING ourselves of the admission card courteously placed at our disposal, we paid a long and admiring visit to this interesting collection. The tedious task of unpacking, arranging, and labelling the specimens was not by any means complete, especially in the departments which chiefly attracted our attention. But for this we can no more censure the Committee and their officials than for the somewhat perplexing character of the building, which renders it difficult for the visitor to know when he has really seen all. On the contrary, having some little acquaintance with the getting up of exhibitions, we should rather congratulate all concerned on having accomplished so much. We cannot, however, pronounce the classification of the objects unexceptionable. Thus, when a chemist visits the chemical department, one of his earliest inquiries will be for the apparatus which first gave chemistry the character of an exact science—the balance. But except certain historical instruments once used by Cavendish, Young, Davy, Black, and Dalton, he will find that he must look elsewhere. In fact, the masterpieces of such makers as Oertling, Sartorius, &c., are placed in the mathematical department among measuring-wedges, water-meters, planimeters, and the like! Yet graduated glasses, burettes, &c., are found not among measuring instruments, but in their ordinary place among the requisites of the laboratory. Surely the true principle—which, indeed, has been generally followed in this exhibition—would have been to place under every science all the instruments required in its study.

One of the most interesting features of this exhibition consists of instruments which once belonged to illustrious discoverers. Men of science are generally described as hard, dry, and unimaginative, but we presume few will be able to look at the balances above mentioned, at the battery with which Davy decomposed the alkalis, at the pneumatic trough of Black, the telescope of Galileo, or the microscope of Leeuwenhock, without reverential emotion. It strikes us, however, that this collection of the rude appliances with which our predecessors achieved such mighty results could easily have been extended by the kindness of our Continental friends. Has France no sacred memorials of Lavoisier, of Gay-Lussac, or of Ber-

* At present the Orgueil and Rhoda meteorites are the only two in which no positive evidence of the presence of nickeliferous iron has been traced; in the Orgueil, however, we find nearly 3 per cent of oxides, nickel, and cobalt, and the Rhoda has not been very critically examined.

thollet, which might for a short season have been entrusted to our care? Could Sweden have sent us no relic of Berzelius or of Scheele? We most sincerely regret that the example set in this respect by Italy, Germany, and Switzerland has not been more universally followed.

Taken as a whole, the formation of this "loan collection" may be regarded as a hopeful symptom as a proof that the importance of science as a main factor in national power and prosperity is approaching nearer to recognition.

We can at present scarcely enter upon a detailed notice of the objects and specimens which most attracted our attention. We were much pleased with the fine series of preparations of vanadium contributed by Prof. Roscoe, F.R.S., of Owens College. These compounds will command general interest on account of the recent application of vanadium in the development of aniline-black. The close similarity between the solutions of vanadous sulphate and copper sulphate, and again between the chromate and vanadate of potassium is very remarkable. The specimens of coal-tar derivatives, contributed by Mr. W. H. Perkin, F.R.S., are also worth a careful examination. Turning from laboratory preparations to commercial chemicals we find samples illustrative of Mond's process for recovery of the sulphur from tank-waste as carried on at the works of J. Hutchinson and Co., of Widnes; a series of specimens explanatory of Spence's process for the manufacture of alum; specimens of oxalic acid made by Roberts, Dale, and Co., by the action of caustic alkali upon woody fibre; carbolic, cresylic, and picric acids, aurin, &c., exhibited by F. C. Calvert and Co., and a series of coal-tar colours as manufactured by Messrs. Brooke, Simpson, and Spiller, including artificial alizarin and the still more splendid anthrapurpurin. In spite of these and some other very interesting exhibits it is plain that the collection does not give, and indeed does not profess to give, a full view of the chemical manufacturers of the day. The apparatus—the main feature—we shall endeavour to describe on a future occasion.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, May 4th, 1876.

Dr. GILBERT, F.R.S., Vice-President, in the Chair.

AFTER the names of the visitors had been announced, and the minutes of the previous meeting read and confirmed, the following names were read for the first time:—Messrs. R. H. Harland, J. Edmunds, M.D., H. Holcroft, J. G. A. Stitt, T. Tyrer, C. A. Buckmaster, B.A., S. Hall, S. Gilchrist, Percy C. Gilchrist, and W. Hibbert. Messrs. H. C. Jones, Adrian J. Brown, and Sir David Salomons were then ballotted for and duly elected after their names had been read the third time.

The first paper, "*On Glycero-phosphoric Acid and its Salts as obtained from the Phosphorised Constituents of the Brain*," by Dr. J. L. W. THUDICHUM and Mr. C. T. KINGZETT, was read by the latter. On boiling kephalin, $C_{42}H_{79}NPO_{13}$, for several hours with baryta water, a solution was obtained, containing one or two nitrogenous bases and the barium compound of glycero-phosphoric acid, $C_3H_5(OH_2)BaPO_4 \cdot H_2O$. The authors then described the lead salt, the normal and acid calcium salts and the barium salt of this acid, also an alcohol-hydrated barium glycero-phosphate containing 20.9 per cent of alcohol and 11.14 of water.

In reply to a question by Dr. Tilden, Mr. KINGZETT explained that no compound had been found in the brain in which the phosphorus was in a lower state of oxidation than phosphoric acid.

The next paper was "*On some Reactions of Biliverdin*," by Dr. THUDICHUM. The author, after stating that the

cause of the yellow colour of the skin of persons suffering from "yellow jaundice" was bilirubin, whilst the dark colour of the so-called "black jaundice" was due to the presence of biliverdin, proceeded to describe some derivatives of the latter substance. *Mono-brominated biliverdin*, $C_8H_8BrNO_2$, was prepared by passing bromine vapour mixed with dry air over finely powdered biliverdin until it ceased to be absorbed, and the product was then heated to $100^\circ C$. in a current of dry air. It is a black powder, insoluble in ether, and very little soluble in alcohol. It is soluble in sulphuric acid, but is precipitated on dilution with water. It is also soluble in caustic soda, being precipitated again in brown flocks by acetic acid. *Hydro-biliverdin* is formed on treating a solution of biliverdin in dilute caustic soda with sodium amalgam. Its dilute alcoholic solution gives a spectrum showing an absorption-band overlying the line F equilaterally, and totally different from the broad band between E and F shown by solutions of hydro-bilirubin.

In reply to a question put by Mr. C. E. Groves, as to how it was ascertained that the colour of the skin of a jaundiced person was due to bilirubin or biliverdin, the AUTHOR said that in the case of yellow jaundice, the bilirubin could be extracted from a portion of the skin by treating it with chloroform, whilst the biliverdin, which produced the colour in black jaundice, could be extracted with alcohol, and the solutions then examined by the spectroscope. He also stated in reply to a question of the Chairman that in the forms of yellow jaundice which were the acute cases some obstruction of the biliary duct caused the bile to be poured out into the blood, whence it passed into the system. In chronic cases, the bilirubin after a time became oxidised to biliverdin, giving rise to black jaundice.

A paper "*On the Relation between Chemical Constitution and Colouring Power in Aromatic Substances*" was then read by Dr. O. WITT. In this paper the author endeavoured to show that the immense variety of colouring matters was due to a clearly definable law, and also the reason why some bodies are coloured whilst others, which are sometimes isomeric with them, are not colours. In the case of nitro-phenol, $C_6H_4(NO_2).OH$, and of nitraniline, $C_6H_4(NO_2).NH_2$, the colouring properties are partly due to the nitro group, but not entirely, as neither mono- nor dinitro-benzene possessed colouring powers. Only the combination of the nitro with the amido or oxy groups produced the colour, and both these were able to form salts, which the nitro group could not do. In these instances the NO_2 was the colouring group, but it could only exert its influence when another acidifying or basifying group was present. He purposed to call the colouring group the *chromophor*, and the substance containing the chromophor, but not a salt-forming group, the *chromogen*. Thus, in the examples given, the NO_2 was the chromophor and nitro-benzene the chromogen of nitraniline and nitro-phenol. In the important colours which were derived from azo-benzene, such as "Bismarck brown," he considered the group $N=N$ as the chromophor; azo-benzene and related bodies, such as tetazo-diphenyl, being the chromogens. It was not necessary that the chromophor should be a group containing nitrogen; the CO groups in the chromogen anthraquinon were the chromophors. On communicating salt-forming properties to anthraquinon by means of hydroxyl or amido groups we obtained colouring matters, whilst if we deprived these groups of their salt-forming properties, by acetylation for instance, the substances so treated lost their colouring properties.

Mr. W. H. PERKIN said he felt much interest in Dr. Witt's remarks, but thought that colour should be considered as such, and not solely in relation to dyeing. The property of dyeing was merely an accidental one, due to some affinity between the fibre and the substance, and independent of its colour. It seemed to him it would be difficult to reconcile the properties of indigo with the theory put forward by Dr. Witt.

Mr. C. E. GROVES mentioned some facts which were in

accordance with Dr. Witt's views. For instance, when the hydrogen of the OH group in picric acid was displaced by ethyl or methyl, it gave an almost colourless substance. Again, resorcin, which was colourless, gave a brilliant trinitro-derivative; but if the hydrogen in the OH groups in this were replaced by ethyl, it was converted into a nearly colourless ethyl compound, $C_6H(NO_2)_3(OC_2H_5)_2$. Orcin, the methyl derivative of resorcin, behaved in a similar manner.

Dr. ARMSTRONG thought this theory might lead to very interesting results, if it were not confined to dyes merely. Körner had noticed that whilst the ortho derivatives were strongly coloured, the corresponding para and meta compounds had a feeble colour. This was well seen in the isomeric mono- and dinitro-phenols. Thymoquinon was of a pale yellow colour, but the oxy-thymoquinon was of a richer colour, and yielded a deep violet with alkalis.

Mr. NEISON thought that a careful consideration of the circumstances influencing the colour of bodies was of very great importance, as it might enable us to determine what was actually the physical constitution of such substances.

Dr. OTTO WITT, in reply to Mr. Perkin's remarks on indigo, said it was a very peculiar case. The influence of the chromophor and the chromogen was divided: the salt-forming group existed only in the white indigo, and when the fabric was dyed and exposed to the air the chromophor was liberated, and produced the colour.

The CHAIRMAN having thanked the author for his interesting paper,

The SECRETARY read a communication, "*On Certain Bismuth Compounds (Part II.)*," by Mr. M. M. P. MUIR. In this the author mentioned that in preparing bismuth tribromide by the process given in his former paper, a certain quantity of the oxybromide, $Bi_2Br_6O_{15}$, was also formed. It is a greyish yellow crystalline substance, insoluble in water, but readily soluble in dilute acids. The remainder of the paper is occupied with the description of four chromates of bismuth, for which the author proposes the following names and formulæ:—*Bismuthyl chromate*, $Br_2O_3.CrO_3$; *bismuthyl dichromate*, $Br_2O_3.2CrO_3$; *monohydrated bismuthyl dichromate*, $Br_2O_3.2CrO_3.H_2O$; and *monohydrated bismuthyl tetrachromate*, $Br_2O_3.4CrO_3.H_2O$. They are all insoluble in water, and the last two are crystalline.

The next two papers were by Mr. WATSON SMITH, the first being entitled "*A New Method for the Preparation of the Hydrocarbons, Diphenyl and Isodinaphthyl, and on the Action at a High Temperature of Metallic Chlorides on Certain Hydrocarbons*." On passing the mixed vapours of benzene and antimony trichloride through a red-hot tube diphenyl is produced, and in larger quantity than with benzene alone. On employing stannic tetrachloride, diphenyl is formed in large quantity, being found in the receiver in solid cakes. This is undoubtedly the best method for procuring diphenyl in quantity. Toluene treated in a similar way gives but unsatisfactory results. When naphthalen and antimony trichloride are passed through a red-hot tube isodinaphthyl is obtained with comparative readiness. The frequent stoppage of the tube by reduced carbon, when tin tetrachloride is employed, renders the method with antimony trichloride far preferable. The author finds that the corrected melting-point for isodinaphthyl is really $187^\circ C.$, and not $204^\circ C.$ as formerly stated.

The second was "*A Note on the Occurrence of Benzene in Rosin Light Oils*." In refining rosin by distilling it in a current of superheated steam a quantity of light oils is simultaneously obtained. On examination it was found that the oils contained a large proportion of benzene, which on treatment with nitric acid yielded nitro-benzene, although somewhat impure. Recently an alteration had been made in the manner of distillation, a much higher temperature being used, and it is now found that no benzene is formed, toluene making its appearance as the lowest boiling member.

A paper "*On the Action of Water and of various Saline*

Solutions on Copper," by Mr. T. CARNELLY, was then read by the Secretary. In his experiments the author employed thin copper-foil, which he cleansed by treatment with dilute nitric acid and washing with water. The pieces exposed 1 square decimetre of surface, and were treated with 100 c.c. of the liquid under examination. He studied the action of distilled water; of potassium and sodium nitrate; calcium, potassium, and magnesium sulphate; sodium and potassium carbonate; sodium and potassium chloride; and ammonium sulphate, nitrate, and chloride; also solutions of mixtures of salts. The copper was dissolved in all cases, but it would seem that the ammonium salts had a far more marked action than any others, on which account waters contaminated by sewage, and therefore, as a rule, containing comparatively large quantities of ammoniacal salts and chlorides, are especially bad where copper pipes, vessels, &c., are employed.

The last communication, by Mr. G. A. HIGHT, was "*Notes on some Experiments made with a view to ascertain the Practical Value of a Proposed Method of Determining the Mineral Strength of Soils by means of Water Culture*." In these experiments plants were grown in a watery extract of the soil under examination, and the rate of growth carefully observed, the plants employed being seedlings of *Acacia Arabica*. The results are given in a long series of seventeen tables at the end of the paper.

Dr. GILBERT, in thanking the author for his paper, said that if this method were tried on plants of different habits it might lead to interesting results. He then adjourned the meeting until Thursday, May 18, when the following papers will be read:—(1) "*The Action of Malt Extract on Starch*," by Mr. C. O'Sullivan; (2) "*On the Gases Enclosed in Cannel Coal and Jet*," by Mr. J. W. Thomas; (3) "*Phenomena Accompanying the Electrolysis of Water with Oxidisable Electrodes*," by Dr. J. H. Gladstone and Mr. A. Tribe; (4) "*On the Estimation of Hydrogen Occluded by Copper, with special reference to Organic Analyses*," by Dr. J. L. W. Thudichum and Dr. H. W. Hake; (5) "*On some New Reactions of Hemine*," by Dr. Thudichum and Mr. C. T. Kingzett.

NOTICES OF BOOKS.

A Series of Exercises in Experimental Physics. By C. J. WOODWARD, B.Sc., and GEORGE SMITH, B.Sc. Part IA. —*Acoustics, Light, and Heat; Elementary Stage.* London: Simpkin, Marshall, and Co. Birmingham: Cornish, Bros.

THIS little work consists of a series of arithmetical exercises in which the student is taught to deal with easy physical problems, such as are often given in the elementary stages of the science and art examinations and the junior Oxford and Cambridge "locals." The authors have wisely refrained from adapting them for use with any particular text-book, as any of the ordinary manuals of physical science contains the necessary preliminary information. In cases, however, where the subject is more than usually difficult, some admirable preliminary remarks are made with the view of assisting the student, and a series of typical questions are completely worked out. Among the explanations we may point to that of the much misunderstood term "elasticity" as being especially good. The reputation of at least one of the joint authors as a successful teacher and ingenious experimentalist is well known, and we hope that "Part IA." will meet with such a reception as will induce the authors to continue their efforts to meet a real want.

Proceedings of the American Pharmaceutical Association at the Twenty-Third Annual Meeting, held in Boston, September, 1875. Philadelphia: Sherman and Co.

To us the most interesting portion of this bulky volume is the Report of the Committee on Adulterations and

Sophistications. It appears that we Englishmen are not the only people in the world, who, when wishing to purchase one thing, obtain in its stead some little-suspected impurity or other. The State of New Jersey appears to abound in manufacturers of essential oils who appear to make scarcely any secret of their illegitimate operations. There commercial oils of cedar, hemlock, and spruce are made by putting branches of the trees into the still along with an amount of turpentine proportionate to the price. The writer of the report states that he has on two occasions purchased four cans of oil of lemon, one lot of which contained 75 per cent of the true oil, and the other scarcely 33 per cent. Had the fraud not been detected the loss on the first lot would have been 75 dollars, and on the latter 175. "The writer recently had in his employ a gentleman who formerly held responsible positions in in two of the largest German houses dealing in essential oils. He exhibited to us a full line of receipts for mixing and cheapening all the more prominent oils, on which he placed great value and which he was very anxious to compound in this country." More recently the writer was informed by the representative of an extensive French firm, of Grasse, that all the cheap grades of lavender, rosemary, and origanum sent to America both by themselves and other houses contained as a minimum 75 per cent of turpentine.

Bees'-wax has been extensively sold in the Philadelphia market adulterated with 80 per cent of paraffin. The sophisticated article differs from the genuine in being somewhat translucent on the edges, and in being a little sonorous if struck. The paraffin may be separated by heating the wax for fifteen or twenty minutes with oil of vitriol to 350° F. The reporter has met with wax consisting almost entirely of a black, earthy matter coated externally with handsome yellow wax by dint of repeated dipping into melted wax.

CORRESPONDENCE.

ORGANISATION AMONG ANALYSTS.

To the Editor of the Chemical News.

SIR,—One of the chief causes which no doubt operates to render the profession of properly qualified analysts unremunerative is the large accession to their ranks of men who, with a *penchant* for practical science, seek this, the only profession exempt from examination a test of fitness, as the field of their labour.

Often, it may be, the *res angusta domi* obliges a man after one session of lectures and laboratory practice to take a situation in a chemical works at a starvation pittance, there to execute manœuvres in analytical chemistry marvellous to himself and probably disastrous to his employers. I have had some years experience in chemistry and could enlarge upon the subject of low fees.

What will Mr. Alldred say when I tell him that some little time ago an iron manufacturer offered the writer the privilege of making five full analyses of iron ore at the rate of 30s. per sample! He said he had never paid more than £2 2s., and that the usual figure was 30s.

I declined the offer. Possibly some one else accepted it and thus assisted to place this liberal patron of applied science in a position to abate the next man at least 10s.

I, with Mr. Alldred, do not suppose that the proprietors of some laboratories themselves make such analyses. Much they probably depute to voluntary assistants, who, with supervision, become expert mechanical analysts. When such a state of things exists it probably does pay to take in iron ores at £2 2s., or pyrites at 10s. 6d. per sample.

Your correspondent alludes to the injunction which Dr. Fresenius lays on the student who wishes to do his work thoroughly. I think, however, we must bear in mind that possibly, in some kinds of work, the rigid accuracy which

the Doctor wisely instils is not always expected for the price paid. I do not, however, advocate slipshod analyses at any price.

I heartily wish an organisation scheme were on foot. It is monstrous that labour requiring such skill should be so miserably paid.

The overcrowding of the profession, and the fierce competition such a state of things entails, sadly wants the touch of reform.

The scheme suggested by Dr. Wright is excellent and would, I think, meet the requirements of the case. As at present there are analysts, and analysts, so will there always be penny wise and pound foolish merchants and manufacturers who will employ the "bottle-washer" and cheap man, whose crude and undigested results bring discredit on the whole profession. To protect ourselves against such we must "set our house in order" lest *forsitan et nostrum nomen miscebetur istis*.—I am, &c.,

P. H.

ORGANISATION AMONG CHEMISTS.

To the Editor of the Chemical News.

SIR,—If any organisation to introduce an examination prior to practice as an analytical chemist is to be carried out it is scarcely likely to receive legislative support unless the rights of those *now* in practice are respected. When medical examinations were introduced persons in practice were not in any way interfered with, and when "registration" was instituted they were permitted to register as qualified and to recover fees, &c. No retrospective measure was made, nor is it likely that the chemical profession will receive a larger share of power or protection than the medical. At present any one may prescribe or practise medicine, and provided he does not falsely assert he is registered he may do so safely even if unqualified. Examinations are excellent things in many respects, but chemists now in practice would be very unlikely to submit to them or the fees incidental thereto.

The Society of Public Analysts, if it became the Society of Analysts, would form an excellent nucleus for an organisation amongst analytical chemists, and there is really no reason why, after a certain date, this body should not institute examinations for admission to its membership and grant a diploma. The Society could select from its members very eligible men as examiners, and consists already of a sufficiently numerous body to constitute an organised association. The Society need incur no expense in the outset, the examinations being held say in the laboratory of one of the examiners in London. As a graceful act all existing "associates" might be constituted "members" without examination. Subsequently a primary examination for associateship should be instituted.—I am, &c.,

A PUBLIC ANALYST.

May 8, 1876.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Seances, de l'Academie des Sciences. No. 16, April 17, 1876.

New Researches on Pyrogenous Carbides, and on the Composition of Coal-Gas.—M. Berthelot.—The author finds that benzin, propylen, allylen, crotonylen, and teren are present in coal-gas.

Researches on the Elasticity of Air under Feeble Pressures.—M. E. H. Amagat.

Existence, Optical and Crystallographical Properties, and Chemical Composition of Microcline, a New Species of Triclinic Felspar with a Potassic Base.—M. des Cloizeaux.—The composition of this mineral is—

Silica	64.30
Alumina	19.70
Ferric oxide	0.74
Potash	15.60
Soda	0.48
Loss on ignition	0.35

101.17

The specific gravity is 2.54.

Conclusions from the Actino-metric Measurements made on the Summit of Mont Blanc.—M. J. Violle.—A mathematical paper.

Daubreite (Oxychloride of Bismuth), a New Mineral Species.—M. Domeyko.—This mineral is an earthy mass of a yellowish or greyish white, containing a great number of crystalline lamellæ, opaque, and of a nacreous lustre. Its hardness does not exceed 2 to 2.5, and its specific gravity is 6.4 to 6.5. Its composition is—

Sesquioxide of bismuth ..	72.60
Sesquichloride of bismuth..	22.52
Water	3.84
Sesquichloride of iron ..	0.72

99.68

Bulletin de la Societe Chimique de Paris,
No. 7, April 5, 1876.

Reclamation with reference to a Paper by MM. Girard and Willm on the Methylated Violets.—M. Ch. Lauth.—In the issue of March 5, 1876, MM. Girard and Willm express themselves as follows:—"It is known that M. Hofmann has obtained mono-methylated rosanilin (very red violet) by the oxidation of methylanilin under the influence of cupric chloride and of trimethylated methyle of rosanilin (parma violet) operating in the same manner with dimethylanilin." MM. Girard and Willm appear, therefore, to ascribe to M. Hofmann the discovery of the methylanilin violets, and of the reaction from which they take their rise. My claim to this discovery has not been contested hitherto, and I think it my duty to reclaim my prior right, never doubting but these gentlemen will admit their error, which a simple examination of dates will suffice to show. MM. Girard and Willm add, in a footnote, that they admit the priority of M. Lauth, and that they merely ascribed to Dr. Hofmann the interpretation of the reaction.

Reclamation of Priority in favour of John Lightfoot.—M. A. Rosenstiehl.—The *Bulletin de la Société Chimique* has recently published a paper by M. Guyard "On the Formation of Aniline-Black by means of the salts of Vanadium, and on the Theory of the Formation of Aniline-Black." This memoir, which contains a certain number of interesting facts, has decided me to put in a reclamation of priority. M. Guyard ascribes to M. Pinkney the discovery of the remarkable influence of the vanadium salts on the formation of aniline-black. The latter gentleman has, in fact, patented the use of vanadium, the French patent bearing date December 4, 1874. But he who, to my knowledge at least, made known the influence of vanadium is the author of the discovery of aniline-black, John Lightfoot. The *Bull. de la Soc. Indus. de Mulhouse* (tome xli., p. 285) contains a paper by him, translated from the English by M. Brandt, which is completely decisive on this point: it was read in the session of Nov. 29, 1871. Lightfoot took for his starting-point my investigation "On the Part Played by Copper in the Formation of Aniline-Black" (*loc. cit.*, tome xxxv., p. 281), which had been read Nov. 29, 1865. I had shown, among other things, that if we touch with a plate of copper or brass in relief a tissue upon which has been just printed a mixture

of chlorate of ammonia and of a salt of aniline we obtain black in the parts which have been in contact with the metal, i.e., a black pattern on a grey ground. He repeated this experiment by placing upon cloth, prepared as just described, several metals which he left in contact for fifteen minutes. After this he hung up the cloth in a warm and moist place for twelve hours, and passed it through an alkaline bath. The most intense black was due to vanadium, the next to copper, then to uranium, and lastly to iron. It must be remarked that Lightfoot does not merely mention vanadium *en passant*. He recurs to it, so much had he been struck with the influence of this metal. His actual words are—"Vanadium, therefore, has still more action than copper, although the action of the latter is very decided." I will add but few more words to the above. Since 1871 no metal has been added to the list drawn up by John Lightfoot. Copper is commonly employed for aniline-blacks which are to be developed by hanging up: iron is used for steam aniline-blacks, whilst vanadium and uranium have just been patented by M. Pinkney. It follows that Lightfoot has had the merit of first calling attention to the metals whose influence upon aniline-black is utilised in practice.

Correspondence from St. Petersburg, Feb. 1/13th, 1876.—M. W. Louguinine.—M. Boutleroff, on behalf of M. Zinine, describes certain derivatives of lepidene. MM. Boutleroff and Wagner gave an account of the following researches from the laboratory of the University of Kazan:—On the addition of the elements of hydriodic acid to an organic compound, and on their separation, by M. Saytzeff; on the bromide of amylen, and on the amylic glycol of diethyl-carbinol, by MM. Wagner and Saytzeff; on the transformation of diethyl-carbinol into methyl-propyl-carbinol, by the same chemists; on the bromide of butylen and the butyl-glycol derived from normal butylic alcohol, by MM. Grabowski and Saytzeff. MM. Kanonnikoff and A. Saytzeff have unsuccessfully attempted to prepare a secondary alcohol containing the radicals, ethyl and allyl. M. Michael Saytzeff gives a preliminary communication on the preparation of diallyl-carbinol by the action of zinc and formic acid upon the iodide of allyl. M. E. Wagner has found that ethyl-methyl-carbinol is formed in the action of zinc ethyl upon acetic aldehyd.

M. Boutleroff gives an account of the following researches executed at the University of Moscow:—

M. Lebedeff has found that the synthetic pyrotartaric acid of Simpson is identical with the pyrotartaric acid obtained by distilling tartaric acid. M. Markovnikoff has found acetone in the urine of diabetic patients, and has studied the laws of the formation of direct combinations in non-saturated organic molecules.

M. Boutleroff, on behalf of M. Popoff, communicates a note on the oxidation of the isopropyl-hexylic acetone described by M. Foux.

The same chemist presented a memoir on the transformation of certain hydrocarbides of the ethylic series into the corresponding alcohols, and communicated researches on the milky juice of *Cyanitum acutum*, supposed to be poisonous to camels.

M. Kourbatoff, in his own name and in that of M. Beilstein, describes products obtained by the action of chlorine upon a solution of acetanilide in glacial acetic acid.

M. Latschinoff, on behalf of M. Koutcharoff, states that the action of an alcoholic solution of acetate of potash on monobromated ethylen, C_2H_3Br , gives rise to acetic ether and acetylen.

M. Menschoutkine, on behalf of M. Kern, describes the action of sulphocyanate of potash on a solution of the chloraurate of sodium. There is produced an orange deposit, which dissolves if gently heated. This reaction is very sensitive. In presence of sulphocyanate of potassium iodide of potassium does not precipitate the salts of palladium.

M. Mendeleeff communicates the results of his experiments on the coefficient of expansion of air under a con-

stant pressure, and points out the agreement between the properties of gallium, as actually discovered by M. Lecoq de Boisbaudran, and which he had deduced from his law of elemental periodicity for the hypothetical element, "eca-aluminium."

The papers referred to will be found at length in the ninth part of vol. vii., of the *Journal of the Russian Chemical Society*.

Blue Colouring Matters derived from the Mixed Tertiary Monamines.—MM. E. Willm and Ch. Girard.

—This patent contains two series of operations. In the first are prepared the mixed tertiary monamines containing simultaneously phenolic radicals (such as phenyl, cresyl, naphthyl), and an alcoholic radical (methyl, benzyl, &c.), or an acid radical (formyl, acetyl, &c.). In the second series these monamines are converted into blue colouring matters.

Preparation of the Mixed Tertiary Monamines.—The authors heat the following mixture in a cohobatory apparatus, provided with a worm to allow the condensed vapours to flow back into the generator:—

Diphenylamin 100 kilos.

Formic acid 30 „

The operation lasts thirty hours at a temperature between 120° and 160°. They next distil to drive off excess of formic acid, and rectify in a vacuum. A single distillation suffices to yield pure formo-diphenylamin. The same mixed monamine is obtained on heating diphenylamin directly with oxalic acid to 160° for ten to twelve hours. Aceto-diphenylamin is prepared in a similar manner by heating directly diphenylamin with glacial acetic acid in presence of a dehydrating body, such as chloride of zinc, fused acetate of potash, &c. It may also be prepared by the action of chloride of acetyl or anhydrous acetic acid upon diphenylamin dissolved by preference in glacial acetic acid. Lastly, it may be produced by the action of acetic acid upon formo-diphenylamin. This last procedure affords a general method for obtaining the mixed tertiary amines, setting out from formo-diphenylamin and an organic acid.

Transformation of the Tertiary Monamines into Blue Colouring Matters.—The authors heat 1 part of formo-diphenylamin and 2 parts of oxalic acid for twenty to twenty-two hours in a retort of enamelled iron to a temperature between 110° and 120°. In case of aceto-diphenylamin the temperature may be raised to 150°, or even 180°, without injury. To isolate the blue product the mass is run into benzin, decanted, and the insoluble residue taken up with boiling water in order to remove the excess of oxalic acid. They dry the residue, and dissolve it in strong alcohol, adding afterwards a small quantity of caustic soda so as to set the base of the blue at liberty, which is easily recognised, the alkaline solution being of a mahogany colour. It is then let cool, filtered, and the blue colouring matter precipitated by treating the solution with a current of dry hydrochloric acid gas. The hydrochlorate of the blue colour is precipitated, carrying with it the chloride of sodium formed at the same time, which is afterwards removed by washing with water. In this state the blue is pure. It may be easily rendered soluble in water by the action of sulphuric acid at 40° or 100°, according to the sulpho-conjugated compound which it is desired to obtain. For this purpose from 1 to 3 parts of acid are employed. These same blue colouring matters may also be obtained by the direct action of dry oxalic acid upon diphenylamin in presence of other organic acids, such as the formic, glacial acetic, tartaric, citric, butyric, valeric, benzoic, and, finally, phenol, &c. The proportions employed are, in general, equal parts of each body, the temperature varying from 120° to 160°, according to the nature of the acid employed. The authors append a new process for the direct production of these same blues in a soluble state. This consists in heating to about 120° a mixture of diphenylamin, of the compound intended to effect the substitution of the third atom of hydrogen, of oxalic acid, and of certain sulphates and bisulphates. The

sulphates suitable are those of magnesia, alumina, of peroxide of iron, zinc, manganese, &c. Among the bisulphates those of potash, soda, and ammonia give good results. The following proportions, for instance, may be employed with sulphate of soda:—

Diphenylamin 100 parts.

Bisulphate of soda (anhydrous) .. 100 „

Dried oxalic acid 200 „

Glacial acetic acid 50 „

Sand 200 „

The glacial acetic acid may be replaced by fused alkaline acetates. As for the oxalic acid and the bisulphates they may be replaced, in certain cases, by the compound which oxalate of ammonia forms with concentrated sulphuric acid. The proportions given above may be modified according to the nature of the substances employed, or that of the blue to be prepared. To the sulphates indicated above may be added those of aniline, toluydin, and their homologues, and that of naphthylamin. To extract the blue from the product of the reaction the mass is washed with water to remove the excess of soluble salts and of oxalic acid: the residue is treated with a slight excess of ammonia, allowed to cool, and the blue is precipitated with a slight excess of sulphuric or hydrochloric acid. The precipitated blue, which is insoluble in a free state, is washed with water to remove the excess of the precipitating acid, and then dissolved in an alkali. The blue of commerce is then obtained on evaporation. The excess of diphenylamin which remains in the sand serves, when dried, for a new operation. A portion of the diphenylamin, not transformed, is also found as a sulpho-conjugated derivative in the mother-liquors obtained from the purification of the blue.

PATENTS.

ABRIDGMENTS OF PROVISIONAL AND COMPLETE SPECIFICATIONS.

Improvements in the production of electric light, and in apparatus therefor. S. A. Kosloff, St. Petersburg, Russia. February 5, 1875.—No. 441. Placing the carbons on insulators, introducing the metal wire into the carbon, and connecting it by hinges of metal; producing the nitrogen gas by rarefying the air in the globe by means of the lighted carbons, and letting the heated air escape; filling up the space in globe, and using an automatic mechanism for the passage of the electric current from one carbon to the other in case of breakage of one of the carbons.

Improvements in the manufacture of bread. J. H. Johnson, Lincoln's Inn Fields, Middlesex. (A communication from T. C. M. Huytens, Gand, Belgium.) February 5, 1875.—No. 445. This invention has for its object the production of bread at a cheap rate, which, whilst equal in nutritive power and flavour to the higher priced best bread, shall be capable of preserving its freshness for a much longer period than the same.

Improvements in the manufacture of vinegar, and in apparatus to be used in the said manufacture. R. Pounsett, Milton House, Lindfield, Sussex. February 5, 1875.—No. 447. This invention consists in causing the air to pass on its way to the acetifying vats through refrigerating apparatus containing purifying and filtering materials.

An improved method of and apparatus for blowing glass. W. Smith, Salford, Lancaster. February 6, 1875.—No. 450. This invention relates to a method of blowing glass, wherein the glass is blown by means of air compressed into a suitable vessel by means of a fan or air-pump, instead of being blown by the breath from the lungs of the workman.

Improvements in the means and apparatus for manufacturing alkali, which improvements are partly applicable to other purposes. W. A. Lytle, The Grove, Hammersmith, Middlesex. February 6, 1875.—No. 455. The features of novelty are as follows:—1. The reduction of sulphate of baryta to sulphide of barium by incorporating it in a powdered state with carbonaceous matter, making the mixture into lumps, and reducing it as specified in the method of reducing iron ores described in my Letters Patent of the 1st September, 1874, entitled "Improvements in the Manufacture of Iron and Steel." 2. Reducing the sulphate of baryta to the state of protoxide of barium, by incorporating the powdered sulphate with carbonaceous matter and powdered iron ore or ferric oxide, and making the mixture into lumps for reduction according to the method described in the Specification of the aforesaid Patent. 3. The production of powdered reduced iron in accordance with the provisions of the said Patent, and incorporating such powdered iron with powdered sulphate of baryta and carbonaceous matter as the means, under the action of heat, of withdrawing all the sulphur from the sulphate of baryta. 4. The use of hydrate of baryta thus prepared for the precipitation of sulphuric acid from all salts containing that acid, but especially from the sulphates of sodium and potassium.

THE CHEMICAL NEWS.

VOL. XXXII. No. 861.

EXPERIMENTAL CONTRIBUTIONS TO THE THEORY OF ELECTROLYSIS.*

By ALFRED TRIBE,
Lecturer on Chemistry in Dulwich College.

THE author first briefly traces the history of electrolysis from the discovery by Nicholson and Carlisle of the decomposition of water by the pile, down to the Bakerian Lecture of Sir H. Davy in 1807.

Grotthus contended that, since in the elementary combinations of the pile of Volta, as in a magnet, there is polarity, it would establish a like condition in the elements of water. To this Faraday added the necessary idea of the revolution of the molecules of the electrolyte.

If the condition of an electrolyte just prior to, and in the act of, decomposition be in accordance with these views of Grotthus and Faraday, an electrolyte may be regarded as a dielectric whose molecules are possessed of the power of mutually exchanging their constituents during discharge. This view, jointly with some supposed points of resemblance between magnetic and electrolytic substances, led to the experiments detailed in the communication.

Experiment I.—A piece of thin copper wire about an inch long was suspended lengthwise, by a piece of cotton, between the copper electrodes (distant about 4 inches) of a Grove's battery, and immersed in a 5 per cent solution of potassium chloride. Gas was at once given off from the end of the wire facing the positive pole, and after the lapse of a few minutes the end facing the negative battery pole was found corroded.

Experiment II.—Four thin silver strips were supported lengthwise in a line between copper electrodes in a 5 per cent solution of copper sulphate mixed with a little potassium chloride. On making contact, silver chloride immediately formed upon the ends of the strips facing the negative pole, and descended in clouds, being apparently attracted towards the battery poles. Copper was deposited upon the other ends.

The preceding experiments demonstrate that an insulated conductor immersed in an electrolysing fluid may become endowed with the power of doing work exactly similar to that done by the battery poles; *may*, because, as is subsequently shown, the working power of the insulated conductors depends upon its length, position in the fluid, and conductivity, as well as chemical activity.

The action is easily explicable upon the supposition that the battery electrodes charged with electricity polarise the insulated conductor through the electrolyte, just as static electricity polarises an insulated conductor in air, the difference between the two phenomena being that a high electric tension is necessary to effect the depolarisation of the particles of air by discharge, which, of course is unaccompanied by decomposition; whereas only a very low electric tension is required to effect the depolarisation of an electrolyte, which, on the contrary, is always accompanied by decomposition.

Experiments III. and IV.—A series of twelve strips, half-inch by quarter-inch, were supported in line as in the last experiment, the electrodes reaching all the way across the trough (which was 12 inches long), and being one-eighth inch distant from the end strips. A determination of the amount of copper on each of the strips showed that the amount of decomposition was equal throughout the length.

* Abstract of a Paper read before the Royal Society, February 17, 1876.

Experiments V. and VI.—Upon replacing the broad electrodes by others only half an inch wide very different results were obtained. The action was a minimum at about the middle of the line, and rose towards each end, but being about twice as great at the positive as at the negative electrode. With the exception of this difference at the two electrodes these experiments are explicable upon the hypothesis of Grotthus. The influence of the *direction* in which the strip is inserted is shown. When placed across the line joining the electrodes it has no action, nor does it interfere with the action of the other strips.

Experiment VII.—The amount of action is shown to increase rapidly with the length of the strip.

Experiment VIII.—Pieces of glass, charcoal, and platinum were supported in solution of copper sulphate. On connecting the copper electrodes with the battery it became evident that the platinum gave the greatest action, the charcoal a little, and the glass none. This illustrates the effect of conductivity.

Experiment IX.—The influence of the chemical activity of the strips was determined. These numbers were obtained for the relative activity:—

Zinc	19.5
Aluminium	15.2
Copper	1.7
Lead	1.6
Silver	1.3
Platinum	1.0

Experiment X.—By the use of strips distributed throughout the liquid, the lines of discharge between electrodes of small dimensions are investigated, and the electric influence is shown to spread out from the electrodes in a manner much resembling the spread of the magnetic influence in the magnetic field of two dissimilar poles.

NOTES ON THE ALLEGED REPLACEMENT OF ELECTRO-POSITIVE BY ELECTRO-NEGATIVE METALS IN A VOLTAIC CELL.

By WILLIAM SKEY,
Analyst to the Geological Survey of New Zealand.

In a paper by Prof. Gladstone, Ph.D., F.R.S., and Mr. Alfred Tribe, which was read before the Royal Society on November 25th, 1875, it is asserted that when zinc and platinum are connected voltaically in a solution of chloride of potassium, "potassium is set free in some form against the platinum, manifesting itself by the presence of free alkali and hydrogen gas;" and the authors of this paper, upon the supposition above stated, and others based in a similar way, argue for the replacement of electro-positive by electro-negative metals under conditions quite contrary to those we have hitherto held to be necessary, explaining this "reversion," as they term it, by assuming that some force superior to that of chemical affinity operates for its production, and which is "called into existence by contact."

I will not here discuss the propriety of resurrecting the "contact theory," which I thought Prof. Faraday had long since disposed of; but I would like to make a few observations upon two statements which appear in this paper.

In the first place, as far as I can understand from the abstract of it given in *Nature*, it is by no means clear that "potassium is set free" in the experiment described. The alkaline reaction upon which this theory of metallic reduction is based may in reality be due to a cause quite different from that of such a reduction. For instance, an alkaline reaction can be readily obtained under circumstances which are similar to those related here, except that "contact" in a voltaic arrangement with dissimilar metals is avoided, and under which it appears impossible that any metallic reduction takes place. Thus an aqueous solution

of chloride of potassium, placed for a short time with amalgamated zinc, or for a longer time with zinc itself, even at common temperatures, becomes very alkaline. Even pure silver in a solution of this salt soon passes it to this condition.* The containing vessels in my experiments for this were agate.

This change in the character of these solutions is hardly wrought by metallic reduction, but rather, in the first case, by decomposition of water and the formation of ammonia (by the interaction of the nascent hydrogen thus liberated upon nitrogen present), assisted perhaps by the formation of oxide of zinc by atmospheric oxidation, resulting finally in the formation of an oxychloride of this metal through substitution. In the second case, that of silver, we have its direct oxidation by the free oxygen present and the reaction of this oxide upon the salt present, chloride of silver and caustic potash resulting, to which last compound, of course, that alkalinity is induced which we observe.

In the case of zinc, it may be that the actions which result in alkalinity of the saline liquid surrounding it may not be so simple as I here suppose, for the investigation seems to be required ere we can fully explain them; but still the results I have here described, and several others I could cite of an analogous nature, certainly tend to show that the conception of metallic "replacement," as given in this paper of Prof. Gladstone's, is as yet scarcely a tenable one, or at least that it requires for adequate support considerably more evidence than has yet been tendered in its behalf.

With regard now to the next statement I have here to remark upon, viz., that mercury and gold in conjunction would decompose mercuric chloride, with deposition not only of lower chloride but also of metallic mercury, I will take leave to suggest, in explanation of this, the possibility of floating dust or other impurities, or even light itself, interfering with what should be the legitimate results of the experiment described. In support of this view I found that mercury, which for utmost purity I had electro-deposited from its potassic cyanide upon platinum, gave a deposit upon gold of mercurous chloride only, in presence of mercuric chloride when kept in the dark and away from dust.

I may state here that the detection of either mercurous chloride or mercury seems greatly facilitated by using platinum in place of gold for the receiving-plate, as this metal (platinum) loses greatly in lustre by minute traces upon it of such chloride, and any mercury present is easily rubbed off upon an angle of gold, and thus readily identified.

Using this modification of Prof. Gladstone's apparatus, I was only able to get, even in sixteen hours, a deposit of mercurous chloride which was so exceedingly thin as not to perceptibly impair the lustre of the platinum upon which it had formed: its presence, in fact, would not readily be detected except by the slight darkening of this platinum in caustic potash. By the addition of hydrochloric acid, however, to the mercuric chloride, thicker deposits of this kind were obtained, but none of mercury.

The deposit of this mercurous salt though, even alone, under the circumstances described by these investigators, appears to be a very suggestive phenomenon, and this because it appears inexplicable as at first viewed. I can only attribute this deposit to the action of a free acid or of free acids upon the mercury; a minute quantity of nitric or nitrous compounds dissolved in the solution used (taken from the air) would certainly be competent to act upon mercury to the extent required for producing mercurous films such as I have obtained.

However, in regard to this deposit, and that chemical action upon mercury by some free acid which I consider necessary for its production, I find that hydrochloric acid, even, readily attacks mercury when the metal is paired voltaically with platinum or graphite: now, as it is cer-

tain that these metals initiate or facilitate this attack by reason of the oxygen condensed (chemically) upon their surfaces, and, further, as gold will also certainly possess a similar though a faster condensing power over this gas, we must consider the possibility of part of the mercurous deposits produced in all these experiments being due indirectly to the metals used in them for the negative or receiving pole. In such experiments we may safely assume that a portion of the hydrochloric acid present has been decomposed, the oxygen condensed upon the negative element oxidising its hydrogen, while the chlorine of this compound attacks the mercury. It is, in fact, a case where both poles conspire to give an effect (that is, the decomposition of hydrochloric acid) not producible by either pole separately; and it may, as now known, throw a light upon the mode in which chemical action is so frequently facilitated, or even at times initiated, by touching the positive metal with a metal negative to it in the solution we may be operating with.

It appears to me that this matter is well worth investigating.

REPORT

ON THE

DEVELOPMENT OF THE CHEMICAL ARTS DURING THE LAST TEN YEARS.*

By Dr. A. W. HOFMANN.

Progress in the Artificial Production of Cold and Ice.

By Dr. H. MEIDINGER.

(Continued from p. 194.)

ACCORDING to experience hitherto, the air machines seem better adapted for the immediate application of cold air than for concentrating and storing up cold in the form of ice, in which respect they fall too far short of the ammonia machines. They may probably be found serviceable in breweries for cooling cellars. Motive power is always to be found in such establishments with which the air-pumps can be readily connected. The introduction of cold air into the cellars secures further the advantage that they are kept very dry by means of this air, which during its compression and expansion has been to a great extent deprived of its moisture, and hence no mould is formed. Cooling with ice, on the other hand, saturates the air of cellars with moisture, and keeps it stagnant. The whole process can be carried on in breweries at a relatively small expense, as in such establishments much heat and especially much hot water is required, and thus both the escaping steam and the hot water obtained by cooling the compressed air can be utilised. An air machine supplied by Mehrlich and Co. to Hildebrand's brewery at Pfungstadt, near Darmstadt, has given for a year very satisfactory results. The principle of the air machine seems also especially adapted for ventilation where it is desirable to combine reduction of temperature with renewal of the air, as in hospitals, public rooms, and steamships. Here a trifling expansion and a small degree of cold would suffice, and hence the working cost would appear relatively low. We may look forward with interest to the further development of this subject.

We have still to make mention of a more extended theoretical investigation which Linde† has given to the public on the "withdrawal of heat at low temperatures by mechanical agencies." The main result which he has arrived at in the way of calculation—which, however, appears at once on an attentive physical consideration of the changes that take place—is that for the economical working of ice machines the temperature of the body used as a medium during expansion must not be lower, and during compression not higher, than is absolutely necessary.

* "Oxidation of Silver and Platinum by Oxygen in Presence of Water." *Trans. of N. Z. Institute*, vol. viii.

* "Berichte über die Entwicklung der Chemischen Industrie Während des Letzten Jahrzehends."

† Linde, *Bayer. Industrie u. Gewerbeblatt*, July, Nov., and Dec., 1870.

This condition has hitherto been frequently overlooked, and ignored. Whilst it has frequently been said, in explaining the merits of an ice machine that it works at such or such low temperatures, the very opposite should be the case; it should be shown that the machine produces ice without requiring a temperature far below the freezing-point of water. The above-mentioned praise is merely a certain proof that the machine consumes much power needlessly. Certainly in this case the machine may be smaller and the first outlay easier, but this advantage generally vanishes in comparison with the drawback of increased working charges. Linde proves by calculation that in a theoretically perfect machine, which produces ice at -3° from water at $+10^{\circ}$ C., 1 kilo. of coal should yield 100 kilos. of ice. He combines also with his theoretical researches a critique on all ice-machines hitherto constructed. All makers of such machines should make themselves thoroughly masters of the principles here developed, which would keep them from going astray.

In 1873* J. Armengaud communicated certain theoretical speculations on air-machines to the French Academy, which, however, contained nothing essentially novel. He lays especial stress upon the importance of cooling the air during compression by means of water. The difficulty of effecting this by means of water injected in the moment of compression he overcame by introducing into the air, as drawn in, water, by means of Gifford's injector, probably as fine spray. According to his experiments it is most advantageous to work with a degree of expansion = 2, in which case the power exerted, in proportion to the cold produced, is only half as great when the refrigeration is carried on during compression as if executed previously.

Nature of Artificial Ice.—Ice rapidly produced, at a very low temperature, is quite opaque and of a milky white. From this appearance—so different from the vitreous, transparent aspect of natural ice—the strangest conclusions have been drawn as to its behaviour. Sometimes it was assumed to be more and sometimes less permanent, sometimes to have more and sometimes less cooling power than natural ice. The truth is that artificial and natural ice differ merely in appearance. A piece of the former just taken out of the machine is of course colder than a block from the ice-cellar, and consequently melts rather more slowly on exposure to the air. Equally large pieces of natural and of artificial ice, at the same temperature, melt with equal speed under similar external conditions, and exert equal refrigeratory powers.

(To be continued.)

RESEARCHES ON THE SOLID CARBON COMPOUNDS IN METEORITES.†

By J. LAWRENCE SMITH, Louisville, Ky.

(Concluded from page 197).

The Alais Meteorite.

Two grammes of this meteorite were pulverised finely and treated with boiling water, which dissolved out a small amount of matter that has been studied by others, and which it is not my object to recur to here.

The powder was then dried and treated with pure ether in the same manner as the graphite from the Sevier iron, and the ether allowed to evaporate slowly at a moderate temperature, when the sides of the vessel became covered with acicular crystals, mixed with a few rhomboidal crystals. The residue had a peculiar odour, similar to that of the ether extract from the graphite of the Sevier iron, which odour it nearly lost in the same way, after several days exposure to the air. The form and appear-

ance of the crystals are the same as those obtained from that graphite; and a portion of the crystals detached and heated in a small tube gave the same character or reaction.

These crystals have already been studied by Prof. Roscoe, of Manchester, as carefully as could be done with the minute quantity at his disposal. My examination is perfectly in accordance with his, and there is no doubt that this product and that from the graphite must be of the same nature.

We must not forget to mention that Prof. Wöhler was the first to call attention to the hydrocarbon in these black meteorites when examining the one which fell at Kaba.

Orgueil Meteorite.

This meteorite is one of the most interesting of all the known carbonaceous meteorites, and there are one or two points connected with it that do not appropriately belong to this paper of which I will furnish a note before long. Through the liberality of Prof. Daubrée, and the Administration of Garden of Plants, I have been furnished with the material on which my investigations have been made. This meteorite has, in most respects, been thoroughly examined by M. Clézet, and by M. Pisani, and their results given in the *Comptes Rendus* for 1864. The former chemist examined the carbonaceous matter as a whole, considering it to resemble humus; and this, on drying at 110° , gave him:—Carbon, 63.45; hydrogen, 5.98; oxygen, 30.75.

I have, as yet, done little toward the re-examination of this substance, which represents from 4 to 6 per cent of the entire meteorite, my examinations being made principally for those crystalline products soluble in ether and bisulphide of carbon, of which I have found about one-half per cent in the meteorite.

The powdered meteorite was first treated with water and heated over a water-bath, and everything soluble in that menstruum thoroughly washed out. The soluble portion dried at 100° C., represents 8.65 per cent of the mass. After carefully drying the insoluble portion at 100° C., it was treated with ether in the same manner as the meteoric graphite. The ether was used in large excess, and allowed to remain for ten or twelve hours in contact with the material; the ether was filtered off, and the residue on the filter well washed with ether. The ethereal solution was evaporated slowly, when the same acicular crystals made their appearance as in the case of the graphite, and numerous rhomboidal crystals were deposited in the bottom of the beaker. These appeared to be identical with those from the graphite. The action of heat on these crystals is the same as on those from the Sevier graphite.

The powdered meteorite exhausted by the water and ether was next treated by the bisulphide of carbon, when an additional quantity of soluble matter was obtained. On evaporating the bisulphide of carbon, a yellow mass remained having the aspect of sulphur. This, when heated, gave evidence of being sulphur mixed with some carbon compound, and to all appearance it was just like the substance obtained by similar treatment of the meteoric graphite.

The crystals in the upper part of the vessel from which the ether was evaporated being detached by scraping the sides of the vessel with a horn spatula, some bisulphide of carbon was poured upon the portions remaining attached to the vessel by which it was dissolved, and the bisulphide of carbon was subsequently evaporated, when a residue was left consisting of a yellow solid surrounded by a dark brown semi-solid mass in minute quantity. This last is evidently a carbon combination not contaminated with sulphur, while the yellow mass is sulphur containing a small portion of the carbon compound.

I was enabled to obtain over 400 milligrams of these mixtures from about 50 grms. of the meteorite, much the larger portion being sulphur. A few attempts were made to separate the sulphur from the carbon compound, but

* Armengaud, *Comptes Rendus*, lxxxvi., 626. Dingler, *Polyt. Journ.* ccviii., 174.

† Communicated by the Author.

unsuccessfully; and I soon saw that by continuing my efforts, I should exhaust the small supply of material without reaching any useful result. So it was thought better to save what was left of the material as a specimen of it.

The other carbon meteorites I have not yet examined with regard to the points embraced in this report, but I hope to obtain sufficient material before long to allow of this being done, though I do not anticipate any different results from those that have been examined.

The Nature of the Hydrocarbon found in the Meteoric Graphite and Carbonaceous Meteorites.

That this substance belongs to the meteorites at the time of their fall there can be no doubt; for in the carbonaceous meteorites there is nothing to enable us to account for its formation in the cabinets in which they have been placed after their fall: and in the case of the graphite nodules they were encased in the interior of an iron mass over 20 c.m. in diameter; and, furthermore, the powder operated with was taken from the interior of a compact nodule of graphite.

I have been strongly inclined to consider this as a hydrocarbon containing combined sulphur forming a sulphydrocarbon. In the absence of chemical evidence sustaining this view, I lay some stress on the peculiar odour of the ether extract, strengthened by a most singular property of the watery extract from the Orgueil meteorite, of which I will make a short statement, reserving for some future occasion any additional remarks.

If a small quantity of the powdered Orgueil meteorite, say 2 grms., be treated with water and heated for a short time over a water-bath, no peculiar odour will be observed however carefully examined. Throw this on a filter and wash with water, then evaporate the filtrate to dryness over a water-bath, and during this time no odour will be observed; allow the residue to cool, and still there is no odour; but now throw upon the residue a little water, say $\frac{1}{2}$ to 1 c.c., move the capsule around to dissolve the mass, and then on bringing it near to the nose, a marked alliaceous odour will be perceived, sometimes so strong as to be disagreeable, reminding one of the odour of the oil of assafoetida. That it is produced by a sulphur compound chemists will be apt to admit, perhaps a minute quantity of sulphur compound, not unlike the sulphydrate of ethylene, $C_4H_6S_4$, and the needle-shaped crystals may not be far removed from the solid quintisulphide of ethylene, $C_4H_6S_5$, corresponding to sulphur 75.00, carbon 20.00, hydrogen 5.00. The crystals I scraped from the sides of the beaker—at the upper part—on which the ether solution of the Orgueil meteorite was evaporated to dryness, gave:—Sulphur, 79.65; carbon, 15.00; hydrogen, 3.00.

In the above analysis the amount of sulphur is well determined; but the examination for carbon and hydrogen was made upon so small a quantity that the results cannot be relied upon as very correct.

Roscoe burnt in dry oxygen 0.008 grm. of the residue from the Alais meteorite, and obtained 0.010 grm. of sulphurous acid, 0.008 grm. of carbonic acid, and 0.003 grm. of water, making sulphur 125 parts, carbon 54 parts, hydrogen 10.

As the above analysis was made with only 8 milligrms., of course the results can be considered only as an approximation; but nevertheless, until we get better they must serve as our only guides.

I have not said anything about the gaseous carbon compounds found in meteorites, as these form a separate study from what is designed in this paper, and besides, Profs. Graham, Mallet, Wright, and others have already studied their nature. Profs. Wright and Mallet are still engaged in this line of investigation.

Conclusions.

These, then, are some of the results of my experiments on the carbon of meteorites, and they are of great interest and importance. That we should find in the graphitic

concretions from the interior of a solid mass of iron such substances as free sulphur and a hydrocarbon, simple, or combined with sulphur, having a marked odour, was certainly not to be expected, especially as we are almost forced to believe that the iron containing it must have been at some period in a state of fusion.*

The graphite nodules themselves are grand chemical and physical puzzles, as well as all the nodular concretions in meteoric irons; that they have resulted from a process of segregation is self-evident, but how marvellous the completeness of this segregation, for if we analyse the iron, even within 2 or 3 m.m. of the concretions, only traces of the characteristic constituents of the nodules are here found. Then, again, in the case of the troilite concretions, this sulphide has been separated from the mass of iron, and a phosphide of iron and nickel has been concreting along with it; and yet there seems to be that incompatibility between these two minerals so that they could not commingle, but the phosphuret is thrust, as it were, to the exterior of the nodule, forming a thin covering to the sulphide, like the skin of an orange over the internal pulp.

Again, the graphitic concretions bear no resemblance to the scaly graphite found in the slag of iron furnaces and between the crystals of cast-iron, either in structure or appearance; the fractured surface is more like that of the Borrowdale graphite, but the oxidising action of the nitric acid and potash chlorate on this last differs somewhat from the action on the meteoric graphite. Many and varied have been the hypotheses formed in my mind to account for the formation and accumulation of this graphite, but I must admit that I have been forced at last to abandon them all, as none covers all the facts of the case. In appearance this graphite is more like the amorphous carbon that is separated from cast-iron, but the oxidising action of the nitric acid and chlorate of potash at once points out their great difference as seen by Berthelot's experiments;† and although it differs in appearance from the scaly graphite of iron, the oxidation of the two are very similar. I am more inclined to adopt the suggestion of Berthelot, that it may be formed by the reaction of bisulphide of carbon upon incandescent iron, as this reaction is known to give rise to an amorphous graphite analogous to the one under consideration, and its association with sulphide of iron would lend some support to this hypothesis; and still further the presence of free sulphur and a carbon compound, either a hydrocarbon or sulphydrocarbon, points also in that direction for a solution.

It is very clear from the present accumulated knowledge of the geological occurrences of graphite that we must abandon all attempt to account for its formation by any one series of reactions on the interior of our globe; for it is to be found in basaltic rocks, in the older crystalline rocks, and through all the series of rocks up to the recent tertiary formations, and when we add to this the laboratory experiments of Berthelot that I have so frequently quoted, this view of the subject is strengthened. But on this point I may have something more to say in a paper on the Ovifak iron, and the graphite in the basalt in which this iron is found.

The carbon from the black meteorites, as the Orgueil, Alais, &c., I consider as having a similar origin to that found in the irons; for I have proved that they both contain similar crystalline products soluble in ether and sulphide of carbon, and while the carbonaceous matter reacts differently when treated with nitric acid and potash chlorate, this may arise from the difference of conditions under which the reaction took place that gave rise to it.

That the carbonaceous matter in the black meteorites is

* In an article recently published by Dr. Mohr (*Annalen der Chem. und Pharm.*, Dec., 1875, p. 257), he advances the theory that meteoric iron and meteoric stones have been formed by the agency of water; his arguments are interesting, but far from being sufficiently convincing to cover all the facts in connection with meteorites.

† *Annales de Chem. et de Physique*, Fourth Series, T. xix., p. 425.

to be regarded as a kind of humus arising from organised matter is contrary to all we know about humus. For if we examine the mineral constituents of these meteorites we find them to be a granular mass, with particles more or less impalpable, composed essentially of olivine and pyroxene, a most unpromising soil for so luxuriant a growth of vegetation as must have occurred to produce so abundant a percentage of carbonaceous matter as that found in the Orgueil meteorite. The action of caustic potash upon it is very different from the action of that alkali upon what is commonly called humus (although we must bear in mind that humus is not a well-defined substance, it being commonly regarded as vegetable matter that has not undergone complete decomposition into water and carbon, but by imperfect oxidation is converted into a varied mixture of carbon and certain organic compounds rich in carbon, some of them soluble in caustic alkalies). After the powdered Orgueil meteorite has been exhausted by water, ether, and sulphide of carbon, caustic potash or soda dissolves but an exceedingly minute trace of the carbonaceous matter, and even that trace may be a little hydrocarbon not extracted from the mass by the ether and sulphide of carbon. If a portion of the same be dried at 110°C ., and then heated in a closed tube, water will not be given off until the temperature is elevated considerably. If the temperature be further increased, only a very slight odour is apparent; and this is another marked difference between it and humus. If heated on platinum foil the carbonaceous matter burns off very readily with little or no odour, leaving an abundant residue. According to my experiments this combustible matter amounts to about 4.5 per cent of the entire meteorite.

It is not at all improbable that the carbonaceous matter of the black meteorites approaches in character the so-called hydrated carbon first pointed out by M. Eggertz, but so clearly defined by MM. Schutzenberger and Bourgeois in a communication made to the Chemical Society of Paris, in April, 1875, which was obtained from white cast-iron by dissolving away the iron. But it is a question, in my mind, whether the carbon combination thus obtained from white iron is to be properly considered a hydrated carbon; that is to say, whether we are to consider the H_2O as united to the carbon in the same way as it is to metallic oxides to form what are known as hydrated oxides. If, however, it is to be considered as combined in a manner analogous to the H_2O , with ethyl to form alcohol, then there may be some plausibility in the hypothesis. For it will be remarked in referring to the actions of this hydrated carbon that it in no way resembles amorphous or ordinary carbon.

It is represented by MM. Schutzenberger and Bourgeois as follows:— $\text{C}_{11} : 3\text{H}_2\text{O}$; carbon, 70.95; hydrogen, 3.23; oxygen, 25.80 per cent.

According to M. Cloez the carbonaceous matter of the Orgueil meteorite, after being dried at 110° , was found to be composed of:—Carbon, 63.45; hydrogen, 5.98; oxygen, 30.75; and when we consider that some of this hydrogen belongs to the hydrocarbon now known to exist in that meteorite, the remainder of the hydrogen will approach near the proportion required to form water with the oxygen; and the quantity of carbon that may exist as a hydrate will be slightly diminished.

Attempts were made to separate completely all the mineral matter from the carbon, but I have failed to do so, after using fluorhydric acid alone, and in conjunction with nitric acid, also fluoride of sodium and sulphuric acid with a small amount of water, then treating the residue with cold nitric acid. There is no difficulty in getting rid of a great part of it, but in every instance the carbonaceous matter has been altered, however carefully the temperature was managed.

When this matter thus obtained is heated in a closed tube, after being dried at 110°C ., it not only furnishes water at about 250°C ., but gives out a very strong odour somewhat like that produced from certain bituminous

coals, at one point resembling the disagreeable odour of an ignited cigar of a very inferior quality of tobacco.*

Viewed in the light of these experimental researches, the most reasonable conclusion is that this carbonaceous matter is not in any proper sense either carbon or humus, but a carbon compound analogous to the one just referred to.

Future researches upon the solid compounds, resembling in appearance amorphous carbon, such as hydrographitic oxide, pyrographitic oxide, carbon hydrate, and similar compounds that may yet be discovered, will doubtless throw some light on the true nature of the carbonaceous compound of the black meteorites. So far as our knowledge now extends, its formation and its origin are wrapped in as much obscurity as the origin of the bodies in which it is found.

What we do know is that this carbonaceous matter occurs with the same minerals, viz., olivine and pyroxene, which are the predominating constituent materials of all stony meteorites; also with the nickeliferous iron found in both stony and metallic meteorites; and furthermore, that this carbonaceous matter contains curious crystalline products soluble in ether and sulphide of carbon, which last have been traced in the graphite nodules in the interior of the metallic meteorites. Moreover, in these graphite nodules we have found magnesia, which is so uniformly a constituent of the minerals of the stony meteorites.

So far then as our present knowledge goes, we know of celestial carbon in three conditions, viz.: in the *gaseous form* as detected by the spectroscope in the attenuated matter of comets; in meteorites in the *solid form*, impalpable in its nature and diffused in small quantities through pulverulent masses of mineral matter that come to the earth from celestial regions; also in the *solid form, but compact and hard*, resembling terrestrial graphite, and this is imbedded in metallic matter that comes from regions in space. But while we speak of these as forms of carbon, I think we should be careful in associating it in our minds with the element carbon as we understand it in its pure state whether crystallised or amorphous, for I cannot reconcile the carbon vapour detected in comets as simply that known as pure carbon in the form of an elastic vapour, nor are we to circumscribe ourselves with the notion that this cosmical carbon has an organic origin.

The researches embraced in this communication, while in many respects of a novel character, are imperfect from their very nature, both from lack of material for a thorough and complete study, as well as from the present imperfect methods of operating upon the minute quantity of the most interesting of the substances obtained.

I have therefore detailed as carefully as I could all the results as they have developed themselves, hoping that future opportunities may be afforded for continuing them, when new celestial messengers of the carbonaceous type shall visit our globe.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, May 18th, 1876.

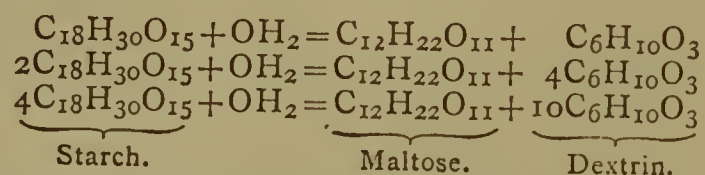
Professor ABEL, F.R.S., President, in the Chair.

AFTER the minutes of the previous meeting had been read and confirmed, and the names of the visitors announced, the following names were read for the first time:—Messrs. A. B. Cortis, G. F. Thomson, J. Heron, C. G. Matthews, G. Evans, J. R. Hakewell, and Dr. Otto N. Witt. Messrs. Joseph Davidson, D. Hesketh Richards, and W. J. Han-

* This odour will be found to belong to the hydrated carbon from cast-iron, when heated in the same way.

ner were elected Fellows of the Society after their names had been read for the third time. Professors Baeyer, Butlerow, Cooke, Friedel, Heintz, and Thomsen were also elected Foreign Members.

The first paper, "*On the Action of Malt Extract on Starch*," was read by the author, Mr. C. O'Sullivan. After a short account of the literature bearing on this subject, the author gave details of his own experiments, from which it would appear that maltose and dextrin are the only products of the action of malt extract on starch, but there is no action in the cold on ungelatinised starch, the malt extract beginning to dissolve starch at the temperature of gelatinisation, or a few degrees lower. This temperature varies for the starches from different sources, and even for the different sized granules of the same starch. If, however, starch be previously gelatinised, malt extract dissolves it almost completely in the cold. When starch is dissolved by malt extract at any temperature below 63° C., if the solution be cooled and filtered after the lapse of five to ten minutes, it invariably contains maltose and dextrin in about the proportion of 67.85 of the former to 32.15 of the latter. At any temperature between 64° and 68° or 70° C. the product will be found to contain them in the proportion of 34.54 maltose to 65.46 of dextrin, whilst at 68° or 70° C. to the point at which the activity of the transforming agent is destroyed they will be in the proportion of about 17.4 maltose to 82.6 dextrin. These three reactions are respectively represented by the equations—



The PRESIDENT thanked the author in the name of the Society, and said they were much indebted to him for the account of these important and interesting reactions.

Prof. CHURCH desired to express his great satisfaction at Mr. O'Sullivan's paper, as he had occasion to examine the same subject himself. From the difficulty there was in dissolving a portion of the starch, he was inclined to agree with the view that it consisted of two bodies—starch granulose and starch cellulose—the latter, of which there was only 4 to 5 per cent, being difficultly soluble. Pure cellulose from cotton-wool dissolves readily in dilute sulphuric acid of a certain strength, and if the solution is at once poured into water the cellulose is precipitated: if the acid solution be allowed to stand some time, however, no precipitate is produced on pouring it into water.

A MEMBER said he had made some experiments on the transformation of starch by sulphuric acid, and had found that the amount of water present had an important effect on the conversion. He might also mention a way by which the difficulty of drying syrupy substances might be obviated, and that was by placing them on a tared piece of filter-paper: when heated the substance melted, soaked into the paper, and dried readily.

Mr. O'SULLIVAN, in reply, said, with regard to the observations made by Prof. Church, the difference observed in the action on starch was not due to the presence of two principles, but to the fact that in the same starch the small granules were far less soluble than the larger ones. In the conversion of starch into dextrose by sulphuric acid he had observed that before all of it was converted other neutral substances were formed. He had been working on this subject, and hoped to lay an account of his experiments before the Society ere long. He stated there was no difficulty in drying the extracts: they were evaporated to a syrup, then put under the receiver of an air-pump over sulphuric acid. On exhausting they swelled up, and soon dried up completely.

Dr. H. E. ARMSTRONG then read a paper "*On 'Meta-xenol,'*" by himself and Mr. GASKELL. After alluding to Fittig's and to Wurtz's researches, who had obtained xenols from mesitylene and from coal-tar xylene, the one liquid and the other solid, the speaker said they had pre-

pared xenol from carefully rectified coal-tar xylene, boiling at 136° C., by fusing the potassium sulphate with potassium hydrate. The xenol obtained, which became a pasty crystalline mass at a low temperature, was converted into the sulphonic acids by treatment with sulphuric acid, and the barium salts prepared. In this way two salts were isolated, one of which crystallised with 3 molecules of water; the corresponding potassium salt when heated with hydrochloric acid in sealed tubes yielded a solid meta-xenol, identical with that obtained by Wurtz, and melting at 76° C.; the other, a basic salt which only contained 2 molecules of water, gave a liquid xenol when similarly treated. The action of bromine on the meta-xenol sulphonic acid gave rise, in the first place, to a monobrominated acid, $\text{C}_6\text{HBr}(\text{CH}_3)_2(\text{HSO}_3)\text{OH}$, which by the further action of bromine was converted into dibromoxenol, $\text{C}_6\text{HBr}_2(\text{CH}_3)_2\text{OH}$, melting at 80°. The latter compound was also obtained on treating xenol with bromine. On adding bromine to xenol and water, however, in the manner practised by Fittig, a mixture of two substances was obtained, which, when crystallised from alcohol, gave yellow crystals melting at 176° C., and supposed by Fittig to be dibromoxenol: (the yellow substance appears, however, to be formed by the action of the alcohol on a bromine addition-product. The authors have also obtained a crystalline dinitro-derivative by the action of nitric acid on the sulphonic acid.

The PRESIDENT having thanked the authors,

Mr. J. W. THOMAS read a paper "*On the Gases Enclosed in Cannel Coals and in Jet*." The author has examined the gases obtained from two specimens of Wigan cannel, two of Scotch cannel, namely, Haywood and Leshmahago, Whitehill cannel shale, and the finest quality of Whitby jet, such as is used for ornaments, the processes employed being similar to those described in his former paper on anthracite, bituminous, and steam coals. He finds that the cannel coals as well as jet differ considerably from these, containing methane, ethane, and probably other gaseous paraffins, as well as oily matters which likewise appear to belong to the same series. Wigan cannels, with regard to the gases they contain, occupy a position intermediate between steam and Scotch cannel, whilst Scotch cannel occupies a position between bituminous house coals and Wigan cannel.

The fourth paper, "*On Phenomena Accompanying the Electrolysis of Water with Oxidisable Electrodes*," by Dr. J. H. GLADSTONE and Mr. A. TRIBE, was read by the former. The authors have employed electrodes consisting of narrow plates of zinc, lead, iron, copper, silver, and platinum, together with the purest water collected directly from the pipe of the still, using a single cell of Grove for each pair of electrodes. The result obtained with platinum was negative, but in the case of the more active metals no oxygen was given off at the positive electrode as gas, but it combined with the metal, forming a hydrate, and as the hydrates are probably more or less soluble in water, a dilute solution of the metallic compound is soon formed, which is successively decomposed, the final result being the deposition of metal on the negative electrode, as a growth of threads, fringes, or arborescent crystals. The comparative solubility of the metallic hydrates in water has doubtless a great effect on the ultimate result, but the differences observed with the various metals must be looked for in their different electromotive force or affinity for oxygen.

The last paper "*On the Estimation of Hydrogen Occluded by Copper, with special reference to Organic Analysis*," by Dr. J. L. W. THUDICHUM and Dr. H. W. HAKE, was read by the latter. From the results of their experiments the authors are inclined to differ from Mr. Johnson—who some time since published a note on this subject—and believe that no error of importance is introduced by the hydrogen occluded by copper which has been allowed to cool in an atmosphere of that gas, especially if the copper employed has been previously used in analysis. The amount of gas evolved under ordinary circumstances

would not introduce a greater error than 0.025 of a per cent, and even this may be avoided by allowing the copper—after it has been reduced by hydrogen—to cool in a current of carbonic anhydride.

The PRESIDENT, in thanking the authors, said the Fellows, especially those engaged in organic analyses, were much indebted to them for pointing out the smallness of the error likely to accrue from the occlusion of hydrogen by copper, and also for pointing out a very simple way by which it might be avoided.

The meeting was then adjourned until Thursday June 1, when the following papers will be read:—"On Hemine, Hematine, and Phosphorised Compounds contained in Blood Corpuscles," by J. L. W. Thudichum and H. Kingzett; also "Note on the General Applicability of Frankland and Armstrong's Combustion Method to the Estimation of Carbon and Nitrogen in Carbon Compounds," by the same authors; "On the Liquid Carbon Dioxide from Different Sources," by W. N. Hartley; "(1) On Peroxides, (2) On the Estimation of Nitrogen, (3) On Chromic and Perchromic Acids, by T. Fairley; "On Aluminium Nitride," by Prof. Mallet; and "Chemical Studies," by Prof. Dewar.

NOTICES OF BOOKS.

Quantitative Chemical Analysis. By Dr. C. REMIGIUS FRESSENIUS. Seventh Edition. Translated from the Sixth German Edition by A. VACHER. Vol. I. London: J. and A. Churchill.

THE re-issue of a work whose merits are fully and universally admitted affords, as a rule, little scope for the reviewer. The present case, however, forms an exception. Like other departments of chemistry the art of analysis has made considerable progress during the last ten years, and an entire revision of every part of the work has hence become necessary. New methods have been introduced, and those formerly in use have been subjected to a minute and scrupulous verification and modified in accordance, or their imperfections, and the condition under which they cease to be trustworthy, have been pointed out. The work has also been enlarged. The first volume, now before us, contains the general part of the subject. The second volume, announced as shortly to appear, comprises the application of the rules and principles laid down in the former part to especial cases, including technological products and mixtures. This section in the earlier editions constituted a mere appendix.

The following passage from the author's introduction bears so pointedly upon a question frequently discussed in our columns, expresses our views with such power and authority, that we do not hesitate to quote it in full:—

"Everyone who has been engaged in quantitative analysis knows that cases will sometimes occur, especially when commencing the study, in which doubts may be entertained whether the result will turn out correct, or in which the operator is even positively convinced that it cannot be quite correct. Thus, for instance, a small quantity of the substance may be spilled, or some of it lost by decrepitation, or the analyst may have reason to doubt the accuracy of his weighing, or it may happen that two analyses of the same substance do not exactly agree. In all such cases it is indispensable that the operator should be conscientious enough to repeat the whole process over again. He who is not possessed of this self-command, who shirks trouble where truth is at stake, who would be satisfied with mere assumptions and guess-work where the attainment of positive certainty is the object, must be pronounced just as deficient in the necessary qualifications for quantitative analytical research as he who is wanting in knowledge or skill. He, therefore, who cannot fully trust his work, who cannot swear to the correctness of his results, ought, on no account, to publish

or use his results as if they were positive, since such proceeding could not conduce to his own advantage, and would certainly be mischievous as regards the science."

But what, then, of the "high" and the "low" commercial analyst who is quite ready to use, publish, and swear to his results, not merely where doubtful, but when he well knows them to be erroneous; and what must be the influence of such conduct on science, and on the chemical profession? Can competent and upright men afford to permit this system to continue?

The section on the determination of phosphoric acid and its separation from bases is exceedingly elaborate. A careful comparison of the precautions here indicated, with the procedures adopted by certain agricultural analysts, will make the conflicting results so frequently obtained perfectly intelligible. The old "commercial process" so-called, precipitation with ammonia as a salt of lime, is very justifiably ignored. The only method pronounced applicable for the separation of phosphoric acid from all bases is Sonnenschein's molybdate of ammonia process, and the precautions to be taken are fully explained. The magnesia process—precipitation with so-called magnesia-mixture—is not recommended in presence of alumina. Ville's modification of the process as described in the CHEMICAL NEWS (vol. xxx., p. 200) is not criticised. Of Chancel's bismuth method it is remarked, quite correctly, that it "cannot lay claim either to quickness or accuracy." The volumetric process with uranium solution is declared inapplicable in presence of sesquioxide of iron and alumina. If these bases have to be previously removed its great advantage, speed, is materially reduced.

For the determination of copper the methods of Steinbeck and Luckow are recommended. The difference between this edition and the abridgment translated from the fifth German edition and published in 1870, cannot be better explained than by a reference to the section on sulphuric acid. If any tyro who has determined this acid with no other precautions than those laid down on page 165 of the earlier edition, comes to read the paragraph on page 299 of the one before us he will doubtless be led to regard his results with some misgivings. In the presence of salts of iron we have found the precaution of adding a little tartaric or citric acid—first recommended, we believe, by Prof. T. E. Thorpe, of the Yorkshire College of Science—very useful in preventing the sulphate of baryta from being contaminated with traces of iron.

The translator has executed his task in a thorough and conscientious manner. There is no attempt at abridgment—a process fatal to a work of this kind where the author has introduced nothing but what his experience has pointed out as absolutely necessary. The old notation and nomenclature are retained by the express wish of the author, who declares that he holds, in common with the highest authorities in Germany, "a most firm conviction that for Inorganic Chemistry it is the simplest and the best." We cannot help respectfully commending this passage to the notice of critics who denounce an author for having used the term "phosphate of soda" instead of the lengthier and perhaps more high-sounding "hydrodisodic phosphate." The index—a matter of great importance in a book intended for constant reference—is very elaborate. It is scarcely necessary to add that no analytical chemist can afford to be without a copy of this new edition of the masterpiece of Dr. Fresenius.

Railway Appliances: a Description of Details of Railway Construction. By JOHN WOLFE BARRY. London: Longmans, Green, and Co.

THIS book belongs to a series entitled "Text-Books of Science Adapted for the Use of Artizans and Students in Public and Science Schools." Now without any wish to call in question its utility, or the accuracy of the information it conveys, we feel considerably puzzled to know why it should be considered a "Text-Book of Science." Its

object is not to explain phenomena by reducing them under laws, but to give rules for attaining certain ends. Hence all sound methodologists would term it a text-book, not of science, but of art.

The first chapter of the work gives a summary of acts of Parliament and other regulations affecting railways. Upon this follow in succession chapters on the permanent way, on points and crossings; on signals, on the block system, on stations, and on rolling-stock. The important subject of signals, we may here remark, is receiving at present an increased amount of attention, in part probably owing to the lamentable Huntingdon collision. The fact that a sudden and violent snow-storm, which, after all, is a possible occurrence at any time during the five or six months of the year, may at any time cause the signal for "danger" or for "caution" to be interpreted as "safety" and allure a train to the destruction of its inmates. Nor is this all; red or green lights produced by the interposition of coloured glasses between a lamp and the eye of an observer are exceedingly reduced in their illuminating power. Hence the question arises whether colour should not be altogether discarded, and whether the number and the position of the lights exhibited might not be made available. Thus a single light might express safety, two in a perpendicular line denoting caution, and three standing for danger.

The author seems somewhat sore on the subject of danger on railways. He "regards as a popular error the supposition which is from time to time advanced that railway engineers and managers are, as a body, remiss in attention to the safety or convenience of the public. From personal experience he can vouch for the care, foresight, and anxiety which are bestowed on these subjects by those in charge of our railways." We are delighted to hear it, and hope that some day all this "care, foresight, and anxiety" will begin to bear a little fruit.

This work, to the best of our knowledge, takes up ground previously unoccupied, and we have no doubt that it will be found highly useful by such as are likely to be engaged in the construction or management of railways.

CORRESPONDENCE.

THE CHEMICAL SOCIETY.

To the Editor of the Chemical News.

SIR,—One would properly expect, from its very name, that the Chemical Society was an association of chemists for the advancement of chemistry, and it would be natural to think that its Fellows were chemists.

Need I say, Sir, that such is not at present the case with the Chemical Society of London. By the indiscriminating admission in times past of all candidates, it has at last reduced itself to its present state—not a body of chemists, but a motley mixture of all kinds of dabblers in science, and often, alas! very often, not even that. It was only to be expected, and the present relations of the Society to the profession too plainly show it to have been so, that it would soon alienate from itself all real chemists, and cease to represent the chemistry of this country.

Recently awakened to the fact that their Society was in disgrace and was begun to be held in derision, and almost in contempt, even by themselves, a number of the Fellows have for some time past steadily refused to admit any but those who show unmistakable evidence of chemical attainments, and so at each successive meeting a larger and larger proportion have been blackballed, until, last meeting but one, two-thirds of the candidates were rejected.

It appeared as if, at last, there were some hope of the Chemical Society making its Fellowship worth the having—it appeared likely to regain its chemical prestige and

attract to itself many who now hold aloof. But, for some reason, this course has not recommended itself to the senior Fellows, and at the last meeting, on the 18th inst., the President, speaking on their behalf, strongly advised that the practice should be discontinued.

Many of those who, like myself, heard that speech must have been deeply pained to hear from the President of the Chemical Society, and that President Prof. Abel, that a "strictly chemical qualification" ought not to be required for its Fellowship.

No doubt Prof. Abel, like ourselves, has the prosperity of the Chemical Society at heart, but while he seeks it in large numbers without much regard to quality, we see its best interest in a high standard of fellowship, by which the dignity of the Society may be so raised that many would be drawn into it who now hold back in disdain. Upon this point, whether the numbers of the Society will be lessened, the opposition turns, and as there does not appear any reason to fear falling off in this respect, it is to be hoped they will adopt the view that the attractiveness of the Society will increase with the standard of qualification required, and heartily join the movement.

It has been admitted by all, even by Prof. Abel and his colleagues, that something must be done to raise the chemical profession out of its present deplorable condition. It must be organised; either the Chemical Society must be made a society of chemists indeed, or some new body, representing the profession, must be founded, such as the "Institute of Professional Chemists" now under consideration. Then I, for one, will not care who is admitted to the Chemical Society, but until then, and so long as the F.C.S. is considered to indicate chemical abilities, so long must the gates of the Chemical Society be closely guarded.

"Sculptor," "Artist," "Clerk in Holy Orders," "B.A.," "B.Sc."—vaguest of all—"Science Teacher;" these, forsooth, are the qualifications stated by candidates on their papers. What must they think of the Chemical Society! What must be their respect for it!

In conclusion, I sincerely hope that the Fellows will continue to exercise more and more care in electing candidates, and they may then hope to have a society commanding the respect of chemists.—I am, &c.,

NUNTIUS.

BLACKBALLING AT THE CHEMICAL SOCIETY.

To the Editor of the Chemical News.

SIR,—On the 4th instant, six out of nine candidates for admission to the Chemical Society were rejected by its Fellows. On the 18th (the last meeting of the Society) the President condemned this act in language which, to say the least, was injudicious, and which most certainly would have been received in a different spirit had it not been for the great respect entertained for the speaker. In brief, it was intimated that the Fellows ought not to use their individual judgment, and that the blackballing was indiscriminate. If this latter be true, it is a grievous fault and calls for immediate rectification. I take it that the qualification required for the Fellowship of the Chemical Society is a chemical one, and a chemical one only. If candidates fail to state this briefly, clearly, and truthfully they, and they alone, are responsible for their non-election. A sculptor or artist may justly aspire to the membership of the Royal Academy; an M.D. to the membership of one or more of the Medical Societies; and a D.Sc. to the membership of a Society for the Propagation of General Knowledge. But as these are not chemical qualifications, the man who offers these and these alone insults the Chemical Society, and should, in consequence, be unanimously rejected. Had the President sought for the real cause of the independent action of which he complained, he would have found it in the profound distrust which the majority of the working Fellows of the Society have for those who should be their profes-

sional leaders, men who, with few exceptions, seldom attend their meetings, and yet, being leaders of their Society, systematically sacrifice the British chemist and British chemistry, simply to increase the number of Fellows, and thus give to the Society of which they are the nominal heads a fictitious importance.—I am, &c.,

TWIG.

May, 22, 1876.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Seances, de l'Academie des Sciences. No. 18, May 1, 1876.

Electromotor Forces Produced by the Contact of Liquids Separated by Capillary Diaphragms of any nature whatsoever.—M. Becquerel.—Solutions, or the bodies which they contain, are condensed in capillary spaces in the same manner as are gases in porous bodies.

Microscopic Examination of Orthose and of various Triclinic Felspars.—M. Des Cloiseaux.—This interesting paper is not capable of abstraction.

Electric Polarisation.—M. Th. du Moncel.—Not suitable for abstraction.

New Researches on Gallium.—M. Lecoq de Boisbaudran.—Inserted in full.

Experiments on Solar Heat.—M. Salicis.—The author's experiments have given the interesting result that if the evaporation is slow in a glass boiler exposed to the sun's rays, which might be expected, it becomes very active if a metallic nucleus is fixed in its centre, such as a phial of mercury, thus procuring in the midst of the water a furnace exhaustless as the sun. A second result is that if an oxidisable metal is used as a nucleus, such as iron, the production of iron is very rapid, and, consequently, also the production of hydrogen.

Phenomena of Interference obtained with Slender Laminæ of Collodion.—M. E. Gripon.—Not suitable for abstraction.

Distribution of Magnetism in Cylindrical Bars.—M. Bouty.—This paper consists chiefly of mathematical formulæ.

Transmission of Electric Currents by Derivation across a River.—M. Bouchotte.—An account of an experiment on the transmission of telegraphic signals through water without wires.

New System of Electro-Magnet.—M. V. Serrin.—The author forms his electro-magnetic spirals with metallic coils deprived of all insulating coatings, and arranged so that the coils may not touch each other.

New Sulphate of Potassa.—M. J. Ogier.—The composition of this salt is—

Sulphuric acid (SO ₃)	44.9
Potassa (KO)	50.6
Water (HO)	4.6

100.1

The formula SO₃KO + $\frac{1}{2}$ HO would require—

Sulphuric acid	43.7
Potassa	51.3
Water	5.0

100.0

The peculiarity of its properties seems due to a very small quantity of benzino-sulphate of potassa.

Origin of Fibre in Puddled Iron.—M. H. le Chatellier.—The grain or absence of fibre is generally produced by the fusibility of the manganiferous or alkaline scoriæ by the softness of carburetted or phosphuretted iron when heated, and by the high temperature at which the puddling is conducted: the fibre, on the other hand, results from the sparing fusibility of partially peroxidised scoriæ, and from the comparatively low temperature of the puddling.

New Crystalline Organic Compound.—M. D. Loiseau.—The newly discovered body has received the name of raffinose. Its elementary composition is—

Carbon	36.30
Hydrogen	7.07
Oxygen	56.63

100.00

corresponding to the formula C₆H₇O₇, or to one of its multiples. It is almost devoid of sweetness; its rotatory power when dissolved in water is greater than that of sugar.

Crystalline System of Several Substances presenting Optical Anomalies.—M. E. Mallard.—The crystalline bodies in question are amphigene, analcime, boracite, senarmontite, and apophyllite.

Bulletin de la Société d'Encouragement pour l'Industrie Nationale, No. 29, May, 1876.

This issue contains no chemical matter.

Moniteur Scientifique, du Dr. Quesneville, April, 1876.

Review of Physics.—M. R. Radau.—This includes a paper on the phenomena produced by the concurrence of two sounds; a notice on a "diapason with variable sound;" a paper on the abnormal dispersion of light; and one on the relations between light and electricity.

Progress of the Manufacture of Artificial Colouring Matters.—M. A. Wurtz.—This memoir, taken from the fifth volume of the Official Report of the French Commission at the Vienna Exhibition, gives an account of the preparation of rosanilin and its congeners, of the manufacture of nitrobenzine and aniline, of nitrotoluenes and toluidins, of violanilin and Coupier's blue, of soluble blues, &c.

Pathological Chemistry.—M. A. Commaille.—A medico-chemical paper on suppurating pancreatitis, on jaundice from retention of bile, and on diabetes mellitus. The author gives instructions for the analysis of the tissues and secretions.

Black Inks.—M. E. H. Viedt.—A very long memoir, giving a full account of the manufacture of black inks.

Manufacture of Extract of Indigo.—M. Max Roesler.—An interesting paper, too long for insertion. The author dissolves the indigo in a mixture of 9 parts fuming sulphuric acid to 2 of the common monohydrated acid. He takes 500 grms. of dry powdered indigo to 2 $\frac{3}{4}$ kilos. of mixed acid, and he adds the acid by two equal successive portions to the indigo.

History of the Manufacture of Turkey-Red.—Theodore Chateau.—A continuation.

Report on the Process of Faure and Kessler for the Manufacture of Sulphuric Acid.—Already noticed.

MISCELLANEOUS.

The Loan Collection at South Kensington.—The conferences in connection with the Loan Collection of Scientific Apparatus at South Kensington have been highly successful. In an early number we shall give abstracts of the papers contributed to the Chemical Section, which was opened on Thursday, the 18th inst., by

an address from the President, Dr. Frankland, F.R.S., Dr. J. H. Gilbert delivered an address on "Some Points Connected with Vegetation;" Mr. W. F. Donkin described Sir Benjamin Brodie's ozone apparatus; and Prof. Andrews gave an account of his investigations on the physical constitution of gases. The second and final meeting of the Section was on Tuesday last, when Dr. Frankland read an important communication from M. Frémy on the Diminution of Scientific Research. Addresses were also delivered by Prof. Roscoe, "On Vanadium and its Compounds;" by Prof. Guthrie, "On Cryohydrates and Water of Crystallisation;" by Prof. Williamson, "On the Manufacture of Steel;" by Mr. W. C. Roberts, "On the Apparatus used by the late Prof. Graham in his Researches;" by Mr. W. N. Hartley, "On Liquid Carbonic Acid in the Cavities of Crystals;" and by Prof. Gladstone, "On the Electrolysis of Organic Compounds with the Copper-Zinc Couple."

PATENTS.

ABRIDGMENTS OF PROVISIONAL AND COMPLETE SPECIFICATIONS.

Improvements in the production of ammonia. A. M. Clark, Chancery Lane, Middlesex. (A communication from F. Maxwell-Lyte, Paris.) February 16, 1875.—No. 563. The invention consists in the special reaction of nascent hydrogen produced in presence of a triad or pentad element and nitrogen for the production of ammonia by synthesis of its elements.

Improvements in filters and percolators. W. B. Gething, Fleet Street, London. February 16, 1875.—No. 566. I form the body of the filter or percolator in the form of an inverted cone, into the bottom of which I insert or place a piece of perforated metal or other material to act as a strainer to the said filter or percolator. I place a metal cover with screw-cap inserted, and at the bottom of the said filter I put a funnel, so that it can be put into a bottle or other narrow mouthed vessel. The said filter or percolator may be made of tinned iron or other metal, and japanned or otherwise ornamented to protect it from damage or corrosion. The advantages I claim are that it (the filter) will always keep the liquor free from dust or dirt or evaporation while in process of filtration.

Improvements in the method or means employed in treating and clarifying sewage or other impure waters. S. Hallsworth, Armley, and R. Bailes, Woodhouse Carr, both near Leeds, York. February 17, 1875.—No. 573. First mixture—Copperas, dry copperas, copperas bottoms, copperas sediment, the sediment that subsides from manufacture of nitrate of iron, the spent residue of iron pyrites, also spent pyrites. Second mixture—Spent residue of iron pyrites, iron ore or slag saturated with liquor from beds of iron pyrites at copperas works, or with diluted sulphuric, or muriatic, or diluted nitric acid. The sewage or other matter is run into tank, to which is added slacked lime or gas-lime, clarifying mixture No. 1, or mixture No. 2; the whole is agitated, allowed to settle, and the pure water run off.

An improved process and apparatus for treating vegetable substances for the extraction of fatty matter, and for the manufacture of spirits and fermented liquids. A. Manbré, Baker Street, Portman Square, Middlesex. February 17, 1875.—No. 579. My improved process is for extracting the fatty matter contained in fruits, cereals, seeds, nuts, roots, and other vegetable substances containing fatty, starchy, or saccharine matters, thereby freeing the starchy and saccharine matters from the fatty matter, and rendering them more suitable for the production of purer and better flavoured spirit and wine, beer, and other fermented beverages.

Improvements in the manufacture of steel. J. Noad, Richmond House, Plaistow, Essex. February 18, 1875.—No. 592. This Provisional Specification describes taking iron or steel obtained by a process of chemical deposition, and placing a small quantity of such iron or steel in a crucible, and pouring upon it cast-iron or other metal melted in a cupola furnace, so as to fill the crucible. The crucible is then closely covered, and the metal is maintained at a high temperature for some time, and then cast into ingots.

A new process for bronzing or giving a metallic appearance to textile fabrics. W. Thackrah, Dewsbury, York. February 19, 1875.—No. 608. The novelty of the invention consists in producing upon piled fabrics, which have been previously dyed, a bronzed or metallic appearance by chemical agency, viz., subjecting such fabrics to various baths composed of tannic acid, picric acid, and then a solution of nitrate of tin and muriate of copper. Finally, the fabrics are boiled in a solution of aniline.

Improvements in the manufacture of colouring matter suitable for dyeing and printing. A. Kottgen, Old Broad Street, London. (A communication from A. Gauhe, Barmen Eitorf, Rhenish Prussia.) February 20, 1875.—No. 619. The process consists in heating the substance known to chemists under the name of anthracen with concentrated or fuming sulphuric acid to a temperature from between 240° to 260° C. until, in diluting a small portion of it with water, no more precipitate is produced. The anthracen used should be as pure as

possible, because the purer the anthracen the less sulphuric acid is required for its perfect solution. The quantity of sulphuric acid varies if the ordinary concentrated acid is used, from between 10 to 20 parts of the weight of anthracen used, if fuming acid be employed, from between 3 to 10 parts of the weight of anthracen. The resulting liquid has to be dissolved in about fifteen to twenty times of its volume of water and neutralised with caustic lime or carbonate of lime. After filtration, the liquid, which contains now the lime salts of what I call the sulpho-acids of anthracen, is precipitated with carbonate of soda. The soda salts of the above sulpho-acids received in this way are heated with two to three times their weight of caustic soda or potash, or a mixture of both, in a suitable vessel to a temperature from between 180° to 260° C. until, in dissolving a portion of the fused mass in boiling water, the colour ceases to become more intense. The fused mass is then dissolved in boiling water, and precipitated with hydrochloric, or sulphuric, or any other convenient acid. The precipitate is collected on a filter, washed with water, and is then suitable for use in dyeing and printing, in the same manner as preparations of madder.

Improvements in filtering and purifying water. O. V. Morgan, Kensington, Middlesex, and G. R. Moelchaerek, Battersea, Surrey. February 20, 1875.—No. 625. This invention consists in the special use of calcium carbonate in addition to or in combination with other filtering media, either as forming part of the filter or in the form of cups or vessels to be used in conjunction with filters of any description, or by themselves, more particularly for the purpose of removing lead from its solution in water to be filtered.

MEETINGS FOR THE WEEK.

SATURDAY, 27th.—Physical, 3. "On Selective Absorption," by W. Ackroyd. "On an Atmospheric Phenomenon in Ceylon," by Rev. R. Abbay.

TUESDAY, 30th.—Society of Arts, 8. (African Section). "The Development of Central Africa," by Edward Hutchinson.

— Royal Institution, 3. "Wheatstone's Discoveries," by Prof. W. G. Adams.

THURSDAY, June 1st.—Royal Institution, 3. "On Voltaic Electricity," by Prof. Tyndall, D.C.L., LL.D., F.R.S.

— Royal, 4. Election of Officers.

— Royal, 8.30.

— Royal Society Club, 6.30.

— Chemical, 8. "On Hematine and Phosphorised Compounds contained in Blood Corpuscles," by Dr. Thudichum and Mr. Kingzett. "On the Liquid Carbon Dioxide from Different Sources," by W. N. Hartley. "Note on the General Applicability of Frankland and Armstrong's Combustion Method to the Estimation of Carbon and Nitrogen in Organic Compounds," by Dr. Thudichum and Mr. Kingzett. "(I.) On Peroxides; (II.) On the Estimation of Nitrogen; (III.) On Chromic and Perchromic Acids," by Thomas Fairley. "On Aluminium Nitride," by Prof. Mallet. "Chemical Studies," by Prof. Dewar. "On the Volumetric Estimation of Mercury," by Prof. Tuson and Mr. Neison.

FRIDAY, 2nd.—Royal Institution, 9. "Recent Discoveries about Vanadium," by Prof. Roscoe.

— Geologist's Association, 8.

SATURDAY, 3rd.—Royal Institution, 3. "On King Arthur's Place in English Literature," by Prof. Morley.

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AIR AND ITS RELATIONS TO LIFE, the substance of a Course of Lectures delivered at the Royal Institution of Great Britain in 1874, with some Additions. By WALTER NOEL HARTLEY, F.C.S., Demonstrator of Chemistry at King's College, London. Second Edition, revised.

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UNIVERSITY OF LONDON.

Notice is hereby given, that the next Half-Yearly Examination for MATRICULATION in this University will commence on MONDAY, the 26th of JUNE, 1876. In addition to the Metropolitan Examination, Provincial Examinations will be held at St. Cuthbert's College, Ushaw; Stonyhurst College; St. Stanislaus College, Tullamore; Owens College, Manchester; Queen's College, Liverpool; and Queen's College, Birmingham.

Every Candidate is required to transmit his Certificate of Age to the Registrar (University of London, Burlington Gardens, London, W.) at least fourteen days before the commencement of the Examination.

May 22nd, 1876.

WILLIAM B. CARPENTER, M.D.,
Registrar.

THE CHEMICAL NEWS.

VOL. XXXII. No. 862.

ON THE CLASSIFICATION OF THE CYANOGEN COMPOUNDS.

By SYDNEY LUPTON, M.A.

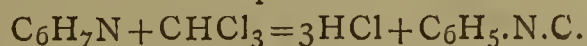
IN a suggestive paper published in the CHEMICAL NEWS (vol. xxxiii., p. 141) Mr. Skey calls attention to the present state of our knowledge of the relationships of cyanogen, and draws the conclusion that it is far more nearly similar in its properties to oxygen than it is to chlorine. It may be of value to attempt to classify a few of the more important cyanides and then to examine the arguments brought forward by Mr. Skey.

From the intimate connection between the various members of the cyanogen group, they are, in general, considered together as a separate section of organic chemistry.

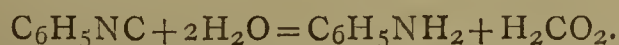
In a general review of these bodies the first thing which strikes us is the large number of compounds in which CN plays the part of a simple monatomic element; the second is the numerous complicated compounds containing the group CN more than once. Considering, then, the cyanogen compounds as a group to themselves, it is evidently of advantage to use as our means of classification the groups CN, and not, as is usually the case, the various bodies with which it may happen to be combined; just as the paraffins, for example, are classified by the number of atoms of carbon which they contain. Our next step is to enquire into the constitution and combining powers of the group CN on which our classification is to be based.

Carbon is in almost all its compounds a tetrad, *e.g.*, CH₄, CCl₄, &c.; it is, however, sometimes a dyad CO. Again, carbon may saturate some of its affinities by combining with itself C₂H₅Cl, C₂H₄, &c.

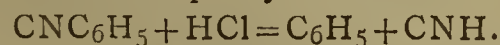
Nitrogen in many of its combinations plays the part of a pentad NH₄Cl, NH₂(CH₃)HBr, &c., in others it acts as a triad NH₃N(CH₃)₃, &c.; in one of its combinations, NO, it is apparently a dyad, and in another, NOH, a monad. Like carbon it possesses considerable power of self-saturation. From the more general valencies of carbon and nitrogen we should expect cyanogen to be tetradic carbon combined with either pentadic or triadic nitrogen. There was considerable doubt as to which of these formulæ belonged to cyanogen until Dr. Hofmann's brilliant discovery of the iso-cyanides, bodies clearly belonging to the former class. They are formed by distilling a mixture of an alcoholic ammonia base and chloroform with alcoholic potash—



The iso-cyanides are scarcely acted on by alkalis, but break up under the action of acids into formic acid and an alcoholic ammonia—



Under similar conditions ordinary phenyl-cyanide breaks up into prussic acid and phenyl-chloride—



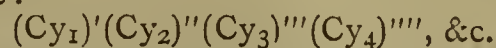
Further confirmation of this view that cyanogen consists of tetradic carbon and triadic nitrogen is afforded by M. Gaultier's and Gal's discovery of the hydro-chlorates, hydro-bromates, and hydriodates of the cyanides and cyanates. In these bodies evidently the two atomicities of nitrogen previously saturated by one another are now saturated by hydrogen and chlorine, &c., just as we have ammonia and its congeners forming the ammonium salts. In the great majority of the cyanogen compounds we may leave out of consideration the fact that nitrogen is a pentad, and regard it simply as a triad in combination

with the tetrad carbon. We thus see how in so many of its compounds cyanogen acts as a monad exactly analogous to chlorine.

But further, just as we find nitrogen combining with itself in the oxides of nitrogen and in the azo-phenyl compounds; and as we find carbon combining with itself to form the connecting link of an organic body, so we may consider that cyanogen combines with itself to give us the more complicated cyanides; and as the addition of each atom of carbon makes an addition of two to the saturation-power of a molecule, so the addition of each group CN causes the addition of one to the saturation-power of the molecule.

It is of course possible that the addition of the cyanogen groups takes place by the combination of the two self-saturated atomicities of nitrogen, and this may account for certain instances of isomerism, *e.g.*, paracyanogen; but this point we have at present no means of deciding.

Leaving out of consideration the iso cyanides as not strictly belonging to the cyanogen group, we may arrange the cyanogen compounds into different classes as they contain once, twice, three times, &c., the group CN' = Cy'; thus we have:—



By this of course we entirely throw over the system of types, and classify by valencies which are in reality at the root of that system. What is the so-called water type but two monad groups each united to one dyad group, and so on for the other types? It must be admitted that in several cases this method of classification places divisions where perhaps there ought to be none, *e.g.*, between melamine and melam, and between potassium cyanide and zinc cyanide; but I venture to hope that it is at least no worse in this respect than any other system of classification which has been proposed.

Many of the cyanogen compounds are so difficult to examine that the difficulty of the classification consists as yet in their true empirical formula, and this can only be overcome by a very extended series of accurate analyses, *e.g.*, the nitroprussides. We know of cyanogen compounds which contain the group CN from 1 to 18 times, but many of the intermediate groups are still wanting.

It may be of interest to arrange the more important cyanides under their respective classes, putting in but few of the cyanides of organic bodies, as they are, in general, simple and would cause useless repetition.

Monocyanides (Cy₁)'.

CyH, prussic acid	CyNH ₂ , cyanamide
CyCH ₃ , methyl cyanide	CyOH, cyanic acid
CyAg, silver cyanide	CyOK, potassium cyanate
CyK, potassium cyanide	CyONH ₄ , ammonium cyanate
CyNH ₄ , ammonium cyanide	CyOC ₂ H ₅ , ethyl cyanate
CyCy, cyanogen	CySH, sulphocyanic acid
CyCl, cyanogen chloride	CySK, potassium sulphocyanide
CyBr, cyanogen bromide	CySNH ₄ , ammonium sulphocyanide
CyI, cyanogen iodide	

The three chief groups contained in this class are the cyanides of monatomic radicals, the cyanates, and the sulphocyanates. Cyanogen itself being the means of classification ought possibly not to appear in this list, but it is put in for the sake of uniformity. The double cyanide and nitride of titanium presents many points of difficulty; it possibly belongs to this class, its formula being written Cy[Ti^{IV}(TiN)₃], but further analyses are wanting to establish its formula conclusively.

Dicyanides (Cy₂)''.

Cy ₂ Cl ₂ (?), cyanogen chloride	Cy ₂ (OH) ₂ , dicyanic acid
Cy ₂ KAg, silver and potassium cyanide	Cy ₂ Se, cyanogen selenide
Cy ₂ Cu, cupric cyanide	Cy ₂ S, cyanogen sulphide
Cy ₂ Cu ₂ , cuprous cyanide	Cy ₂ HSH, cyanogen sulphhydrate

Cy_2Hg , mercuric cyanide	$\text{Cy}_2\text{H}_2(\text{SH})_2$ (?), cyanogen disulphhydrate
Cy_2Fe (?), ferrous cyanide	$\text{Cy}_2\text{SH.S.SH}$, persulphocyanic acid
Cy_2Co , cobaltous cyanide	$\text{Cy}_2\text{S}_2\text{Cu}$, cupric sulphocyanide
Cy_2Ni , nickelous cyanide	$\text{Cy}_2\text{S}_2\text{Cu}_2$, cuprous sulphocyanide
Cy_2KAu , potassio-aurous cyanide	
$\text{Cy}_2(\text{NH}_2)_2$, dicyano-diamide	
$\text{Cy}_2(\text{NH}_2)\text{OH}$, dicyanamic acid	

We are here met by one of the first difficulties of our classification. Should such a body as Cy_2Cu be placed here or in the first class, that is to say are the cyanogen groups directly connected together, or is each only connected with copper? It appears to me, looking at the ease with which the cyanogen groups coalesce, that the first case is probably the true one; but the point needs further investigation.

Recent determinations of the vapour density of liquid cyanogen chloride seem to point to the formula CyCl , but it is possibly a case of dissociation.

The dicyanic acid mentioned is of course not fulminic acid, but the true dicyanic acid discovered by Poensgen.

Cyanogen disulphhydrate and persulphocyanic acid are very difficult bodies to classify. It seems possible that the two latent nitrogen affinities of the cyanogen may here come into play.

Tricyanides (Cy_3)ⁱⁱⁱ.

Cy_3Cl_3 , cyanuric chloride	$\text{Cy}_3(\text{NH}_2)_3$, cyanuramide (melamine)
$\text{Cy}_3\text{Cl}_2\text{H}$, chlorocyanic acid	$\text{Cy}_3(\text{NH}_2)_2\text{OH}$, ammeline
$\text{Cy}_3(\text{OH})_3$, cyanuric acid	$\text{Cy}_3(\text{NH}_2)(\text{OH})_2$, melanurenic acid
$\text{Cy}_3\text{O}_3\text{H}_2\text{K}$, { hydrogen and potassium cy- anurates	$\text{Cy}_3(\text{NH}_2)(\text{SH})_2$, sulpho- melanurenic acid
$\text{Cy}_3\text{O}_3\text{Na}_3$, sodium cyanurate	$\text{Cy}_3(\text{NH}_2)_2\text{Cl}$, chloro-cyanamide
$\text{Cy}_3\text{O}_3\text{H}(\text{NH}_3.\text{cu})_2.\text{H}_2\text{O}$, $\text{Cy}_3\text{O}_3\text{H}_2(\text{NH}_3.\text{cu})$, hydrogen and cupram- monium cyanurates	
$\text{Cy}_3\text{SH.S}_2$, pseudosulphocyanogen	
Cy_3CH , cyanoform	
Cy_3P , phosphorus cyanide	

The most important groups belonging to this class are the cyanurates and the amidated bodies.

Some doubt has recently been thrown on the existence of the chlorocyanic acid of Wurtz. It seems very possible from the formula of normal silver cyanurate—



that the true formula of cyanuric acid is $\text{Cy}_6\text{O}_6\text{H}_6$; but the evidence is yet hardly sufficient to warrant such a change. The notation cu is used for an equivalent of copper to simplify the formulæ. The formulæ of the amidated compounds are but slightly changed from those given by Kekulé.

Do-deka-cyanides (Cy_{12})^{xii}.

$\text{Cy}_{12}\text{Fe}_2\text{H}_8$, hydrogen ferrocyanide
$\text{Cy}_{12}\text{Fe}_2\text{K}_8$, potassium ferrocyanide
$\text{Cy}_{12}\text{Fe}_2\text{Fe}_2\text{K}_4$, potassio-ferrous ferrocyanide
$\text{Cy}_{12}\text{Fe}_2\text{Ca}_4.\text{H}_2\text{O}$, calcium ferrocyanide
$\text{Cy}_{12}\text{Fe}_2\text{Sr}_2\text{K}_4.\text{H}_2\text{O}$, potassio-strontium ferrocyanide
$\text{Cy}_{12}\text{Fe}_2(\text{NH}_3.\text{cu})_8.\text{H}_2\text{O}$, cuprammonium ferrocyanide
$\text{Cy}_{12}\text{Fe}_2(\text{NH}_3.\text{ni})_8$, nickelammonium ferrocyanide
$\text{Cy}_{12}\text{Fe}_2\text{Fe}(\text{NH}_3.\text{fe})_4$, ferric ferrammonium ferrocyanide
$\text{Cy}_{12}\text{Ni}_2\text{K}_8$, potassium nickelocyanide

Tetra-cyanides (Cy_4)^{iv}.

Cy_4ZnK_2 , potassio-zinc cyanide	$\text{Cy}_4(\text{NH}_2)_2\text{H}_2$, hydrazulmin
$\text{Cy}_4\text{Cu}_2\text{K}_2$, potassio-cuprous cyanide	$\text{Cy}_4(\text{NH}_2)\text{H}_2\text{OH}$, hydrazulmoxin
Cy_4CoK_2 , potassio-cobaltous cyanide	$\text{Cy}_4\text{H}_2(\text{OH})_2$, azulmic acid
Cy_4AuK , potassio-auric cyanide	
Cy_4PtH_2 , hydrogen platino-cyanide	
Cy_4PtK_2 , potassium platino-cyanide	
Cy_4PtCu , cupric platino-cyanide	
Cy_4PdK_2 , potassium palladio-cyanide	
Cy_4SPtK_2 , potassio-platinous sulphocyanide	

The azulmic acid mentioned is that of Pelouze and Richardson; it is either isomeric or identical with mycomelic acid. It seems more probable that mycomelic acid has the formula $\text{Cy}_8\text{H}_4(\text{OH})_4.\text{H}_2\text{O}$.

Penta-cyanides (Cy_5)^v.

$\text{Cy}_5\text{Fe}_2\text{K}$, potassio-ferrous cyanide of Städeler.

Hexa-cyanides (Cy_6)^{vi}.

Cy_6PtBa , barium platinic cyanide	$\text{Cy}_6(\text{NH}_2)_4(\text{NH})''$ melam
Cy_6RuK_2 , potassio-ruthenic cyanide	$\text{Cy}_6(\text{NH}_2)_3(\text{OH})_3$, ammelide
Cy_6OsK_2 , potassio-osmic cyanide	$\text{Cy}_6(\text{N})'''(\text{OH})_3$, cyameluric acid
Cy_6IrK_2 , potassio-iridic cyanide	
Cy_6RhK_2 , potassio-rhodic cyanide	
$\text{Cy}_6\text{S}_6\text{Pt}_6\text{K}_2$, potassio-platinic sulphocyanide	
$\text{Cy}_6\text{O}_6\text{Ag}_6.\text{H}_2\text{O}$, silver cyanurate	
$\text{Cy}_6\text{O}_6\text{Pb}_3.\text{H}_2\text{O}$, lead cyanurate	
$\text{Cy}_6\text{Fe}_2.\text{H}_2\text{O}$, ferric cyanide	

Ferric cyanide is obtained according to Wyruboff when potassium ferrocyanide is boiled with ammonium chloride.

Hepta-cyanides (Cy_7)^{vii}.

Wanting.

Octo-cyanides (Cy_8)^{viii}.

$\text{Cy}_8\text{Fe}_2\text{Fe}_4.\text{H}_2\text{O}$, magnetic cyanide of iron of Pelouze
$\text{Cy}_8\text{Cu}_2\text{K}_6$, β potassio-cuprous cyanide
$\text{Cy}_8(\text{NH}_2)_2\text{O}_2(\text{OH})_2.\text{H}_2\text{O}$, azoulmoxin.

Ennea-cyanides (Cy_9)^{ix}.

$\text{Cy}_9\text{N}(\text{NH})_3$, mellon
$\text{Cy}_9\text{H}_6(\text{OH})_3$, azulmic acid of Gautier
$\text{Cy}_9\text{H}_6(\text{OH})_2\text{OAg}$, silver azulmate.

Deka-cyanides (Cy_{10})^x.

$\text{Cy}_{10}\text{Pt}_2\text{K}_4$, potassium platino-cyanide
$\text{Cy}_{10}\text{Fe}_2(\text{NO})_2\text{Na}_4$, sodium nitro-prusside
$\text{Cy}_{10}\text{Fe}_2(\text{N}_2\text{O}_3)\text{Na}_4$, sodium nitro-prusside (Hadow).

$\text{Cy}_{12}\text{Fe}_2\text{H}_6$, hydrogen ferricyanide
$\text{Cy}_{12}\text{Fe}_2\text{K}_6$, potassium ferricyanide
$\text{Cy}_{12}\text{Fe}_2\text{Ca}_3$, calcium ferricyanide
$\text{Cy}_{12}\text{Fe}_2\text{Ni}(\text{NH}_3.\text{ni})_2$, nickelic nickelammonium ferricyanide
$\text{Cy}_{12}\text{Fe}_2\text{K}_3\text{Na}_3$, potassium and sodium ferricyanide
$\text{Cy}_{12}\text{Fe}_2\text{Ag}_6.3\text{NH}_3$, ammonio-silver ferricyanide
$\text{Cy}_{12}\text{Fe}_2\text{Fe}_3$, ferrous ferricyanide
$\text{Cy}_{12}\text{Fe}_4$, ferric ferricyanide (Prussian green)
$\text{Cy}_{12}\text{Co}_2\text{K}_6$, potassium cobalto-cyanide
$\text{Cy}_{12}\text{Mn}_2\text{K}_6$, potassium manganic cyanide
$\text{Cy}_{12}\text{Cr}_2\text{K}_6$, potassium chromic cyanide

The formula I have used for the nitro-prussides is double that formerly given; if Hadow's formula be correct, this doubling follows of necessity; if, as Städeler's researches seem to show, that generally used be correct, the doubled formula given above still seems the best from considerations of the atomicity of iron and of the great analogy of the nitro-prussides with the ferricyanides.

Hendeka-cyanides (Cy_{11})^{xi}.
Wanting.

Do-deka-cyanides (Cy_{12})^{xii}. (See preceding page).

Octo-deka-cyanides (Cy_{18})^{xviii}.
 $\text{Cy}_{18}(\text{Fe}_3)^{\text{vi}}\text{Fe}_4$ (?), Prussian blue.

Besides theoretical reasons, based upon the atomicity of iron, for considering the ferro- and ferri-cyanides as containing the group CN twelve times in each molecule; the single molecule of water found by Wyruboff in calcium ferrocyanide dried at 100°, and the three molecules of water found by the same observer in potassio-strontium ferrocyanide dried at 110°, the constitution of Laurent's potassium and sodium ferricyanide, and finally the constitution of the ammonio-silver ferricyanide furnish powerful arguments tending in the same direction.

Nickel seems to form nickelo-cyanides similar to the ferrocyanides; cobalt, manganese, and chromium form cobalti-cyanides, mangani-cyanides, and chromi-cyanides similar to the ferri-cyanides.

The formula of Prussian blue has long been a *vexata quæstio* among chemists; that given above is the older and perhaps more generally received.

(To be continued)

REPORT ON THE DEVELOPMENT OF THE CHEMICAL ARTS DURING THE LAST TEN YEARS.*

By Dr. A. W. HOFMANN.

Progress in the Artificial Production of Cold and Ice.

By Dr. H. MEIDINGER.

(Continued from p. 214.)

A FEW singular proposals for effecting a reduction of temperature may be finally mentioned. J. B. Toselli, of Paris,† causes a spiral pipe to revolve in a vessel of water, from which it simultaneously, during each rotation, raises a certain quantity of water and transfers it to an adjacent vessel, whence it flows back through a worm into the former. The spiral, during its revolutions, has its entire surface moistened. A ventilator drives air against it, evaporates the adhering layer of moisture, and thus lowers the temperature of the tube and of the water it contains. A refrigeration of from 2.7° to 18.3° C. is said to be thus produced according to the weather. In the second vessel, which is traversed by a worm containing the cold water, is placed the liquid to be cooled, such as worts of beer, artificial mineral waters, &c. The effect produced can be but trifling, and depends entirely on the state of the weather, and on the amount of atmospheric moisture, which is never wanting. A psychrometer fixed in the place where the experiment is to be made will show the result beforehand with tolerable accuracy.

Ballo,‡ of Pest, produces cold by forcing very finely divided air through bisulphide of carbon. The condensation of the liquid needful for its recovery is a hindrance, on which, in fact, the entire project must be wrecked. A recovery of the bisulphide of carbon by any other means than by condensation and refrigeration of the air saturated therewith is, in the absence of suitable solvents,

impossible. Even by this means it would involve much difficulty and a great expenditure of force, and would bring us back to the principle of the air machine. In this direction the problem is practically incapable of solution.

Preservation of Ice.

As a supplement to our report on the principles of the artificial production of ice, and on the apparatus hitherto devised for this purpose, a few words must be added on the arrangements for the preservation of cold in the concentrated form of ice. This is a question of great practical importance. Ice machines, however they may be eventually improved and their effect increased, will never, in the more northern parts of the temperate zone, where a moderately cold winter with frost is generally experienced, acquire importance enough to meet the demand even approximately. They will serve merely as valuable substitutes to render us independent of the fickleness of the seasons. Even in more southern regions where ice machines are the only source for obtaining ice, they must work to stock and fill magazines, since the demand does not go hand in hand with the production, but varies with the weather. There is in general no conception of the quantities of ice which certain trades require, and which are consumed in domestic life where its use has grown into a necessity. In 1866 the quantity of ice consumed in New York and its vicinity amounted to 250,000 tons (254,015 metric tons) or 5 cwts. per head. The weight stored up was 543,000 tons (551,721 metric tons), whilst the capital employed in the trade amounted to 2,160,000 dollars. The retail price was for quantities of 5 to 12 kilos. 4 pfennige* per kilo., but for quantities of 1 to 10 cwts. only one shilling per cwt.

In 1871 a company in Berlin, the "North German Ice Works," stored up 600,000 cwts. of ice, and delivered it to subscribers at 77 pfennige per cwt. The quantities of ice consumed in brewing may be learned from the following data, which the author obtained in 1869 from Dreher's brewery at Klein Schwechat, near Vienna:—This establishment brewed, in 1867, 483,150 Viennese eimers, = 273,463 hectolitres, and stored up 515,600 cwts. (28,874,219 kilos) of ice. In the following year these numbers rose to 492,499 eimers (278,754 hectolitres) of beer and 563,058 cwts. (31,531,924 kilos.) of ice. On an average 1 cwt. of ice is used per eimer (56.6 litre). In a prolonged frost of 2 months this quantity can be procured at the cost of 7 Austrian kreutzers (14 pfennige) per cwt. In shorter periods of cold the price rises to from 10 to 12 kreutzers, to which must be added 1 kreutzer for shovelling into the ice cellars. In mild winters the ice is brought in part from Styria; as the cold weather in 1869 set in late, 26,000 cwts. (1,456,031 kilos.) were procured from there, costing, by the time it reached the brewery, 115 florins per 200 cwts.

(To be continued.)

SCHIEBLER'S PROCESS FOR SACCHAROSE ESTIMATION.

By ROBERT FRAZER SMITH.

WITH reference to the very able abstract of Professor Gunning's remarks in his report upon Scheibler's process for the determination of the crystallisable cane sugar in raw sugars given by Mr. Humphrey in the *American Chemist* and copied in the *CHEMICAL NEWS* (vol. xxxiii., p. 205), perhaps the enclosed table of results obtained by the use of that process upon Colonial sugars by Lotmann may be of interest to those chemists who have not had time or opportunity afforded for giving it a fair trial. Owing to the results being uniformly higher than those

* "Berichte über die Entwicklung der Chemischen Industrie Während des Letzten Jahrzehends."

† Toselli, *Mech. Mag.*, 1872, 433. *Dingl. Pol. Journ.*, ccv., 28.

‡ Ballo, *Dingl. Pol. Journ.*, ccxi., 345.

* The German "pfennig" is about the tenth part of an English penny.

Table of the Refining Values (Raffinations Werthes) of Different Raw Sugars.
100 Parts of the Raw Sugar Contain

Name of the Sugar used.	Crystallisable Sugar.	Glucose.	Ash.	Water.	Organic and Insoluble Matters.	Nett Sugar. Ash $\times 5$, Glucose $\times 1$.	Refine Value Scheibler.	Polarisation of the Washed Product.	Remarks.
1.	2.	3.	4.	5.	6.	7.	8.	9.	
Best refined	100.0	—	—	—	—	—	99.80	—	Control trial.
"	100.0	—	—	—	—	—	99.90	—	"
Java, 15	98.2	0.47	0.234	0.20	0.896	96.56	95.90	99.6	
" 15	94.6	1.97	0.320	1.95	1.160	91.03	91.25	99.7	
" 15	93.0	2.56	0.220	2.90	1.320	89.34	88.67	99.6	
" 14	97.0	1.28	0.310	0.70	0.710	94.17	94.32	99.5	
" 12	93.5	3.00	0.240	1.80	1.460	89.30	88.52	99.0	
" 11	95.2	1.50	0.670	1.25	1.380	90.35	91.75	99.1	
" 8	93.5	3.00	0.770	1.70	1.030	86.65	88.60	99.1	
" 6	89.5	4.38	0.936	2.90	2.284	80.42	82.18	98.0	1 p. c. sand.
Surinam	86.5	4.05	1.440	5.60	2.410	75.25	81.26	98.0	
"	87.0	3.51	1.190	5.70	2.600	77.54	77.85	97.0	
"	89.0	4.01	1.070	4.32	1.600	79.64	83.12	98.0	
Bahia	86.0	2.35	2.420	4.21	5.020	71.55	81.70	97.1	
Cuba	85.7	3.87	1.350	5.00	4.080	75.08	79.53	96.0	
Madras	82.0	6.10	2.650	6.10	3.150	62.65	76.15	93.0	
Surinam	90.5	2.98	0.900	3.58	2.040	83.02	84.12	97.0	
Porto Rico	89.0	3.40	0.480	3.61	3.510	83.20	82.91	98.0	
Manilla	87.0	5.10	1.820	4.00	2.080	72.80	78.10	94.0	} Much sand and lime.
"	83.0	5.26	2.130	5.91	3.700	67.09	72.17	94.5	
"	85.6	4.41	1.720	4.53	3.740	72.59	81.03	94.0	
Beets									
1st product	98.0	—	0.540	1.15	0.310	95.30	95.25	99.9	
"	96.0	—	1.080	1.98	0.940	90.60	91.15	99.7	
"	96.5	—	1.170	2.50	0.830	90.65	91.12	99.5	
2nd product	94.5	—	1.570	2.00	1.930	85.65	88.90	99.0	
"	93.5	—	1.720	2.28	2.500	84.90	89.60	99.5	
"	93.7	—	1.670	3.00	1.630	85.35	87.53	99.4	
"	95.5	—	1.440	1.28	1.780	88.30	89.54	99.5	
"	95.0	—	1.680	1.57	1.750	86.60	89.87	99.2	
3rd product	87.0	—	4.040	4.98	3.980	66.80	75.12	98.0	

found by the French method, it probably has no chance of being adopted by the buyers of raw sugars or those acting for them; but in the private laboratories of refineries the information such a determination of the actual quantity of saccharose present in any sample gives may induce some chemists to procure or erect the apparatus. The Table will show that it is absolutely indispensable to dry all samples containing more than 2 per cent of moisture previous to treatment, in which case the ether will be unnecessary. Upon sugars containing much caramel or other colouring matter an alcoholic 3 per cent hydrochloric acid solution will be found to answer better than the acetic, giving at the end a perfectly white product.

With regard to the "amorphous sugar" it might be well to wait for more light before accepting the existence of such a body. The sugar obtained by precipitating with alcohol from a sample of molasses which has stood for a year without any signs of crystallisation, presents the appearance of an impalpable powder, and from its state of minute division is slightly more soluble in alcohol than the crystalline cane sugar of commerce, containing 99.8 per cent of saccharose, but on solution in water and evaporation it crystallises in the ordinary form. Crystalline salts dissolved in gum or gelatin behave in exactly the same manner, but no one has yet talked of amorphous common salt or nitre. Molasses containing, on dried product—

Saccharose	75.50
Glucose	0.41
Ash	11.31
Organic matters	12.78

100.00

which a friend kindly sent me from Paris and said to be derived from the residues of the "sucrate of the hydrocarbonate of lime process" on being diluted to 30° B. and

exposed to a freezing mixture to get rid of excess of salts and saccharose, the mother liquor, treated with alcoholic ether (being first concentrated) yields a large crop of what I understand to be the so-called amorphous sugar. But a glass of small power shows distinct crystalline faces, and it yields to water and evaporation, the usual result. I humbly think that the amount of crystallised sugar got from any solution of saccharine matter will be found to depend, other things being equal, upon the value which pertains to the figure denoting the coefficient of viscosity.

Some salts, such as the magnesian, instead of hindering promote sugar crystallisation. In short, the salts which take longest when in solution to transpire through capillary tubes are those which hinder crystallisation most. Hence the alkaline organic salts are the great molasses formers, and hence also the benefit derived from Marguerite's process in the beet fabriques, which adds sufficient hydrochloric acid to transform these into chlorides, the other acids being sent off in the boiling.

The rate at which any syrup travels through a capillary tube (a solution of pure sugar being 100) will express the crystallising capabilities of the sugar contained in it; due regard being had to temperature, pressure, strength, &c., being alike in the various trials. Mr. Humphrey mentions cases in which glucose is present in an optically inactive condition, but, so far as I know, this never occurs in cane sugars, but is so abundantly in date sugar and also the sugar from many fruits.

23, Roselea Drive, Glasgow.

The Loan Collection at South Kensington.—A series of lectures in connection with the above Exhibition have been arranged for the free evenings. The first lecture will be given on Saturday evening next, at 8 o'clock, in the Conference Room, by Prof. Roscoe. The subject is "Dalton's Instruments and what he did with them,"

UNIVERSITY COLLEGE, BRISTOL.

THE operations of this new College will, we are glad to hear, begin in October next in the temporary premises, Park Row, Bristol. It will be remembered that the College is established for the study of science and literature for young people of both sexes above the ordinary school age residing in the West of England and South Wales. The models kept in view are University College, London, and the Owens College, Manchester. The Council consists of Prof. B. Jowett, nominated by the Vice-Chancellor of the University of Oxford; Prof. Stuart, nominated by the Vice-Chancellor of the University of Cambridge; Mr. W. Lant Carpenter, nominated by the Chancellor of the University of London; Prof. H. Smith, by Balliol College, Oxford; Rev. H. B. George, by New College, Oxford; Mr. R. W. Coe, by the Bristol Medical School; Mr. F. N. Budd (Chairman); Mr. W. Proctor Baker (Treasurer); Rev. J. W. Caldicott; Mr. Lewis Fry; Rev. F. W. Gotch; Rev. J. Percival; Mr. G. F. Schacht; and Mr. William Smith, the last eight gentlemen being elected by the subscribers. Out of 106 candidates Mr. Edward Stock, of Clare Street, Bristol, has been elected Secretary. The Council will shortly appoint a Professor of Chemistry. The necessary qualifications are, first, a good teacher; and, second, one who has done and will do original work. The Council offer £300 per year, two-thirds of the Lecture Fees, and one-third of the Laboratory Fees, the College finding the Laboratory in apparatus and chemicals, and they guarantee a minimum emolument of £400. In order to improve the technical education of those engaged in the cloth-making districts in the West of England, the Cloth-workers' Company offered to the promoters of the College the subvention of 500 guineas per year for five years to assist in the establishment of a Department of Textile Industries at the College, and it is understood that special attention will be given both to the principles and detail of the mechanism employed in cloth manufacture, and also to the chemical principles involved in the processes of wool-scouring, dyeing, &c. In addition to the instruction given at the College there will probably be classes conducted in the cloth-working districts themselves, at Stroud and elsewhere.

PROCEEDINGS OF SOCIETIES.

PHYSICAL SOCIETY.

May 27th, 1876.

Professor GLADSTONE, F.R.S., Vice-President, in the Chair.

THE following candidates were elected Members of the Society:—Herbert Taylor, Rogers Field, and Channell Law.

Mr. W. ACKROYD read a paper on "*Selective Absorption*." Two typical experiments were shown upon which a division of selective absorption may be based. In the first, light is transmitted through bichromate of potash at the normal temperature, and again at about 200° C., and the spectrum of the transmitted light is examined. The widening of the absorption-bands, which takes place at the higher temperature, is traced to structural alterations. In the second experiment, light is sent through two thicknesses of the same coloured solution—as, for example, sulphate of copper—and in the greater thickness the absorption-band has widened out; but this is plainly not owing to any structural alteration. That in the first experiment he proposes to term *structural*, and that in the second *transverse* absorption, and he considers that these two kinds have not hitherto been sufficiently distinguished. Certain colour relations which exist among anhydrous binary compounds led the author to the conclusion that

the width of a structural absorption-band bears a direct relation to interatomic distance. The necessity for separating high temperature spectra from low was shown, and the bearing of the subject on the study of organic colouring matters briefly alluded to.

The SECRETARY then read a communication from the Rev. R. Abbay, "*On certain Remarkable Atmospheric Phenomena in Ceylon*." The most striking of these is witnessed from the summit of Adam's Peak, which is a mountain rising extremely abruptly from the low country to an elevation of 7200 feet above the sea. The phenomenon referred to is seen at sunrise, and consists apparently of an elongated shadow of the mountain, projecting westward to a distance of about 70 miles. As the sun rises higher it rapidly approaches the mountain, and appears at the same time to rise before the observer in the form of a gigantic pyramid of shadow. Distant objects may be seen through it, so that it is not really a shadow on the land, but a veil of darkness between the peak and the low country. It continues to rapidly approach and rise until it seems to fall back upon the observer, like a ladder which has been reared beyond the vertical, and the next instant it is gone. Mr. Abbay suggests the following explanation of the phenomenon:—The average temperature at night in the low country during the dry season is between 70° and 80° F., and that at the summit of the peak is 30° or 40° F.; consequently, the low strata of air are much the less dense, and an almost horizontal ray of light passing over the summit must be refracted upwards and suffer total internal reflection, as in an ordinary mirage. On this supposition the veil must become more and more vertical as the rays fall less horizontally, and this will continue until they reach the critical angle, when total internal reflection ceases, and it suddenly disappears. Its apparent tilting over on the spectator is probably an illusion, produced by the rapid approach and the rising of the dark veil without any gradual disappearance which can be watched and estimated. It will be evident that the illumination of the innumerable particles floating in the atmosphere causes the aerial shadow to be visible by contrast. Another interesting phenomenon visible in the mountain districts admits of an equally simple explanation. At times broad beams, apparently of bluish light, may be seen extending from the zenith downwards, converging as they approach the horizon. The spaces between them have the ordinary illumination of the rest of the sky. If we suppose, as is frequently the case, that the lower strata of air are colder than the upper, the reflection spoken of in the case of Adam's Peak will be downwards instead of upwards. If several isolated masses of clouds partially obscure the sun, we may have several corresponding inverted veils of darkness, like blue rays in the sky, all apparently converging towards the same point below the horizon. This latter phenomenon is called by the natives "Buddha's Rays."

Prof. Dr. FOREL, of Morges, Switzerland, then gave, in French, an account of some interesting observations which he has recently made on the periodic waves which take place on the Swiss lakes, and are there called "Seiches." It was long since observed that the waters of most of these lakes are subject to a more or less regular rise and fall, which at times have been found to be as much as 1 or 2 metres. M. Forel has studied this phenomenon in nine different lakes, and finds that it varies with the length and depth of the lake, and that the waves are in every way analogous to those already studied by Prof. Guthrie in artificial troughs, and follow the laws which he has deduced from his experiments. Most of the observations in Switzerland were made on the Lake of Geneva, but that of Neuchatel was found to be best fitted for the study of the subject, possessing as it does an extremely regular geometric form. The apparatus he employed was very sensitive to the motion of the water, being capable of registering the waves caused by a steamboat half an hour after it had passed, and five minutes before its arrival, and was so constructed as to eliminate the effect of common

waves, and to register the motion side by side with a record of the state of the barometer, on paper kept in continuous motion. While he found the duration of waves to be ten minutes at Morges, it was seventy minutes at Geneva, and this is explained by the narrowness of the neck of the lake at the latter place. This period he proves to be independent of the amplitude, and to be least in the shortest lakes. For shallow lakes the period is lengthened, and his observations show that the period is a function of the length and depth, and that longitudinal and transverse waves may coexist, just as Prof. Guthrie has shown to be the case in troughs.

M. FOREL, replying to a question by the President, stated that he considers the origin of the oscillations to be in many cases barometric disturbances, but occasionally blows given by the wind. He has observed that the movements are, as a rule, higher when the barometer is low than when it is high, and he considers that the highest recorded are in all probability due to some convulsion of the earth.

M. BOSANQUET said that the quantity of water flowing into the Lake of Geneva from the Rhône is not a matter of indifference, as this must show itself as a wave on the lake, which would be liable to be confounded with the influence of barometric pressure unless special precautions are taken to avoid such an error. He also considered that in a lake of so complicated a form as that of Geneva observations would be required at at least ten different points, and a complete solution of such a case would be analogous to the study of magnetic intensity in the manner suggested by Gauss.

NOTICES OF BOOKS.

Science made Easy: A Series of Familiar Lectures on the Elements of Scientific Knowledge most required in Daily Life. By THOMAS TWINING. London: Chapman and Hall.

THIS work is not, as the title might possibly seem to imply, one of the many elementary treatises for private study which have recently appeared in such numbers and variety. The lectures are arranged for delivery at popular institutions, schools, &c., on the so-called binary principle. Two persons are required, the *reader*, who gives his undivided attention to the delivery of the text before him, and the *demonstrator*, who, in like manner, is solely concerned with preparing and performing the experiments and exhibiting the specimens or models required in illustration of the subject. This plan is undeniably good for amateur lecturers, who are very apt to become confused amidst their complicated duties and, in consequence, to bungle in their experiments. The object of the course of lectures, the spread of sound scientific information, and yet more of scientific habits of thought among the general public—and especially among the working classes—is most important, and we must wish the author every success in an undertaking of such great national moment. At the same time it must not be forgotten that in the second quarter of the present century public lectures on scientific subjects were delivered in abundance at Mechanics' Institutes and similar establishments all over the kingdom. The lecturers of those days were often able men, good elocutionists, and skilful experimentalists, and for a time lectures on chemistry and certain branches of physics were very popular. Nor can it be denied that real and enduring good was effected. Many men were led to take science home with them to the workshop, and to carry on their daily work on the only sound principle, "theory with practice." At the same time somewhat clearer and more rational views were diffused among the general public. But, despite these admitted benefits the interest in scientific lectures gradually fell off. Subjects of a more "human interest"—*i.e.*, such as required less thought on the part of the audience—took their place in our popular scientific institutions, and the

scientific lecturer of the thirties and forties became, as a species, extinct. We, therefore, fear that as a "commercial speculation" the delivery of Mr. Twining's lectures would prove a failure. But if taken up by some educational authority and delivered at schools, &c., they would doubtless be of great benefit.

As regards the lectures themselves they are, as a matter of course, extremely elementary. Their simplicity of language and freedom from all those phantastic and unproved hypotheses, so common at the present day, are highly to be commended. The reader who wishes for further details will find in connection with each lecture a list of suitable works for private study.

Food Chart. By R. LOCKE JOHNSON, L.R.C.P., &c. London: Hardwicke and Bogue.

THIS publication is intended to give the names, classification, composition, alimentary value, rates of digestibility, adulterations, tests, &c., of the alimentary substances in general use—the whole drawn up on a broad sheet adapted for the wall of the study, the office, the board room, &c. Accurate information of this kind is very much wanted in official, as well as in private quarters, *e.g.*, by boards of guardians, visiting justices of prisons, and, in short, by all who have to decide on the scale of dietary of any large establishment, and this the more as such bodies are too apt to turn a deaf ear to the advice of their medical officers. The idea of this chart must, therefore, be pronounced happy.

The execution, however, might have been much improved if the author had taken a little more care. Thus we are told that "carbo-hydrates conduce to the production of fat. They consist of starch, cane sugar, grape sugar, lactin (milk sugar), inosite (muscle sugar), amyloid substances, gum, dextrin, cellulose, woody fibre, &c., &c." Why are the amyloid substances separated from starch? Whether cellulose and woody fibre are chemically distinct is, to say the least, very doubtful, and their power of becoming assimilated in the human economy and of "conducting to the production of fat" does not even admit of a doubt. They are evacuated unchanged. In the table of vegetable alimentary substances we find the onion omitted from the class of "roots and tubers," and banished to the "herbaceous substances." The "varieties of sugar," we learn, are "cane, grape, beet, maple, and starch sugars." This is exceedingly apt to mislead the ordinary reader; grape and starch sugar differ from cane sugar in their chemical composition and their properties, whilst beet-root sugar is identical with cane sugar, although obtained from a different source. Concerning rice we find the astonishing statement that the "ash should range from 80 to 85 per cent." Under the head "sausages, pork-pies, and preserved meats in tins or otherwise, Australian, South American, &c.," the adulterations met with are said to include saw-dust (!) and "productive portions of animal miscellanea"—whatever that may mean. Among the sophistications of wine, of which a tolerably full list is given, we find no mention of sulphate of lime.

Typographical errors are also numerous, and many of them are of a nature calculated to bewilder the uninitiated. We hope that in case of a second edition this chart will undergo a thorough revision.

The Royal Society.—At a meeting held yesterday the following gentlemen were elected Fellows of the Royal Society:—Capt. William de Wiveleslie Abney, R.E.; Prof. Henry Edward Armstrong, Ph.D.; Rev. William B. Clarke, M.A., F.G.S.; James Croll, F.R.S.E.; Edwin Dunkin, Sec.R.A.S.; Prof. John Eric Erichsen, F.R.C.S.; David Ferrier, M.A., M.D.; Col. Augustus H. Lane Fox; Prof. Alfred Henry Garrod, M.A.; Robert Baldwin Hayward, M.A.; Charles Meldrum, M.A., F.R.A.S.; Edward James Reed, C.B.; Prof. William Rutherford, M.D.; Robert Swinhoe, F.R.G.S.; Prof. Thomas Edward Thorpe, Ph.D.

CORRESPONDENCE.

THE CHEMICAL SOCIETY.

To the Editor of the Chemical News.

SIR,—Might I be permitted to make a few remarks on the two letters on this subject which appeared in your last. History repeats itself. It is a misfortune that at some period or other in the existence of every corporate society, whether it be scientific, social, or religious, a few young men—they are invariably young men—having managed to get themselves admitted Members of the Society, conceive that it is their duty to try and reform it. These six or eight young men form themselves into a “clique,” and with more zeal than discretion proceed to blackball everyone they do not know. As their personal knowledge of the qualifications of the candidates is in most cases somewhat limited, the result is more curious and instructive than agreeable, although not unentertaining to a student of human nature if he be not interested in the welfare of the Society. The Chemical Society is passing through such a crisis.

It will be evident that as by the bye-laws of the Society one negative ball neutralises three affirmative ones, Messrs. “Nuntius” and “Twig,” with the help of four or five friends, can easily blackball all the candidates if the meeting is thinly attended, as was the case on May 6th, or if only a small number of those present go up to vote. That the majority does not approve of this process of rejection, or even an important section of the Fellows, was clearly shown by the way in which the President’s remarks on the subject were received at the last meeting. These, so far from being merely tolerated “from the great respect entertained for the speaker,” as stated by Mr. Twig,* were received with what newspaper reporters term “unbounded applause.”

If a candidate is blackballed whom the President of the Society and other well known chemists—Professors in our Universities and Colleges—assert of their *personal knowledge* to be a fit and proper person to be elected a Fellow, it means one of two things—either that these gentlemen have knowingly proposed a notoriously unfit person, or that the blackballers are much better judges of the candidate’s qualifications (whose name in all probability they have never heard before) than the eminent chemists who have signed his certificate from *personal knowledge*.

The letter of “Nuntius” seems to be straightforward and to the point. The writer evidently thinks the Chemical Society is a close society, and that those who are not professional chemists should be rigidly excluded. The formation of such a chemical institute is much to be desired, but if “Nuntius” had taken the trouble to look over his “Charter and Bye-laws” before he wrote his letter, he would have seen that the Chemical Society was founded with a very different object: not as a kind of trades’ union, or for the purpose of conferring honorary degrees, but for “the promotion of chemistry and those branches of science immediately connected with it,” or as it is in the Charter, “For the general advancement of Chemical Science.” Might I, in all humility, suggest to the consideration of “Nuntius” and his friends that it is scarcely justifiable to make use of the power given to the minority by the vote by ballot for the purpose of virtually altering the constitution of a society.—I am, &c.,

CHARLES E. GROVES.

May, 27, 1876.

To the Editor of the Chemical News.

SIR,—Though not a Fellow of the Chemical Society, I am very pleased to learn from the correspondence in the CHEMICAL NEWS that there has been some extensive blackballing of candidates for admission to that honour. It is sad to see the manner in which titular letters are

* The name of Twig does not occur in the list of Fellows; if it is a *nom de plume* it is singularly infelicitous.

used in order to deceive the public, and all who have any respect for our “learned” (as they ought to be) societies would like to see extensive curtailing of admissions. If money is required would it not be possible to have Associates, and reserve the title “Fellow” to those who can show some other claim to it than the recommendation of a few friends?—I am, &c.,

C. J. W.

To the Editor of the Chemical News.

SIR,—Your correspondents of last week have told us with what object the systematic blackballing at the Chemical Society has been carried on. It is well that the reasons of this proceeding should be at last plainly stated. Will you kindly allow me space for a few remarks on the question?

The Chemical Society clearly stands on the same footing as the Linnean, Geological, Physical, and other scientific societies. The object of such societies may be well expressed in the words of the resolution adopted at the formation of the Chemical Society, namely, “the promotion of . . . Science . . . by the reading, discussion, and subsequent publication of original communications.” These societies do not seek to advance Science by exercising the functions of an examining body; their sphere of action is quite different, and may be briefly described as the stimulation of original research. To be a Fellow of one of these societies does not stamp a man as a good chemist, a good botanist, or a good geologist; it simply implies that the person in question is one who has a genuine interest in these sciences, and wishes to enjoy the facilities which the respective societies afford. The attempt to create any one of these societies into a tribunal to judge of the scientific attainments of candidates, and thus to give the character of a diploma to the admission into Fellowship, is therefore clearly founded on a mistake of the purpose for which such societies were founded.

It is quite true that when a science like chemistry acquires such practical importance that it is adopted as a profession, some means of discriminating the worthy from the unworthy professors becomes urgently needed. But if this is the case, let an appropriate examining machinery be at once sought for, after the model of the medical and other licensed professions. Such a movement would obtain the support of the majority of chemists, and would surely be far better than trying to force the Chemical Society into an unnatural position.

But if the object which your correspondents avow is a mistaken one, and foreign to the fundamental idea of a scientific society, I venture to think that their mode of attaining that object is still more mistaken. The gentlemen who systematically blackball at the Chemical Society are continually condemning persons of whom they know nothing; a proceeding which in other places would be considered most unjust and absurd. A short time since a gentleman well known to the older Fellows of the Society was a candidate for Fellowship. He was a man of original scientific thought, and had a large practical experience of chemistry. His certificate set forth that he was a Fellow of various scientific societies, and that his occupation was that of “merchant.” The certificate was signed from personal knowledge by a number of our best known chemists. The word “merchant” was, however, fatal to him, and he was blackballed. A few months after he was again put up by his friends, and this time described as “chemical manufacturer” (a statement which was perfectly true); he was then elected without a single negative vote!

This is a fair illustration of the folly of the present blackballing system. The gentlemen who exercise this function will not accept the positive testimony of a number of eminent men to whom the candidate is personally known; they at once reject the candidate, though they know nothing against him, because his business occupation is that of Merchant! Surely in cases where a candi-

date is not personally known to a voter, his fitness to be elected should be decided by the testimony of those to whom he is well known, and not by the nature of his daily occupation.

I may add, in conclusion, that the system of rejecting all candidates who do not declare themselves as chemists, can have no effect in checking the real abuse of the letters F.C.S., namely, the employment of them for the purpose of advertisement. It is the chemists who are not gentlemen, and not the gentlemen who are not chemists, who are most likely to sin in this respect. The only plan of dealing with this real, though happily limited evil, is by making some addition to the obligation clause signed by all Fellows, as already suggested by Dr. Williamson.—I am, &c.,

R. WARINGTON.

May 30, 1876.

To the Editor of the Chemical News.

SIR,—Having just read a letter in the CHEMICAL NEWS (vol. xxxiii., p. 220), headed "Blackballing at the Chemical Society," I beg to draw attention to the following words:—"In brief, it was intimated that the Fellows ought not to use their individual judgment, and that the 'blackballing' was indiscriminate." "If this latter be true it is a grievous fault, and calls for immediate rectification." I take the liberty of pointing out that that which the writer calls a "grievous fault" was committed on the evening of May 4, and therefore calls for "immediate rectification." One rejected candidate was most thoroughly well qualified in every respect for the Fellowship of the Society: after a very complete course of study in London and Heidelberg he is now engaged in research work. It cannot be urged that his qualifications were not plainly stated on his certificate, and no one who has the pleasure of his acquaintance can possibly imagine that personal dislike was the cause of his rejection. Duties elsewhere generally prevent my being present at the voting, and I am unaware what proceedings have usually taken place, but that some care should be exercised in the signing of certificates I think is desirable in order to prevent the humiliating spectacle of a learned society proceeding to elect or reject by ballot a distributor of worthless degrees or a promoter of bogus companies.—I am, &c.,

WALTER NOEL HARTLEY.

King's College, London, May 30.

To the Editor of the Chemical News.

SIR,—It was with unqualified satisfaction that I read the letters of "Twig" and "Nuntius" in your last issue. I hail them as evidence of a feeling, rapidly gathering strength, which is calculated, in my opinion, to give to the Chemical Society a much better position than that which it has held for some time.

I have often heard the Fellowship of the Society spoken of in words of open contempt, and recent movements among chemists show that my experience is by no means a solitary one. It is hardly two months since a chemist, well skilled in his profession, spoke about the Chemical Society as one which it was not worth joining. If we come to examine the grounds on which such an assertion could be made we discover the justification of that course which Professor Abel so strongly condemned on the 18th instant.

It cannot be denied that the Chemical Society numbers amongst its members the majority of the best chemists in England, and if we looked at this fact alone it would be difficult to say how its reputation had fallen so low. But when we consider that many of these chemists habitually slight the Society by absenting themselves from its meetings, and when we find that many of its Fellows pretend to no chemical qualification whatever, we see the cause of the present discontent among working chemists.

Now, as all the present Fellows have been proposed according to rule, it would appear that there must be

either some laxity on the part of those who propose, or else very different opinions as to who are fit and proper persons for election.

It is on this last point that Professor Abel differs so strongly from your correspondents. A man who is a chemist by profession is evidently eligible, but how much further you can go with profit to the Society is a question which receives very different answers from different persons.

I, like your correspondents of last week, accept only a chemical qualification, and in so doing sincerely believe that my action will tend to advance the chemistry of our country.—I am, &c.,

BEE.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Seances, de l'Academie des Sciences. No. 19, May 8, 1876.

On Osmium.—MM. Ste.-Claire Deville and H. Debray.—Osmium as obtained by the authors has a fine blue colour, shaded with grey. It forms small crystals, either cubic or rhombohedral, closely bordering upon the cubic form. It is harder than glass, which it scratches with ease. It is the heaviest body known, its specific gravity being 22.477. Crystalline osmium is obtained by passing the vapour of osmic acid, repeatedly rectified, over pure carbon. The sesquioxide of osmium is frequently deposited in the tube in crystalline scale of a fine coppery-red. It is permanent in the air, and consists of—

Osmium..	89.13
Oxygen	10.87
						100.00

Note on Fermentation, with reference to the Views of Drs. Brefeld and Traube.—M. L. Pasteur.—The author announces that the chemists just mentioned have withdrawn their objections to his views.

Electric Transmissions without Conducting Wires, with reference to the Recent Communications of MM. Bouchotte and Bourbouze.—M. Th. du Moncel.—The author explains that the idea of communication without wires is far from novel, having been experimentally tested thirty years ago, both in England and America. Thus, messages were sent from Gosport to Portsmouth (and, we believe, across to the Isle of Wight), a distance of about 3 kilometres.

Calcareous Alabaster from Mexico.—M. A. Damour.—This material is known in commerce as the onyx of Tecali. It varies in colour from milk-white, yellowish white to pale green, certain samples displaying brown veins shading into red. It takes a fine polish. Its specific gravity is 2.77. It is readily and entirely soluble in nitric acid. Its composition is—

Carbonic acid	43.52
Lime	50.10
Magnesia	1.40
Ferrous oxide	4.10
Manganous oxide	0.22
Water	0.60
Silica	traces

99.94

Extraction of Gallium from its Ores.—M. Lecoq de Boisbaudran.—The blende is dissolved in *aqua regia*, and pieces of sheet-zinc are placed in the liquid, and withdrawn when the escape of hydrogen has greatly subsided,

but is still perceptible. In this manner is separated the greater part of Cu, Pb, Cd, Ir, Tl, Ag, Hg, Se, As, &c. To the clear liquid zinc is added in large excess, and it is boiled for several hours, when an abundant precipitate is formed, containing alumina, sub-salts of zinc, and gallium. This precipitate is re-dissolved in hydrochloric acid, and the solution boiled again with zinc. All the gallium present is thus concentrated in a liquid of small bulk. The last gelatinous precipitate is dissolved in hydrochloric acid, acetate of ammonia is added, and the solution treated with sulphuretted hydrogen. This operation is repeated for the complete removal of the alumina. The hydrochloric solution of the white sulphides is precipitated fractionally with carbonate of soda, when the gallium is found concentrated in the first portions deposited. The spectroscope indicates the point at which it is necessary to stop. To complete the separation of the zinc, the oxide of gallium is dissolved in sulphuric acid, and then supersaturated with ammonia in excess. The gallium which remains in the ammoniacal solution may be expelled by boiling to expel free ammonia, destroying the ammoniacal salts with *aqua regia*, and fractional precipitation with carbonate of soda. The pure oxide of gallium precipitated by ammonia is dissolved in potassa and submitted to electrolysis, when gallium is deposited on the negative platinum electrode. The positive electrode, likewise of platinum, should be larger than the negative. Five or six Bunsen elements are sufficient to decompose 20 to 30 c.c. of the concentrated solution. On placing the negative electrode in cold water, and bending it, the gallium is easily detached. The author has sought for gallium in the following substances:—

A. *Rich Substances*.—Black blende from Beusberg (specimens sent by the Vieille Montagne Mining Co.); yellow transparent blende from Asturias; brown blende from Pierrefitte (Pyrenees).

B. *Rather Poor Substances*.—Powdered zinc from the Vieille Montagne; zinc dross from Corphalie(?)

C. *Very Poor Substances*.—Yellow opaque blende from Mandesse (Gard); brown blende from Sweden; black-brown blende from Schwarzenberg, in Silesia; blende in rods from Nouvelle Montagne. No gallium was found in the following,—Ribbon blende from Vieille Montagne; tutty from Corphalie; galenas from Pierrefitte and elsewhere; metallic zinc from Vieille Montagne, as used at Cognac for building purposes; calamines from Sardinia and Le Gard; commercial hydrochloric and nitric acids.

Influence of Carbonic Acid on the Respiration of Animals.—M. F. M. Raoult.—The author concludes that the presence of carbonic acid in the air inspired is an obstacle to hæmatosis.

Acetyl-persulpho-cyanic Acid.—M. P. de Clermont.—Not suitable for abstraction.

Interchange of Ammonia between the Atmosphere and Vegetable Soil.—M. Th. Schloësing.—The experiments undertaken show that the soil derives, in general, ammonia from the atmosphere.

Action of Zinc on Solutions of Cobalt.—M. Lecoq de Boisbaudran.—It is correctly supposed that the salts of cobalt are not precipitated by zinc, whether in the cold or at a boiling heat. Nevertheless, sometimes considerable quantities of zinc are found in the metallic sponge produced by the action of zinc upon the solution of blende in *aqua regia*. The following observations have been made as to the cause of this peculiarity:—The presence of a metal readily reduced by zinc is necessary. Copper and lead may carry down cobalt, especially the former. Cadmium gives only negative results. If the liquid containing copper and cobalt is very acid the copper alone is deposited. It is only in liquids bordering closely upon neutrality that the deposition of copper induces that of cobalt also, when the liquid is quickly decolourised. In a solution rendered basic by prolonged contact with zinc cobalt, far from being reduced, is re-dissolved if it has been previously separated. To decolourise the liquid

anew, it is sufficient to add a very small quantity of acid. The cobalt is reduced to the metallic state, and resists dilute acetic acid. Hydrochloric acid attacks the metallic sponge slightly, with evolution of hydrogen, but the action soon ceases. This reaction shows that the copper and cobalt are intimately mixed. A metallic sponge contained four-fifths of its original amount of cobalt after remaining for forty-eight hours in concentrated hydrochloric acid. The presence of a certain amount of a salt of copper is necessary.

Existence of Mercury in the Department of l'Herault.—M. N. Thomas.—Mercury has been found in certain places indicated. Nothing is said as to the quantity.

Action of Hydriodic Acid upon Quercite.—M. L. Prunier.—Not adapted for abstraction.

Analysis of the Native Magnetic Platinum of Nischne-Tagilsk (Ural).—M. Terreil.—A specimen of the platinum gave, on analysis,—

Platinum, with traces of iridium	81.02
Osmide of iridium and platinum metals	}	3.33
insoluble in <i>aqua regia</i>		
Silver	traces
Copper	3.14
Iron	8.18
Nickel	0.75
Chrome-iron	{ Cr ₂ O ₃ , 1.75 FeO, 1.01 Al ₂ O ₃ , 0.37 } 3.13
Silica	0.13
Alumina, magnesia, and iron in the state of silicates	traces
		99.68

Reimann's Farber Zeitung,
No. 16, 1876.

A blue shade of eosin is now in the market which gives upon wool a colour resembling cochineal ponceau. The dye is soluble in water, and has a strong affinity for wool, no mordant being required except hyposulphite of soda.

It is announced that Dr. F. Springmühl, editor of the rival tinctorial journal, the *Muster-Zeitung*, is at present in prison in Vienna. In connection with this affair Dr. Reimann speaks of the "horrible condition" of the technological press of Germany.

Gazzetta Chimica Italiana.
Anno vi., 1876, Fasc. iii.

Researches on Usnic Acid, and on Two New Principles which accompany it in Zeora Sordida.—E. Paterno.—The author describes the preparation of usnic acid, to which he assigns the formula C₁₈H₁₆O₇, in preference to C₁₈H₁₈O₇, as proposed by Hesse and Stenhouse. He further examines its behaviour with hydrochloric, nitric, and sulphuric acids; with bromine; with a mixture of sulphuric acid and bichromate of potash; with anhydrous phosphoric acid, zinc turnings, chloride of acetyl, and anhydrous acetic acid; with alcohol, with which, if heated in a closed tube, it yields decarbousnic acid, C₁₅H₃₆O₅. The two new accompanying principles are zeorin, C₁₃H₂₂O, and sordidin, C₁₆H₁₈O₇.

PATENTS.

ABRIDGMENTS OF PROVISIONAL AND COMPLETE SPECIFICATIONS.

Improvements in the treatment of gases, liquids, and pulverulent substances, and in the machinery or apparatus employed therein. J. H. Johnson, Lincoln's Inn Fields, Middlesex. (A communication from A. J. Huet and A. E. Geyler, Paris.) February 24, 1875.—No. 681. This invention relates to the application of centrifugal force for bringing into contact the molecules of various bodies (gaseous, fluid, or in a pulverised condition) in order to obtain in a continuous man-

ner and in a limited space considerable acting surfaces, and thus to facilitate by its acceleration the absorption or the separation of the elements composing a mixture of gases, fluids, or pulverulent substances.

Improvements in the production of soluble phosphates of lime. J. T. Way, Russell Road, Kensington, Middlesex. February 24, 1875.—No. 686. This Provisional Specification describes—(1) An improved method of producing chloro-phosphate of lime, a compound of the acid phosphate of lime with chloride of calcium. (2) Means of separating chloride of calcium from chloro-phosphate of lime, leaving the phosphate of lime in a pure or nearly pure state.

An improved prism for spectroscopic and other purposes. J. Beck, Cornhill, London. (A communication from J. W. Queen and Co., New York, U.S.A.) February 25, 1875.—No. 689. This invention consists in the construction of a prism either hollow and filled with bisulphide of carbon or other dispersive fluid, or solid of glass or other transparent substance in such a manner that the incident ray and the mean ray of the emergent pencil shall be in the same right line: that is, the action of the prism is direct, giving obvious advantages over the common form.

Improvements in the treatment of alunite, or of natural products containing the same, so as to obtain aluminous compounds therefrom. J. H. Johnson, Lincoln's Inn Field, Middlesex. (A communication from La Société Financière de Paris, Paris.) February 25, 1875.—No. 704. The essential points of this invention are—Effecting the production of alum from alunite by the employment of a solution of sulphuric acid or of alum, or of mixtures of the same, and effecting the separation of any iron the solution may contain by the addition of potash, or of alumina, or alunite.

Improvements in apparatus and processes for the treatment of auriferous and other ores and metallic compounds. D. G. Fitzgerald, Brixton, Surrey. February 26, 1875.—No. 713. The invention relates to the treatment of ores or oxidisable substances in a converting vessel containing nitric acid, or this acid in admixture with other mineral acid. By means of a tube communicating with the external atmosphere air is forced or drawn through the mixture of the oxidisable substance and acid. Heat may, if necessary, be applied to the mixture contained in the converting vessel, which latter is connected with a receiver in which any volatilised acid may be condensed. This receiver is by preference connected with the lower portion of the converting vessel in such a manner that the condensed acid may be allowed to flow back into the latter. The receiver is connected with three or more Woolfe's bottles or other equivalent vessel, respectively containing substances by which the vapours, such as nitrous acid, peroxide of nitrogen, which have escaped condensation in the receiver may be absorbed.

An improved method of and apparatus for simultaneously disinfecting, deodorising, disintegrating, and desiccating animal and vegetable matter for fertilising purposes. J. Cox, Great Grimsby, Lincoln. February 26, 1875.—No. 723. According to my invention the matters or substances to be treated are agitated by peculiarly constructed knives or stirrers borne on a shaft in a closed cylinder, into which deodorising agents, together with steam and hot air, are injected.

Improved means for discharging calcined ores from the furnace, or crushed ores from other sources, and for washing and separating such ores. J. Hall, Chacewater, Cornwall. February 27, 1875.—No. 731. The calcined ore falling from the furnace is first sifted, and the finer portions conducted through a tube by means of a screw conveyor into a wetting-chamber, where it is cooled by a stream of water. The mixture of ore and water next passes to a separator, the fine ore being conducted to the buddle, and the larger portions to be re-stamped, as are also the lumps rejected in the first sifting.

NOTES AND QUERIES.

** Our Notes and Queries column was opened for the purpose of giving and obtaining information likely to be of use to our readers generally. We cannot undertake to let this column be the means of transmitting merely private information, or such trade notices as should legitimately come in the advertising columns.

Urine, &c.—Can any reader state reliably the average number of ounces of urine and number of ounces of fæces voided daily per head of the population; also the percentage of nitrogen and of phosphorus or phosphoric acid in each? If so, he would much oblige.—ARTIFICIAL GUANO.

MEETINGS FOR THE WEEK.

MONDAY, 5th.—Royal Institution, 2. General Monthly Meeting.
TUESDAY, 6th.—Royal Institution, 3. "Wheatstone's Discoveries," by Prof. W. G. Adams.
— Zoological, 8.30.
WEDNESDAY, 7th.—Geological, 8.
— Microscopical, 8.
THURSDAY, 8th.—Royal Institution, 3. "On Voltaic Electricity," by Prof. Tyndall, D.C.L., LL.D., F.R.S.
FRIDAY, 9th.—Royal Institution. Weekly Meeting, 8. 9. "On the Parallel Roads of Glen Roy," by Prof. Tyndall.
— (Close of the Season).
— Astronomical, 8.
— Quekett Microscopical Club, 8.
— Anthropological, 8.
SATURDAY, 10th.—Royal Institution, 3. "On King Arthur's Place in English Literature," by Prof. Morley.
— Physical, 3.

TO CORRESPONDENTS.

D. C. L.—Your question respecting valonia is only suitable for our advertisement columns.

W. S. C.—Consult Richardson and Watt's "Chemical Technology." Papers on the subject have also appeared in this Journal.

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CONTENTS OF No. VI.—JUNE, 1876.

Effect of Chemical Treatments upon the Tensile Strength of Cotton, by Charles O'Neill, F.C.S.—Materials for a History of Textile Colouring (No. 2)—Researches upon the Relation of the different Colouring Matters of Madder to one another, and the part they take in Dyeing, by M. A. Rosenstiehl—Upon India-rubber Coated Bowls for Machine Printing—Upon the Method of Transferring Designs to Copper, by M. G. Witz—Steam Blue from Indigo—Critical and Historical Notes concerning the Production of Adrianople or Turkey Red, and the Theory of this Colour, by Theodore Chateau (continued)—Abridgments of Complete Specifications of Patents Recently Published—British and Foreign Patents, from the Commissioners of Patents Journal, April 21 to May 23, 1876, inclusive.

Manchester: PALMER & HOWE, London: SIMPKIN & Co.

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THE CHEMICAL NEWS.

VOL. XXXII. No. 863.

ON SOME ISOMERIDES OF ALIZARIN.*

By EDWARD SCHUNCK, Ph.D., F.R.S., and
HERMANN ROEMER, Ph.D.

CONSIDERING the importance of everything connected with the history of alizarin, we have been induced to undertake the study of such of the isomerides of that body as we have been able to obtain. These isomerides are interesting from a theoretical point of view, as presenting a problem with regard to internal constitution which has not yet been solved, and technically some of them are interesting as they occur along with artificial alizarin and not being available for tinctorial purposes are the source of loss to the manufacturer. The isomerides of alizarin hitherto observed are the following:—

1. *Purpuroxanthin*, a body first obtained by Schützenberger from commercial purpurin, and afterwards prepared artificially by the action of reducing agents on purpurin. It crystallises in yellow needles, soluble in alkalies with a blood-red colour.

2. *Isoalizarin*, a substance derived from madder and described by Rochleder, having properties very similar to those of purpuroxanthin, and perhaps identical with it.

3. *Frangulic Acid*, a substance also very similar to purpuroxanthin, obtained by Faust by the decomposition of frangulin, a constituent of the bark of *Rhamnus frangula*.

4. *Anthraflavic Acid or Anthraflavin*, a body accompanying artificial alizarin, first described by one of the authors in a paper read before this Society,† and subsequently examined by Mr. Perkin. Its isomerism with alizarin was established by Mr. Perkin, who was the first to obtain it in a state of perfect purity. It is easily distinguished by the colour of its alkaline solution, which is yellow.

5. *Anthraflavon*, a product obtained by the action of diluted sulphuric acid on oxybenzoic acid. We have not yet had an opportunity of preparing and examining this body, but on reading the description of it given by its discoverers, Barth and Sennhofer, it is evident that it bears a strong resemblance to the preceding.

6. *Quinazarin*, obtained by Baeyer, by the action of phthalic acid on hydroquinon. Of all the isomerides it most resembles alizarin itself. Its alkaline solutions have the same violet colour as those of alizarin, and it dyes mordants, while the other isomerides have no tinctorial properties.

7. *Chrysazin*, a body formed by the action of nitrous acid on hydrochrysammide, and carefully examined by its discoverer, Liebermann. By the action on it of strong nitric acid it yields chrysammic acid, the nitro acid first obtained from aloes by one of the authors many years ago. To these we have now to add—

8. *Isoanthraflavin*, a substance accompanying artificial alizarin, generally found along with anthraflavin in the commercial product, and which we shall describe presently.

Chrysophanic Acid, the crystalline colouring matter of rhubarb, which at one time occupied a place in the list, has been erased, since it has been shown by Liebermann that it is in reality a homologue of alizarin, having the formula $C_{15}H_{10}O_4$, and is derived, not from anthracen, but from a methylanthracen.

We propose in this paper to give an account of some experiments on two of these isomerides, viz., anthraflavic

acid and the one generally accompanying it, which we have lately observed for the first time.

Anthraflavic Acid or Anthraflavin.

We have little to add to the description of this substance given in the paper above referred to. Its melting-point is above $330^{\circ}C$. It is less soluble in glacial acetic acid than in alcohol. The analysis of a carefully purified specimen of the substance gave numbers agreeing exactly with the formula $C_{14}H_8O_4$, and confirming the results obtained by Perkin. The barium salt, which has been previously described, loses, when dried over sulphuric acid, a considerable quantity of water, becoming at the same time much lighter in colour. On being now heated at a temperature of 150° to $180^{\circ}C$. it loses two molecules of water, and the dried salt has a composition corresponding with the formula $C_{14}H_6BaO_4$. Our results do not quite agree with those of Perkin, who found the formula of the salt dried at 180° to be $2C_{14}H_6BaO_4.H_2O$.

Tetrabromanthraflavin, $C_{14}H_4Br_4O_4$, is prepared by adding bromine in excess to an alcoholic solution of the substance. It crystallises in yellow needles, which are almost insoluble in the usual menstrua, such as alcohol, benzol, and glacial acetic acid.

Nitroanthraflavic Acid, a body already referred to in the paper of 1871, is prepared by dissolving anthraflavin in fuming nitric acid, and after allowing to stand for some time, adding water, which precipitates the nitro acid as a light yellow crystalline powder. It is obtained on spontaneous evaporation of its alcoholic solution in large well-defined rhombic crystals of a deep yellow colour, the composition of which is expressed by the formula $C_{14}H_4(NO_2)_4O_4$. Most of the salts, such as the potassium, sodium, magnesium, barium, silver, and mercury salts, are soluble in boiling water, and crystallise in lustrous needles, varying in colour from light yellow to brownish red. By reduction with tin and hydrochloric acid the nitro acid yields a dark blue powder, which is almost insoluble in alcohol, glacial acetic acid, &c., but dissolves in caustic alkalies with a fine violet colour like that of alkaline solutions of alizarin.

Diacetylthraflavin has already been described by Perkin.* We found its melting-point to be at $227^{\circ}C$.

Diethylanthraflavin, $C_{14}H_6(CH_3)_2O_4$, was prepared by heating a mixture of anthraflavin, caustic soda, iodide of ethyl, and a little alcohol in sealed tubes to 120° , and crystallising the product from boiling alcohol. It crystallises in light yellow needles, which are soluble in benzol and glacial acetic acid, but insoluble in water. It fuses at 232° . The fused substance on cooling is converted into a mass of prismatic crystals. The spectrum of the solution in concentrated sulphuric acid, which is red, shows a well-defined absorption band between the green and blue.

Dimethylanthraflavin, the preparation of which is similar to that of the preceding, has almost the same properties as the ethyl compound. It fuses at 247° to 248° .

Isoanthraflavin.

This isomeride of alizarin was prepared from a by-product of the manufacture of alizarin supplied to us some time ago by Mr. Perkin, and which, according to the latter, had been obtained by treating the crude alizarin with lime-water, filtering and precipitating the red extract with acid. The product was treated with dilute caustic soda lye, in order to separate some anthraquinon. The filtrate gave with hydrochloric acid a yellow gelatinous precipitate, which was filtered off and treated with cold baryta water, until nothing more dissolved. The residue left undissolved after this treatment consisted of barium anthraflavate, and was employed for the preparation of anthraflavin. The blood-red solution was mixed with hydrochloric acid, which gave a yellow precipitate consisting of isoanthraflavin. This was purified by repeated

* A Paper read before the Manchester Literary and Philosophical Society.

† "Memoirs," 3rd series, vol. v. p. 227.

crystallisation from boiling alcohol, and was obtained in long yellow crystalline needles. Sometimes it yielded golden yellow lustrous scales, but these on re-crystallisation always gave needles. These needles, after drying over sulphuric acid, still contain 1 molecule of water of crystallisation, which is driven off by heating to 120°. The dried substance has a composition agreeing with the formula $C_{14}H_8O_4$, five analyses giving as a mean C 69.79, H 3.65, the calculated amounts being C 70.00, H 3.33. The properties of isoanthraflavin resemble those of anthraflavin. It melts at a temperature above 330°. When slowly heated between watch-glasses it yields a sublimate consisting of lustrous bright yellow needles and plates. It is a little more soluble in boiling water than anthraflavin. It dissolves easily in boiling alcohol and in hot concentrated sulphuric acid, but is almost insoluble in benzol and chloroform. It imparts no colour whatever to mordants and differs in this respect very widely from alizarin. It may be easily distinguished from anthraflavin by the colour of its alkaline solutions, which is distinctly red, while the colour of anthraflavin solutions is deep yellow, or when concentrated reddish yellow. In concentrated sulphuric acid isoanthraflavin dissolves with a cherry-red, anthraflavin with a yellow colour. The two substances may also be readily distinguished by their behaviour towards lime and baryta water, in which isoanthraflavin dissolves easily in the cold, yielding red solutions. Anthraflavin, on the other hand, is almost insoluble in cold baryta water, and only dissolves on boiling, while in lime water it is almost insoluble at all temperatures. Isoanthraflavin in most of its properties approaches purpuroxanthin even more closely than it does to anthraflavin; but having prepared a specimen of purpuroxanthin according to Schützenberger's process, we are enabled to assert positively that the two bodies are not identical. One of the characteristic properties of purpuroxanthin is that it yields phthalic by oxidation with nitric acid, whereas isoanthraflavin gives with nitric acid a nitro-substitution product.

The barium compound of isoanthraflavin can be made to crystallise (though not without some difficulty) in dark red needles resembling barium anthraflavate. It contains water of crystallisation, which it loses on being heated to 150°. The composition of the dry salt corresponds with the formula $C_{14}H_6BaO_4$.

Tetrabromisoanthraflavin, $C_{14}H_4Br_4O_4$, is prepared in the same way as tetrabromanthraflavin. It crystallises in yellow needles, soluble in boiling alcohol and in glacial acetic acid.

Diacetylisoanthraflavin, $C_{14}H_6(C_2H_3O)_2O_4$, was obtained by the action of acetic anhydride on isoanthraflavin at 160° to 180°. It crystallises in light yellow microscopic needles, which are soluble in alcohol and more easily soluble in glacial acetic acid. At 175° it commences to soften, and at about 190° it fuses completely. It is decomposed by alcoholic potash solution.

Diethylisoanthraflavin, $C_{14}H_6(C_2H_5)_2O_4$, was prepared in the same way as diethylantraflavin, which it closely resembles. It crystallises from alcohol in long light yellow shining needles, soluble in alcohol and ether, more soluble in glacial acetic acid and benzol. It fuses at 193° to 194°. It dissolves in concentrated sulphuric acid, forming a purple solution, the spectrum of which shows two ill-defined absorption bands, one in the yellow the other in the green.

Isoanthraflavin gives, with fuming nitric acid, a nitro-substitution product similar in its appearance and general properties to nitro-anthraflavic acid, but we have been unable from want of material to examine it minutely.

We will conclude with a few remarks on the action of caustic alkalies on anthraflavin and isoanthraflavin. On a former occasion it was stated by one of us that anthraflavin yields by the action of fusing hydrate of potash alizarin, and it was this supposed convertibility into alizarin which led Liebermann and others to the conclusion that anthraflavin was identical with monoxanthraquinon.

We are now in a position to assert with confidence that the product of the action of alkalies is not alizarin. On repeating the experiment on a larger scale by fusing the substance with caustic potash in a silver basin we obtained a substance which, after being freed from impurities crystallised from alcohol in orange-coloured needles strongly resembling, but certainly not identical with, alizarin. On heating it yields a sublimate in needles very similar to sublimed alizarin. The solution in alkalies is, however, devoid of the violet tint characteristic of alizarin, and on dilution appears distinctly red. The spectrum of the solution shows two absorption bands similar to those of alizarin solution, but these bands, according to the determination kindly undertaken for us by Dr. Schuster, lie further away from the red end than the bands of alizarin. It appears probable that this body may turn out to be an isomeride of purpurin, resembling Mr. Perkin's anthrapurpurin. Isoanthraflavin when treated in the same way yields a body which has most of the properties of anthrapurpurin, though it seems to crystallise more readily and in longer needles than the latter substance does, according to Mr. Perkin's account. We are at present engaged in the investigation of these products.

ON THE CLASSIFICATION OF THE CYANOGEN COMPOUNDS.

By SYDNEY LUPTON, M.A.

(Concluded from page 225).

ACCORDING to Schorlemmer and others, when a ferric salt is added to potassium ferrocyanide a deep blue precipitate, $Cy_{12}Fe_2(Fe)_2^{VI}K_2$, is formed, which is insoluble in salt solutions, but soluble in water. On dissolving this precipitate in water, and adding a ferrous salt, a blue precipitate, $Cy_{12}Fe_2(Fe_2)Fe$, falls, which is the chief constituent of Prussian blue. Turnbull's blue is supposed, when pure, to have the same composition; the ferricyanide oxidising the ferrous to ferric salt, and being itself simultaneously reduced to ferrocyanide.

But it is stated, on the other hand, that when common Prussian blue is treated with solution of potash it breaks up with formation of potassium ferrocyanide and *ferric oxide*. When Turnbull's blue is treated in the same manner it also yields potassium ferrocyanide, but *magnetic oxide of iron*.

Cyanides of Unknown Condensation, $(Cyn)^n$.
 Cyn , para-cyanogen.
 $Cyn(OH)_n$, cyamelide.

We have as yet no means of determining n in these cases. Jacobsen and Emmerling consider para-cyanogen to be (Cy_4) . It is possible that n in each of these cases is one, the difference from cyanogen and cyanic or cyanilic acid being caused by the different modes of combination of the self-saturated affinities of the nitrogen.

We should expect from our classification to find many analogies between the cyanides composing our first class and the chlorides, bromides, and iodides: thus we have—

CyH	CyCy	CyOH	CyCH ₃
ClH	CyCl	ClOH	ClCH ₃
BrH	CyBr	BrOH	BrCH ₃
IH	CyI	IOH (?)	ICH ₃

While we should expect analogies between the cyanides composing our second class and the oxides, sulphides, and selenides; thus we have—

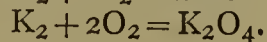
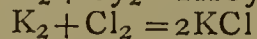
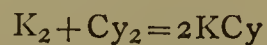
$Cy_2(OH)_2$	Cy_2S	Cy_2Cl_2 (?)	Cy_2KAg
$S(OH)_2$	SS	OCl_2	OKH
		SCl_2	SKH

We may also compare one or two other cyanides with well known oxides—Pelouze's cyanide of iron, Cy_8Fe_3 , with magnetic oxide of iron; ferric cyanide, Cy_6Fe_2 , with

ferric oxide; and cyanide of zinc and potassium, Cy_4ZnK_2 , with oxide of lead and silver, O_2PbAg_2 .

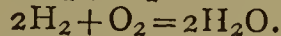
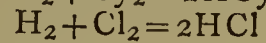
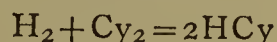
Several cases of similarity between cyanogen and chlorine may be brought forward besides those adduced by Mr. Skey.

Let us first consider the direct combination of each of the three bodies with potassium:—



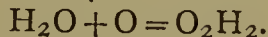
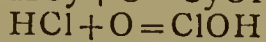
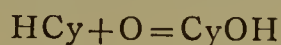
No one has yet been able to form K_2O by this reaction, but of course it is possible to form lower oxides than K_2O_4 at lower temperatures.

Next take the direct combination with hydrogen:—



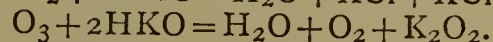
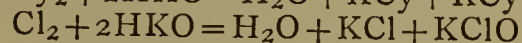
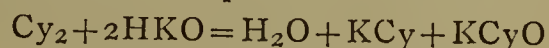
In the first two cases the combination takes place without condensation, in the third case there is a condensation of one-third.

Potassium cyanide may be directly oxidised by lead oxide and hydrogen cyanate thus procured, hydrogen chloride is directly oxidised by potassium permanganate, and water may be indirectly oxidised—



In the first two cases the oxidised body possesses acid properties; hydrogen dioxide is certainly not an acid.

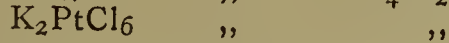
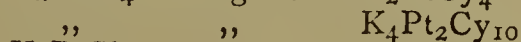
If the three bodies, ozone being used for oxygen, be passed into a solution of potash in water—



The first two reactions are evidently very similar, and differ entirely from the last one.

Let us now briefly examine the arguments which Mr. Skey brings forward in support of his views. Leaving out the acidity of prussic acid, the evidence for which is doubted by Mr. Skey, I pass to the alkalinity of potassium cyanide. This argument cuts both ways, for if KCl is neutral to litmus so is K_2O , and $HKCy_2$, the true analogue of HKO , has not yet been formed with certainty.

With regard to the cyanides and chlorides of the heavy metals, I would advance with due deference that there is a very strong similarity, though in many cases no corresponding salts are known in the other series. Thus, taking the potassio-platinum compounds, we have—



We have as yet no chlorides analogous to the platinum cyanides, but, leaving out of consideration the perhaps doubtful $BaPtCy_6$, we have $K_2PtCy_4Cl_2$, and all the other metals of the platinum group form cyanides exactly analogous to K_2PtCl_6 .

Further, I quite agree with Mr. Skey that cyanic is not analogous to chloric acid, but it is analogous to hypochlorous acid. Unfortunately, our knowledge of the hypochlorites is too limited to form a satisfactory basis for an argument. We have, in the present state of our experimental knowledge, certainly no right to formulate prussic acid as Cy_2H_2 : the vapour density found by Gay-Lussac was 0.947, which closely agrees with 0.9405, that required by the formula HCy .

The question of the relative solubility of the cyanides, oxides, and chlorides is too large a subject for me to enter upon at the conclusion of a long paper, but I might venture to suggest, in the first place, that such a reason is far too slight to justify us in making so important a change; and, secondly, that the evidence by no means lies entirely on the side so ably advocated by Mr. Skey. Silver cyanide,

for example, is insoluble in water but soluble in potassium cyanide, zinc oxide is insoluble in water but soluble in potassium hydrate. But, on the other hand, cuprous chloride is insoluble in water but soluble in potassium or ammonium chlorides; silver chloride is insoluble in water, but soluble to a slight extent in sodium chloride.

Lastly, Mr. Skey draws a parallel between chlorine and sulpho-cyanogen, which I fear the facts of the case hardly bear out; they agree I admit in one very important particular—each is a monad, but I think the resemblance goes no further.

In conclusion, I must apologise that want of space compels me to leave out graphic formulæ and references, and venture to express a hope that another paper from the pen of Mr. Skey may help to clear away the many difficulties which still stand in the way of a satisfactory classification of the cyanogen compounds.

NOTE ON WATER ANALYSIS.

By SIDNEY W. RICH.

IN the ammonia process of water analysis, when we deal with a tolerably good water, the error of observation due to the limited number of shades that may be discriminated by the eye is unnecessarily multiplied by the three or four separate comparisons of the distillates for albuminoid ammonia. Again, many waters will continue to yield ammonia until the contents of the retort have been boiled to nearly dryness; in point of fact, in such waters the organic matter is of such a kind that, although its ammonia may perhaps be eliminated by boiling with alkaline permanganate, the time allowed before the whole of the water shall have been boiled away is insufficient for such decomposition.

In the first instance mentioned above I find a very sharp reading may be obtained by concentration of the distillate, and in the second a slightly increased quantity of ammonia may be obtained by prolonged boiling, although, in nearly all cases, a definite cessation of the elimination of ammonia is at length reached. In either case I find that the object in view may be conveniently attained by returning a portion of the distillate to the contents of the retort and then re-distilling.

It has been my practice to carry the distillation out as follows:—As recommended by Wanklyn, I employ 500 c.c. of the water and distil first 50 c.c. and a further 150 c.c. for “free” ammonia; I then add the alkaline permanganate, and distil a first quantity of 50 c.c., and a further quantity of 150 c.c. The first 50 c.c. contain the larger proportion of the albuminoid ammonia, and may be at once Nesslerised; the second quantity of 150 c.c. is returned to the retort, and re-distilled until 50 c.c. have come over, which quantity is Nesslerised; the distillation is continued and the next 50 c.c. tested, but it will generally be found to contain less than one-hundredth of a milligram of ammonia. In very good waters which require to be compared it is useful to return the whole of the 200 c.c. of distillate to the retort after the addition of alkaline permanganate, and re-distil so as to get the whole of the albuminoid ammonia into the one quantity of 50 c.c. In very bad waters there is an advantage in the prolonged boiling, but the distillate will, in many cases, contain too much ammonia to allow of Nesslerising; it must accordingly be diluted with distilled water free from ammonia. This little modification, as I believe, of the general practice will also be found useful where circumstances render it necessary to perform the analysis on an unusually small quantity of water. In such cases I do not discriminate between “free” and “albuminoid” ammonia, but distil at once with alkaline permanganate, return the whole distillate to the retort, and re-distil 50 c.c. By this means 100 c.c. will quite suffice for an analysis; indeed it is a question whether the smaller quantity is not the better in skilful hands.

REPORT ON THE DEVELOPMENT OF THE CHEMICAL ARTS DURING THE LAST TEN YEARS.*

By Dr. A. W. HOFMANN.

Preservation of Ice. By Dr. H. MEIDINGER.

(Continued from p. 225.)

In breweries ice is still universally stored in walled pits, which are placed near the store cellars, and keep the latter cool. In Dreher's establishment the cellars occupy 113 cubic fathoms = 771.05 cubic metres, serving to store 3600 to 3800 eimer of beer = 2038 to 2151 hectolitres, and the adjoining ice pit contains 65 cubic fathoms (413.52 cubic metres) holding about 6510 cwts. (368.466 kilos.). The ice pits have the defect of being costly in construction and uncertain in their action. If ground water flows in over the floor, the ice melts rapidly. Where ice is stored up for sale in large quantities it is better to construct ice houses above ground, after the American plan, consisting essentially of double walls of wood with an interval of at least 0.3 filled with some bad conductor of heat, such as sawdust, chaff, loose peat, &c., in a dry state. Thus, the North German Ice Works at Berlin had, in 1871, an ice house 180 metres long, 24 wide, and 10 high, for the storage of 600,000 cwts. of ice. Such ice houses are cheaper than the subterranean ice pits, more convenient in use, and preserve the ice better, if only the layer of non-conducting matter is thick enough. The author† has treated this subject at length elsewhere. Every season new methods of preserving ice are announced in the papers. None of these are at all novel in principle; they depend upon causing a heap of ice to freeze together, if possible, and then covering it with a bad conductor of heat, such as straw, moss, &c. This is in the south of Germany but an unsatisfactory method; far in the north, especially in Russia, it may succeed. Such coverings, further, are very perishable. A cheap and effective ice house on a small scale can only be made in our latitude by preparing two cubic boxes, the inner measuring not less than 2 metres with an interval of at least $\frac{1}{2}$ metre between it and the exterior box in every direction. This interval is not left void, but filled with chaff, chopped straw, dry spent tanner's bark. The only entrance is a door of the same thickness as the sides. According to calculation in a well-made ice house of this construction the ice scarcely melts in a year at the distance of 15 centimetres from the walls. To divide the interval between the double sides into several compartments alternately filled with a bad conductor and left empty—as occasionally recommended—is decidedly irrational, being more costly and less effective than a single well-filled broad interval. The air, though the worst conductor of heat, yet if it can move freely in a given space, rapidly transfers heat from a warmer to a colder surface. It is sometimes recommended, and even attempted, to improve a bad ice house by throwing a quantity of salt upon the ice. The author has shown‡ that this is a very irrational procedure, since, although the cold may be increased to the senses, a considerable loss of ice ensues, the access of heat to the ice pit being accelerated by the augmented difference in temperature.

The preservation of ice is not only important on the large scale, but it is of consequence on the small scale in domestic operations. Food is to be kept cold and thus preserved from decay, or ice is to be used directly for cooling purposes. For this purpose closed boxes are employed under the name of ice cupboards or ice chests. The theory of these contrivances has been examined by the author.¶ The ice closets are cupboards with double

ides, the interval filled with chaff, &c., and the insides carefully lined with sheet zinc. The interval between the sides is often too small. According to the author's experiments, the entire breadth of the double side should not be less than 10 centimetres if the ice is to be preserved from rapid melting and a temperature of say 4° C. is to be maintained within. It is also preferable to place the ice in the entire upper third of the closet so that the lower two-thirds remain at liberty for food, &c. The entire cover is then made to open. The ice can then be easily taken out in pieces, and the whole lower space is equally cold, whilst if the ice is placed in a lateral compartment, as soon as it melts only the lowest part of the closet is thoroughly cooled.

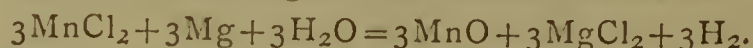
(To be continued)

ON THE ACTION OF MAGNESIUM ON SOME METALLIC SALTS.

By SERGIUS KERN, St. Petersburg.

SOME further experiments have been made referring to the action of magnesium on aqueous solutions of metallic salts (CHEMICAL NEWS, vol. xxxii., p. 309; vol. xxxiii., p. 112).

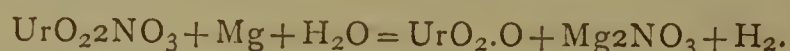
1. In a saturated solution of manganous chloride, after about six or eight hours, magnesium yields manganomanganic oxide. Hydrogen is freely evolved. The Mn_3O_4 is formed during this reaction as follows:—



But the manganous oxide is quickly oxidised by the water, and is transformed into Mn_3O_4 by the following reaction:—

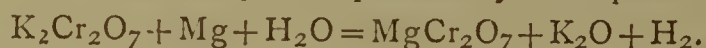


2. A salt of uranium was also decomposed by magnesium, viz., the nitrate. In some hours the metal was covered by a yellow insoluble powder, which is formed by the reaction—



Thus during this reaction uranic oxide is formed.

3. The solution of bichromate of potassium is very slowly decomposed by magnesium. The action of this metal in this case may be explained by the equation—



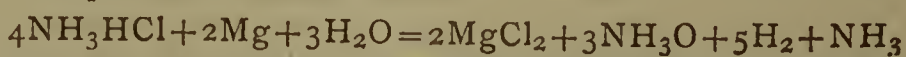
But as the potassium oxide cannot exist in free state during this reaction it is quickly transformed by the water into potassium hydroxide.

4. The action of magnesium on aluminium salts is very feeble. This metal being for many days in contact with aluminium chloride yields a small amount of aluminium hydrate ($Al_2O_3 \cdot H_2O$). Hydrogen is slowly evolved.

5. With palladium salts magnesium evolves hydrogen freely. Palladium monoxide is formed; it was also observed that palladium is also partly precipitated in the metallic state on the magnesium. The precipitated metal was found to contain hydrogen; thus it may be supposed that a part of the hydrogen resulting from the decomposition of water combines with the palladium to form the hydrogenated palladium of Debré (Pd_2H).

6. In copper salts magnesium is rapidly covered with copper; very rapid evolution of hydrogen is observed. This reaction could be used for the detection of small quantities of copper.

7. In a solution of ammonium chloride hydrogen is evolved very rapidly, the solution becomes even frothy. A piece of magnesium weighing one centigramme was entirely destroyed in twenty-five minutes. The solution quickly turns red litmus paper blue. The reaction may be represented as follows:—



But as the compound NH_3O (hydroxylamin) is a very

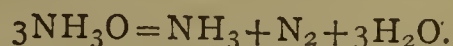
* "Berichte über die Entwicklung der Chemischen Industrie Während des Letzten Jahrzehends."

† *Baden. Gewerbezeitung.*, 1870, 71, iv., Nos. 5 and 6.

‡ *Baden. Gewerbezeitung.*, 1868, 74.

¶ *Baden. Gewerbezeitung.*, 1868, 66, and 1869, 11 and 16.

unstable compound it is quickly decomposed by the reaction:—



With a solution of ammonium nitrate magnesium also evolves hydrogen very freely. A piece of magnesium weighing 2 centigrammes was destroyed in two hours. The solution becomes very caustic owing to the presence of ammonia, which is formed exactly in the same manner as when magnesium acts on NH_3HCl ; the only difference is that instead of magnesium chloride magnesium nitrate is formed.

PROCEEDINGS OF SOCIETIES.

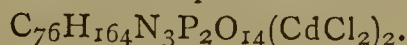
CHEMICAL SOCIETY.

Thursday, June 1st, 1876.

Professor ABEL, F.R.S., President, in the Chair.

THE minutes of the previous meeting having been confirmed, and the donations announced, the names of Messrs. T. H. Johnson, O. Hehner, G. C. Thomson, H. A. Bernays, W. J. Fuller, and G. Auerbach were read for the first time. Messrs. Robert Henry Harland, James Edmunds, M.D., Harold Holcroft, C. A. Buckmaster, Samuel Hall, Percy C. Gilchrist, and Walter Hibbert were duly elected, after their names had been read the third time.

The first paper, "*On Hemine, Hematine, and a Phosphorised Substance contained in Blood Corpuscles*," by Dr. J. L. W. THUDICHUM and Mr. C. T. KINGZETT, was read by the latter. The hemine employed was prepared by a modification of Wittich's process, and on examination was found to consist of hematine hydrochloride, mixed with free hematine and a phosphorised substance analogous to the brain myelins which gave a double salt with cadmic chloride of the composition of—



After recapitulating Paquelin and Jolly's researches on hematine—who state that it does not contain iron—the authors give the results of their experiments made with a view to verify this statement. They find, however, that the hematine obtained in this manner, instead of being free from iron, contained as much as 9.8 per cent of the metal, the largest amount ever before found in any preparation of hematine.

The PRESIDENT having thanked the authors for their important and interesting communication,—

Prof. W. N. HARTLEY read a paper "*On the Natural Carbon Dioxide from Various Sources*." After alluding to the examination of the liquids in the cavities of minerals made by Bryson, by Sorby and Butler, the experiments of Vogelsang and Geissler,—who proved that the cavities contained carbonic anhydride,—and his own paper on the subject read a short time ago before the Society, the author described the methods he had adopted for accurately determining the critical point of the liquid in these cavities, and also an ingenious apparatus for use on the stage of the microscope, to enable the operator to ascertain at once whether the liquid in a cavity was carbonic anhydride or not by subjecting it to a jet of heated air, a Nicol's prism being fixed under the stage to get rid of double refraction in quartz and similar crystals. The author gives a table of the critical point as determined in cavities in fifteen minerals, including topaz, sapphire, tourmaline, beryl, and rock crystal. The critical point is not the same in all, but varies slightly in several instances from that determined for pure carbonic anhydride by Andrews. These variations the author attributes to the presence of nitrogen or of hydrochloric acid, the former lowering and the latter raising the critical point. The

paper concluded with some suggestions as to the probable manner in which these liquid cavities were formed.

The CHAIRMAN said that the hearty manner in which the Fellows had recorded their thanks showed how much they appreciated Mr. Hartley's valuable paper.

Mr. KINGZETT then read "*A Note on some Trials of Frankland and Armstrong's Combustion Process in Vacuo*," by Dr. Thudichum and himself. In this paper the authors give the details of several combustions made by this method, employing, however, cupric oxide prepared from the nitrate instead of that made from metallic copper. The amount of carbonic anhydride obtained did not in all cases accord very closely with that required by theory; the nitrogen determinations, however, were very good. As it is generally necessary to make a combustion by the ordinary method for determining the hydrogen along with the carbon, the authors consider that the vacuum method—especially if potassium dichromate and sodium carbonate be used as a source of carbonic anhydride to sweep out the last traces of nitrogen—surpasses all others in accuracy and simplicity for the determination of nitrogen, or as a test of the presence of that element.

Mr. FRISWELL said he had made experiments in 1871 with this process in analysing the thallium compound of the formula $\text{Ti}_2\text{Pt}(\text{CN})_4\text{CO}_3\text{Ti}_2$, the results of which were published in the Society's *Journal*. The chief difficulties he had had to contend with were those of extracting the last traces of nitrogen from the tube, and of weighing the very small quantities of substance to be analysed with sufficient accuracy.

Mr. W. THORP said that if, instead of employing the cupric oxide made from the nitrate, the author had used that prepared from metallic copper, as recommended by Frankland and Armstrong, he believed they would have obtained better results: he had used the process for elementary analyses some two or three dozen times, and had not experienced any difficulty. The chief objection to its general use was that pointed out by Mr. Friswell, namely, the weighing such small quantities of substance.

Mr. KINGZETT replied that he had employed about 0.020 grm. of substance in his experiments, and had found no difficulty in weighing this quantity with sufficient accuracy.

Mr. T. FAIRLEY then gave a short abstract of three communications, the first being "*On Peroxides*." The author finds that acid solutions of hydrogen peroxide readily and quickly dissolve finely divided silver, whilst a mixture of the peroxide with hydrochloric acid also dissolves both gold and platinum. These results help to explain the action of finely divided metals on hydrogen peroxide in neutral solutions: the oxide of the metal is first formed, which, in contact with excess of hydrogen peroxide, undergoes double decomposition,—



After some observations on the heat of formation of the oxygen molecule and of water, the author passed on to the preparation of sodium peroxide, of which he exhibited a fine specimen. It is prepared by mixing a solution of sodium hydrate (10 to 20 per cent) with hydrogen peroxide, and precipitating by alcohol, taking care that the peroxide is not in excess. The crystals have the formula $\text{Na}_2\text{O}_2 \cdot \text{OH}_2$. On mixing solutions of pure hydrogen peroxide and uranium nitrate or acetate, a yellowish white precipitate, having the empirical formula $\text{UO}_4 \cdot 2\text{OH}_2$, is produced. Its rational formula, however, is three times this, $\text{U}_3\text{O}_{12} \cdot 6\text{OH}_2$, or $\text{UO}_6 \cdot 2\text{UO}_3 \cdot 6\text{OH}_2$, as is shown by the fact that when treated with an alkaline hydrate ordinary uranic hydrate is left, whilst a salt of peruranic acid enters into solution. The author has also succeeded in obtaining an anhydrous tetroxide, UO_4 , besides salts of peruranic acid. The ammonium compound,—



is an orange-yellow precipitate, obtained by adding alcohol to a mixture of uranic nitrate, hydrogen peroxide, and

ammonia. The sodium salt, $\text{UO}_6 \cdot 2\text{Na}_2\text{O} \cdot 8\text{OH}_2$, is precipitated in golden needles and plates on dissolving uranic hydrate or tetroxide in sodium hydrate, adding hydrogen peroxide and then a little alcohol. The potassium salt was prepared in a somewhat similar manner, but is less stable than the sodium and ammonium compounds.

The second paper was "*On Chromic and Perchromic Acids*." The author's attempts to prepare pure salts of perchromic acid were without success: by an indirect method, however,—namely, observing how much potassium dichromate was necessary to decompose a known quantity of hydrogen peroxide acidulated with sulphuric acid,—he obtained results which suggest the formula $\text{CrO}_6 \cdot 3\text{OH}_2$ for perchromic acid.

Mr. Fairley's third paper, "*On the Estimation of Nitrogen*," gives the details of a process which combines Cloëz's method of making the combination in an iron tube with Will and Varrentrap's soda-lime process. The substance is burnt with soda-lime in a wrought-iron tube in a current of coal-gas, purified by passing it first over heated soda-lime, and then over pumice saturated with sulphuric acid. The author has obtained most satisfactory results with the process.

After the CHAIRMAN had thanked the author,—

The SECRETARY read a paper by Prof. J. W. MALLET, "*On Aluminium Nitride and the Action of Aluminium on Sodium Carbonate at a High Temperature*." In order to ascertain if aluminium would take up carbon in the same way that iron does, a mixture of sodium carbonate with excess of metallic aluminium was very strongly heated. The desired result was not obtained, however; the product—which was a dark grey sintered mass mixed with unaltered aluminium—contained, besides carbon, small crystals of aluminium hard enough to scratch topaz. On the surface of the aluminium regulus were little crystalline patches of a yellow colour. This substance, of which the quantity was but small, was found on examination to be aluminium nitride, Al_2N_2 . Acids attack it rapidly if concentrated, slowly if dilute.

The last paper, by Prof. TUSON and Mr. E. NEISON, was "*On the Estimation of Mercury*." The method is founded on Hannay's process for the estimation of mercuric chloride, which has been extended by the authors so as to include all the salts of mercury. The addition of ammonia or an ammonium salt to an alkaline solution of the mercuric salt produces an opalescent white precipitate, which is re-dissolved on adding a standard solution of potassium cyanide, the end of the operation being very readily ascertained by the extinction of a peculiar blue opalescence that the solution previously possesses. The accuracy of the process is such that if the amount of mercury is large it can be determined to within 0.02 per cent, whilst even with a few milligrammes it can be estimated to 0.2 per cent. The paper contains an investigation of the influence of other salts on the sensitiveness of the process, and concludes with the analysis of a number of mercuric salts, including the nitrate, sulphate, acetate, citrate, oxalate, sebate, &c.

The CHAIRMAN, having thanked the author, adjourned the meeting until Thursday, June 15th, for which the following papers are announced:—1. "Chemical Studies," by Prof. Dewar. 2. "Researches on the Reduction of Nitric Acid, and on the Oxides of Nitrogen: Part I.—On the Gases Evolved by the Action of Metals on Nitric Acid," by Dr. H. E. Armstrong and Mr. Ackworth. 3. "The Composition and Formula of an Alkaloid from Jaborandi," by Mr. C. T. Kingzett. 4. "The Simultaneous Action of Iodine and Aluminium on Ether and Compound Ethers," by Dr. J. H. Gladstone and Mr. A. Tribe. 5. "On Compounds of Antimony Pentachloride with Alcohols and with Ethers," by Mr. W. C. Williams. 6. "On the Volatility of Barium, Strontium, and Calcium," by Prof. J. W. Mallet. And 7. "Note on the Action of Potassium Pyrogallate on Nitric Oxide," by Dr. W. J. Russell and Mr. Lapraich.

NOTICES OF BOOKS.

Notes on the Practical Chemistry of the Non-Metallic Elements and their Compounds. By WILLIAM PROCTER, M.D., F.C.S., Lecturer on Chemistry at St. Peter's School, York. London: Simpkin, Marshall, and Co.

ALTHOUGH the author, in his preface, states that he has found the want of a text-book on the non-metallic elements, we are at a loss to discover any special advantage possessed by the work under notice, as there appears to be little or no originality in its arrangement, and the information is merely that which is to be found in dozens of other text-books, if we except some rather startling statements which are in opposition to general experience or the accepted principles of chemistry. Thus, on page 12, we are told that "the combination of the two suffocating gases, ammonia and chlorine, produce (*sic*) the white, solid, inodorous, sal ammoniac," and the same statement is substantially repeated on page 14. On the next page it is stated that "if lead sulphide be heated, the combination between the lead and sulphur is broken up, the latter being volatilised and the former left." On page 70, a solution of cupric sulphate in ammonia is stated to dissolve carbonic oxide, and further on the formation and decomposition of phosgene gas is said to be valuable as "a test for CO united with other gases. The paragraph referring to phosgene is one of a section on *olefiant gas*, which gas is stated to be absorbed by cuprous chloride. It is but fair to say that the book contains a good deal of information in a small space. The formulæ are those in favour at South Kensington. At the commencement there is a sheet of very roughly executed woodcuts of apparatus.

CORRESPONDENCE.

THE CHEMICAL SOCIETY.

To the Editor of the Chemical News.

SIR,—The varied, but on the whole favourable, criticism elicited by the letters of "Twig" and myself upon this subject calls, on some points, for explanation and reply.

Both your first correspondent and Mr. Warrington, who are our only adverse critics, agree in their charge against us. We are altering the aims of the Chemical Society, say they.

Now, looking at its charter, I find the object to be "the general advancement of Chemical Science." I fail to see how, when refusing unqualified candidates for admission, we at all depart from this intention.

Deny it as one may—and this is the real grievance—it is clear to all that the Society does not command respect, and I think that when the body supposed to represent Chemistry is allowed to become a bye-word and a laughing-stock it is high time to enquire whether such is the best means of promoting Chemical Science. I differ from Mr. Warrington in believing that the Chemical Society was intended to be an association of chemists, and chemists alone, not a body of miscellaneous *dilettanti*—else why ballot for Fellows at all? Were it really a body of chemists it would surely forward the interests of our Society with much more ability and effect than at present, and so subserve better the purpose for which it was founded.

If it is a money matter, and these outsiders wish to subscribe to the funds, or to have the advantages offered by the Society, why not do something like that proposed by C. J. W.?—say, let them be admitted associates, receive the *Journal*, and pay double. We would not then require even to ballot for them.

I hope that in future properly qualified candidates will take care to state fully and clearly their claims to Fellow-

ship; there will then be no doubt about the result of their application.—I am, &c.,

NUNTIUS.

To the Editor of the Chemical News.

SIR,—Being one of the first of your contributors on the subject of the blackballing at the Chemical Society may I be permitted to make some additional observations.

Your correspondents "Nuntius," "Bee," "C. J. W.," and, in the main, Mr. Hartley, accord in opinion with myself. Messrs. Groves and Warrington fairly represent the party of which it is sincerely to be hoped they are the last remaining shreds, and repeat the old, old story which has unfortunately so often lulled the Chemical Society when it should have been fighting the battle for which it was called into existence.

The point at issue is a very simple one. Is the Chemical Society to be an association of chemists or an assemblage of indescribables. We have been reminded that the Society exists for the advancement of chemistry. Surely this is the legitimate work of the truest chemists, and not likely to profit much in the hands of the merchant, or even the manufacturer of umbrellas? With its charter and start of thirty years the Society ought to be the one grand representative in 1876 of the chemists and chemistry of England. The Society of Analysts and the Chemical Section of the Society of Arts are, however, sad evidences of its sloth and lack of power. We want unity and vitality, and I know of no reason why all who are really engaged in chemistry, whether technological or scientific, should not acknowledge one chief, and work manfully for one common end.

As long as the Chemical Society is supposed to represent chemists—badly as it may fill this function—I shall do all that lies in my power to cause the rejection of candidates other than chemists for its Fellowship, and in so doing shall feel that I am discharging a duty which falls to the lot of all who desire the reform of the Society rather than either its annihilation or its reduction to the most ignoble of positions.—I am, &c.,

TWIG.

June, 1876.

To the Editor of the Chemical News.

SIR,—Among the five letters on this subject last week there was one containing so many misrepresentations that it calls for explanation and rebuke.

Your first correspondent, himself a younger brother, made the altogether gratuitous assumption that the recent blackballing at the Chemical Society was due to a "clique" of five or six young men. Now, those acquainted with the matter will know that this is no sudden outburst, but that for more than twelve months past a feeling of distrust has gradually been becoming more and more evident, by the increasing number of candidates refused. These chimerical "five or six young men" must verily have been winning the support of more mature heads.

Again: we are to accept the signatures of the President and senior Professors. But if they do *not* require "a strictly chemical qualification," how can we, who do, trust their signatures who don't? Certainly Prof. Abel admitted that it was thought by the Council that *dilettanti* (his very word) chemists, and even persons taking only a general interest in chemistry, ought to be elected. How, then, can your correspondent think that *their* model of candidate is to be thought suitable according to *our* standard?

Allow me to direct attention again to the excellent letter of Nuntius, who, so far from requiring a close professional society only, asks for evidence of chemical attainments, and chemical attainments only. As "Twig" also observes, one who offers any non-chemical recommendation—even be it "Merchant,"* under the heading

* Vide letter of Mr. Warrington.

"Qualification"—on his certificate, certainly insults the Society, and ought to be rejected even though a thorough chemist; we may ask, Why did n't he say so? How are we to know it?

In conclusion I hope, Sir, that the Chemical Society will soon again see the day when it shall be respectable and respected.—I am, &c.,

VINDEX.

Liverpool, June 6, 1876.

To the Editor of the Chemical News.

SIR,—I am looking at the discussion going forward in your columns, respecting the exercise of the Ballot at the Chemical Society, with great interest. Whether the six or eight young men spoken of by Mr. Chas. E. Groves are exercising their privilege with "more zeal than discretion" I do not know; but it is certain that had a little more zeal or discretion been used for some years past, the honour—if it is one—which the Chemical Society bestows would not now be held in the contempt it is.

Mr. Warrington says, "These societies do not seek to advance Science by exercising the function of an examining body." I contend that practically they do, and that any person carrying the F.C.S. has a right to claim to be a chemist, and that the Chemical Society has endorsed and stood sponsor for his claim. It is useless to deny that the world looks upon these titles which societies grant as a species of degree, and that their possession is a guarantee of the holder's proficiency in some branch of Science,—in the case of the Chemical Society, of chemistry,—and not simply a "genuine interest in these sciences." In these days a sufficient number of small local societies exist into which these genuine interest men can gain admission, and which possess sufficient scope for their small scientific maundering, and in which they can disport themselves at will. My own case will probably be that of many chemists in this country. I have been in the actual practice of chemistry, both laboratory work and manufacturing, for the last twenty-five years; have published at least a dozen papers on analytical and kindred chemical subjects; possess no degree of any kind; would have offered myself for admission to the Chemical Society years ago, but was deterred by the fact that their title indicated nothing as to attainments, but had been distributed right and left with so lavish a hand as to become a bye-word; that the learned knew how valueless it was; and that, in many cases, it served only to gratify the vanity of would-be scientific men, with which they tickled the groundlings, or, if a deeper deep could exist, was bought for advertising purposes. I hope this agitation will be continued until the F.C.S. shall mean something of definite value,—something worthy of honourable ambition,—alike worthy to be held and worthy of the society which grants it. It will probably be long, if ever, before it becomes so valuable as the title the Royal Society can confer; but it may be made one which can be worn without shamefacedness, and having to be apologised for because nothing better can be obtained.—I am, &c.,

P. H.

Manchester, June 5, 1876.

To the Editor of the Chemical News.

SIR,—Not having yet seen the last number of your journal I may perhaps repeat ideas already broached, in venturing a few remarks on election to the Chemical Society. If so, I trust I may be excused.

It must be an object with all of us to keep up the *status* of the Society. At the same time we must remember that funds are required for its work, and especially for the publication of its most valuable *Journal*, and that to this end a large number of subscribers are necessary.

The first of these objects might perhaps be attained, and the research work of the Society increased, if it were

made a necessary condition of election that the candidate should first contribute a satisfactory paper to the Society. This would not seem an unreasonable requirement in a Society of which the first object is the encouragement of original research; and while such a restriction might at first diminish the number of candidates, it would much augment the value of the Fellowship.

It might also help the funds of the Society, and gracefully recognise those who desired to advance its objects, if the title of Associate could be given to those who paid perhaps a rather lower subscription and did not undertake original research. These might fairly receive the *Journal*, and have the right of personal admission to all the meetings of the Society, without that of introducing others.—I am, &c.,

A FELLOW.

ORGANISATION AMONGST CHEMISTS.

To the Editor of the Chemical News.

SIR,—Many of your readers will be glad to know that the proposal to establish an Institute of Professional Chemists is assuming a definite form, a scheme for the establishment of the Institute having been already prepared in readiness to be submitted to the Organisation Committee already appointed if the Council of the Chemical Society come to a decision upon the questions now before them affecting that Institute adverse thereto.

Some gentlemen, well known chemists, for whom I am professionally concerned, and who take a great interest in the establishment of the Institute, desiring that the proposed scheme should be brought under the notice of all members of the profession before the Institute is actually formed, have instructed me to address to you this communication as the only means of reaching all persons interested in this important subject.

After consideration my clients advise that the following scheme, with such amendments or additions as may be considered necessary, should be adopted, viz.:—

1. The Institute should be called the Institute of Professional Chemists.

2. Its objects should be—

(1.) For the general advancement of technical chemistry in its application to the arts, manufacture, agriculture, and public health.

(2.) To ensure that persons adopting the profession of consulting chemists or analytical chemists for reward are qualified by study and training for the proper and competent discharge of the duties they undertake.

3. Persons eligible for membership to give evidence of the possession of one or more of the undermentioned qualifications, viz.:—

As to persons now employed as chemists—

He should (if a British subject and permanently resident in Great Britain or Ireland) be a Fellow of the Chemical Society, and he should have practised on his own account in the profession of a consulting or analytical chemist, or have been employed at some recognised University, College, or Medical School as a professor or demonstrator of practical chemistry for at least five years.

As to persons not now employed as chemists—

(1.) He must be more than 25 years of age, must have passed through a satisfactory course of at least three years daily study of theoretical and analytical chemistry and physics, and have had subsequent employment for at least three years in some or one of the following manners, viz.:—In responsible situations as Assistant to a Member or Members of the Institute, or as Chemist in a Chemical Manufactory, or he must have been employed to the satisfaction of the

Council for a period of at least three years in the active prosecution of original chemical research.

Or,

(2.) He must have passed such an examination or examinations in theoretical and analytical chemistry and physics as the Council may direct, such examinations to be conducted by examiners to be appointed by the Council, and he must produce such other evidence as the Council may require of qualifications and general fitness for the work and duties of a professional chemist.

All persons proposing to become Members (except persons complying with the provisions of sub-section 2, who are to be entitled to membership without election) to be elected by ballot after due notice given to every Member of proposal and candidate's qualifications for membership. All Members to be at liberty to describe themselves in certificates of analysis, &c., as "Members of the Institute of Professional Chemists," or to adopt such abbreviated form as may hereafter be decided upon.

4. The officers of the Institute to be a President, two Vice-Presidents (of whom one shall be the retiring President in those years when the President is not re-elected), a Treasurer, and two Secretaries. All these to be ex officio Members of the Council. All officers to be elected annually, but to be eligible for re-election, except as to the President, who shall not be eligible for election for more than two successive years. The Council to consist of thirty-six persons (including the six above-mentioned ex officio members), five Members of the Council to be nominated by the Council of the Chemical Society. The other Members of Council to be elected by Members of the Institute. One-third of the Council to retire each year, but to be eligible for re-election. Three Members may recommend any person for election to the Council, and a list of 50 names at least of persons eligible for election to be prepared by Council and forwarded to each Member one month before time fixed for election.

5. Power to be reserved of expelling Members in cases where ten or more Members in writing signed by them and delivered to the Secretary shall require such expulsion, and the Council, having enquired into the matter and sought explanations from the person charged, find good reason for the proposed expulsion, the Secretary to send a letter to the person proposed to be expelled requesting him to withdraw from Institute. If this advice is followed no entry beyond the fact of the Member in question having resigned his membership to be made on minutes nor any public discussion allowed; but if advice is not followed nor satisfactory explanation given, Council to summon special general meeting for the purpose of voting by ballot upon the question of proposed expulsion.

6. Quarterly or more frequent meetings of the Institute at discretion of Council to be held when papers to be read, discussions, &c., to be allowed.

7. An entrance fee of £5 5s. to be paid by persons joining during the first three months of the Society's existence. Afterwards the admission fee to be £10 10s., with an annual subscription in both cases of £2 2s. Where examination requisite a special fee of £10 10s. to be paid by each candidate. The whole income and property of the Institute to be used in promoting the interests thereof.

The above is a short statement of some of the provisions of the scheme for the organisation of an Institute which, if earnestly supported and judiciously managed, will advance alike the interests of its Members as well as the science to which they devote their talents and life. It is thought that when the name of the Institute and the high qualifications demanded from all persons seeking its membership are known the Institute will carry with it a reputation which will reflect honour upon its Members and will constitute membership a prize to be coveted and a necessity for professional success.

There are several plans of organising a society of this character, and the plan to be recommended to their *confreres* has been carefully considered by my clients who are amongst the most active of the many promoters of this enterprise. The several methods of organising the Institute may thus be briefly stated.

1. By special Act of Parliament or Charter.
2. By Deed of Settlement.
3. By Incorporation under Section 23 of the Companies' Act, 1867.
4. By the publication of Rules and Regulations to be signed by every Member upon his admission as a Member.

Under one of the methods stated in the first and third cases alone can the Institute become a Corporation, but as the Government would not at the present time grant a Charter or support an application for a special Act of Parliament for the Incorporation of the Institute, the only course open to the promoters is to register the Institute as a Joint Stock Company in accordance with the Companies' Act, 1867, or to form a Society with or without a Deed of Settlement, which would be in its nature a purely voluntary Society without legal existence and having none of the incidents of a corporate body. After mature reflection my clients think it better that the powers given to them by the Companies' Act should be employed.

For the information of such of your readers as may not be acquainted with the provision of the section of the Act to which reference is made, I may remark that thereby, where any association is formed as a limited company, if it proves to the Board of Trade that it is formed for the purpose of promoting art, science, commerce, religion, charity, or any other useful object, and that it is the intention to apply the profits or other income of the association in promoting its objects and to prohibit the payment of any dividend to the members of the association, the Board of Trade may by license direct such association to be registered with limited liability without the addition of the word limited to its name, and upon registration the association enjoys all the privileges and is subject to the obligations imposed upon limited companies, but it is not required to use the word limited as any part of its name, or to publish its name, or to send a list of its members, directors, or managers to the Registrar of Joint Stock Companies.

Several advantages would accrue from the Institute being incorporated under this statute, but it is obvious to my clients that objections may be raised thereto, and their desire is that all persons interested with themselves in this subject should have an opportunity of considering this matter before they are called upon in public meeting to decide upon the form in which the Institute is to be organised.—I am, &c.,

J. PETTENGILL.

32, Walbrook, London, June 7, 1876.

NEUTRAL PHOSPHATE OF SODA UNSTABLE IN SOLUTION.

To the Editor of the Chemical News.

SIR,—During the evaporation of phosphate of soda I was surprised to observe that the roof and rafters immediately above the evaporating pans became slowly coated with a fine white dust, which, after a few months accumulated to a considerable extent. This dust was sampled and analysed, and found to be neutral phosphate of soda which had slowly volatilised with the steam during the concentration of the liquor. This salt was understood to be stable when in solution, and it is a curious circumstance that it should behave in same manner as boracic acid, though in a smaller degree when the latter is evaporated in solution.—I am, &c.,

J. B. READMAN.

175, Buchanan Street, Glasgow,
May 31, 1876.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Seances, de l'Academie des Sciences. No. 20, May 15, 1876.

Theoretical and Experimental Determinations of the Relation of Two Specific Heats in Perfect Gases whose Molecules are Monatomic.—M. Y. von Villard.—Not adapted for abstraction.

Determination of the Temperature of Solidification of Liquids, and, in particular, of Sulphur.—M. D. Gernez.—The author remarks that the slowness with which a body melts in a bath of a constant temperature little above its point of fusion, and the imperfect conductivity of the substances, which enables certain portions of the liquid to reach temperatures higher than those of the part not yet melted, have induced experimentalists, instead of the point of fusion, to determine the point of solidification, *supposed* to be identical, in which case, however, the results obtained may be often falsified in consequence of phenomena of surfusion. Sulphur insoluble in the bisulphide of carbon, as obtained by exhausting the flowers of sulphur, solidifies at 114.3°, whatever the temperature at which it has been melted. In octohedric sulphur the point of solidification is highest when it has been melted at the lowest possible temperature. Prismatic sulphur behaves like insoluble sulphur if obtained from it, but if subjected to repeated fusions and solidifications its congealing-point may be raised more than a degree.

Calorific Spectra.—M. Aymonnet.—Incapable of useful abridgment.

Presence of Selenium in Silver.—M. H. Debray.—Silver even containing 99.8 to 99.9 per cent of the pure metal is sometimes found not adapted for the preparation of industrial alloys—a circumstance due to the presence of traces of selenium. Refiners should therefore be careful to employ only such sulphuric acid as is free from selenium. The impurity may be eliminated by melting the silver as precipitated by copper along with nitrate of potash or soda.

Chemical Researches on Vegetation; Functions of Leaves; Origin of the Carbon.—M. B. Corenwinder.—The leaves of plants do not merely acquire carbon by their surfaces, but they also assimilate carbon from the carbonic acid which circulates in their tissues.

Crystalline System of Various Bodies presenting Optical Anomalies; Theory of Crystalline Groups; Explanation of Dimorphism.—M. E. Mallard.—Reserved for insertion in full.

New Mineral from the Pyrenees.—M. E. Bertrand.—The new mineral, for which the author proposes the name of Friedelite, occurs in the manganese mines of Adervielle, in the valley of Louron, and is a hydrated silicate of the protoxide of manganese. Its composition is—

Silica	36.12
Protoxide of manganese	53.05
Magnesia and lime	2.96
Water	7.87

100.00

Antiseptic Properties of Borax.—M. Bedoin.—The author maintains that borax will be most precious in the treatment of virulent bacterial affections, and for disinfecting unhealthy localities.

Moniteur Scientifique, du Dr. Quesneville,
April, 1876.

Recent Progress in the Industry of Aniline-Black.—A. Guyard.—This paper has been already noticed under

the *Bull. de la Soc. Chim. de Paris*. There is here, however, an appendix on the claims of the late Mr. John Lightfoot which was not found in the *Bulletin*, and which may be found interesting:—"Dr. Quesneville has called my attention to the question of priority raised in the CHEMICAL NEWS on the subject of aniline-black with vanadium, on which I think I ought to say a few words. Mr. James Higgin, of Manchester, writes to the CHEMICAL NEWS that, without discussing the question as to whether M. Pinkney has or has not discovered vanadium aniline-black, he must recall the fact that John Lightfoot, the inventor of aniline-black, has pointed out this reaction, and insisted that vanadium is the metal most proper to develop aniline-blacks. All this is perfectly correct, and is found distinctly expressed in an article by John Lightfoot, published in the *Moniteur Scientifique*, 362 livraison, February, 1872, p. 169. I will add merely that till lately we had no clear notions concerning aniline-black, and were ignorant of the part played by metals in its formation. Thus Lightfoot first, and then Pinkney, recommend the use of uranium, whose salts only produce aniline-black when they contain traces of vanadium, as I have pointed out, or when they are in the lowest stage of oxidation. On the other hand, Lightfoot states that cerium, molybdenum, and tungsten do not produce black. But cerium contributes to the formation of excellent blacks, and I have found that the salts of molybdenum and tungsten at the lowest stage of oxidation are intermediate between vanadium and copper, and produce splendid blacks."

Electrolytic Aniline-Black.—M. F. Goppelsröder.—Already noticed.

May, 1876.

Researches on Viscous Fermentation.—M. A. Commaille.—Not suitable for abstraction.

Use of Sulphide of Sodium in Tanning.—Wilhelm Eitner.—Sulphide of sodium has been employed with great success in removing the hair from hides.

Historical and Chemical Study on the Manufacture of Turkey-Reds.—M. T. Chateau.—A continuation of the treatise already noticed.

Gazzetta Chimica Italiana.

Anno vi., 1876, Fasc. iii.

Two Benzol-Bisulphuric Acids, and their Relation with other Compounds.—W. Körner and G. Monselise.—The author names these two acids respectively α -benzol-bisulphuric acid, corresponding to isophthalic acid, and β -benzol-bisulphuric acid, corresponding to terephthalic acid. He describes the potash, the baryta, lead, copper, cadmium, and soda salts of the former; also α -bicyanobenzin, and the chloride and amide of α -benzol-bisulphuric acid. Under the second he mentions the corresponding compounds.

Constitution of Veratric Acid and Veratrol.—G. Körner.—A paper devoted chiefly to hypotheses.

Santonin Ether.—F. Sestini.—The author ascribes to this compound the formula $C_{15}H_{19}(C_2H_5O)_4$.

An Addition-Product of Chloride of Acetyl and of Acetaldehyd.—R. Schiff.—This paper consists of complicated formulæ and of references to the views of Geuther.

A Chemico-Toxicological Study on Atropin.—F. Selmi.—An exhaustive examination of the reactions of atropin.

them." Dr. Roscoe pointed out that a mind like Dalton's obtained important results with rude and unimportant apparatus. His first rain gauge was a wine bottle with a tin funnel 7 inches in diameter. His mercurial trough was an old halfpenny earthenware cup. His weights were made of sheet lead, and on being tested before being sent to the Exhibition were found to be fairly accurate. The law of chemical combination in multiple proportion, the experiments on the diffusion of gases, the luminosity of flame, and other important researches of Dalton were clearly explained by Dr. Roscoe. Prof. Guthrie, F.R.S., will lecture to-morrow evening at 8 o'clock on "Cold," and on Monday evening the Rev. S. J. Perry, F.R.S., will lecture on "Methods Employed and Results Obtained by the late Transit of Venus Expedition." The following gentlemen have also kindly undertaken to give lectures:—Prof. F. A. Abel, F.R.S.; Capt. Abney, R.E., F.R.S.; Dr. Warren De la Rue, F.R.S.; Prof. G. C. Foster, F.R.S.; Dr. J. H. Gladstone, F.R.S.; Mr. J. Baillie Hamilton; Mr. J. Norman Lockyer, F.R.S.; Rev. R. Main, M.A., F.R.S.; the Right Hon. Lyon Playfair, C.B., M.P., F.R.S.; Mr. W. H. Preece; The Earl of Rosse, F.R.S.; The Lord Rayleigh, F.R.S.; Dr. W. J. Russell, F.R.S.; Mr. W. C. Roberts, F.R.S.; Dr. W. H. Stone; Mr. W. Spottiswoode, M.A., F.R.S.; and Mr. C. V. Walker, F.R.S. Demonstrations will also be given on Saturday, Monday, and Tuesday, next as follows:—11 a.m. Marine engines in motion, Room D. 11.30, Fog-horns, Room E. Electric light, Room F. Spectrum of electric light, Room F. 12.45, Time-gun apparatus, Room K. 1.30, Radiometers, Room Q. 2.0, Pictet's ice making machine, Room F. 2.30, Orreries, Room L. 3 p.m. (Monday only), Sir J. Whitworth's millionth measuring machine and true planes, Room H. 3.30, Electric light, Room F. Musical instruments (Monday only), Room Q. Ancient musical instruments (Tuesday only), Room Q. 4.40, the *Times* composing machines, Room F.

Bunge's Chemical Balance.—One of the principal features of this balance is its short beam, which is of the form of a right-angled triangle. The portions which represent the sides of the triangle act as trusses to the main portion of the beam, which represents the triangle's base; this construction, together with the tough material employed for the trusses, namely, aluminium bronze, combines great rigidity with the least possible weight; it is capable of carrying a heavy load without appreciable flexure. The knife edges and bearings are of Rock Crystal. The edges touch the planes on a very long line, by which means great durability is obtained, and an adjustment is provided which precludes any deviation from perfect parallelism of the knife edges, and which is a means of obtaining maximum sensitiveness. For weighing by means of riders a divided scale, attached to the beam, is projected in front so as to be easily seen, and to permit of the placing of riders on any division of the divided scale by means of one sliding arm, which works from the right hand side, and takes the whole range of the beam from one end to the other; this arm lifts off the riders vertically, and its action is facilitated by a counterpoise, which carries it out of the way of the beam when it is let go. The mechanism for putting the beam in and out of action is efficient:—By turning a crank handle (on the left of the base) with a forward movement, the supports under the pans are lowered, and by limited reversal of the movement of the handle, the pans can be quieted without otherwise interfering with the balance; on turning the crank further forward, in continuance of the initial movement, a bearing, on which the pans hang when out of action, is lowered, and in its descent places the pan-suspensions upon the terminal knife edges of the beam; a still further movement of the crank puts the balance in complete action by raising the central axial bearing. Messrs. Mawson and Swan are the English agents.

MISCELLANEOUS.

The Loan Collection of Scientific Instruments.—On Saturday evening last a large audience assembled in the Conference Room to hear Professor Roscoe's Lecture on "Dalton's Instruments and what he did with

THE CHEMICAL NEWS.

VOL. XXXII. No. 864.

ON THE PROCESS OF COMBUSTION WHICH TAKES PLACE IN THE INTERIOR OF CERTAIN POROUS FILTERS.

PART I.

By J. ALFRED WANKLYN,

Corresponding Member of the Royal Bavarian Academy of Sciences.

IN the year 1867, almost the first observation made by the discoverers of the ammonia process of water analysis, when they came to apply the process to the examination of natural waters, related to the exceeding purity of deep spring water.

This observation, together with the remarkable purification effected by the Thames Companies when they filter Thames water through sand filters, led me, in the year 1872, to make a further investigation into the changes which take place when water is filtered through a considerable stratum of porous material. This investigation was published, in an incomplete form, in the *British Medical Journal*. The subject has a general aspect full of interest, and not unworthy of the notice of chemists. I shall, therefore, on this occasion publish my results obtained in the year 1872, and hope shortly, on a future occasion, to publish the further results of the investigation with which I am at present occupied.

Platinum black, as is well known, possesses the property of causing combustible gases and vapours to combine with oxygen. Sometimes this combination is rapid, and accompanied with incandescence; and at other times it is slow, and unaccompanied with incandescence.

I have observed a very analogous phenomenon in the action of the peculiar material used by the Silicated Carbon Filter Company.

This material possesses in a high degree the property of causing the oxygen dissolved by the water to combine with organic nitrogenous matter when the latter is in very dilute solution.

The following experiment may be cited in illustration:—

Some of the Southwark and Vauxhall water was submitted to analysis, and gave—

	Parts per million.
Free ammonia	0.02
Albuminoid ammonia	0.14

showing that at that rate this water was almost devoid of ammonia, but considerably charged with complex nitrogenous organic matter.

The water was quickly filtered once through a cake of "silicated carbon" about 4 inches in thickness. The filtrate contained—

	Parts per million.
Free ammonia	0.16
Albuminoid ammonia	0.04

The filtrate was passed three times through the filter, and then it contained—

	Parts per million.
Free ammonia	0.14
Albuminoid ammonia	0.01

With dilute solutions of urine and of milk a similar result—viz., destruction of "complex nitrogenous organic matter," and formation of ammonia as a product of the destruction—was observed.

The rate of filtration (and of consequent destruction) was very rapid. I found that 225 c.c. of water passed through the small filter in one minute.

Thus it will be seen that passage through a "silicated carbon filter," and boiling with potash and permanganate

of potash, have the same effect upon a dilute solution of complex nitrogenous organic matter: both operations decompose complex nitrogenous organic matter, and both operations generate ammonia.

ANALYSIS OF PEROXIDE OF MANGANESE.

By Dr. T. L. PHIPSON, F.C.S.

THE following is the composition of a sample of peroxide of manganese, largely used both for laboratory purposes and in the arts:—

Water	2.02
Peroxide of manganese	72.17
Manganic oxide	6.20
Ferric oxide	3.66
Alumina	0.90
Yttria	0.10
Baryta	0.58
Lime	4.01
Magnesia	0.24
Oxide of lead	0.14
„ bismuth	trace
„ copper	0.09
„ nickel	0.04
„ cobalt	trace
„ zinc	trace
„ thallium	0.01
„ indium	distinct trace
Arsenic acid	0.15
Phosphoric acid	0.35
Carbonic acid	3.20
Potassa	0.70
Lithia	trace
Silica and rock	4.00
Loss, including fluorine	1.44
	100.00

The quantity of MnO_2 being calculated in the usual manner, the rest of the Mn was considered as Mn_2O_3 , though there are doubtless small quantities as MnO either as carbonate or phosphate. The presence of so notable an amount of phosphoric acid is remarkable. The yttria was found on adding sulphide of ammonium to the filtered liquid of the carbonates precipitated by a mixture of carbonate of soda and ammonia. Nothing was obtained at first, but after twenty-four hours there was a gelatinous precipitate, soluble in HCl , precipitated by ammonia, insoluble in an excess, insoluble in potash, precipitated by oxalic acid, soluble in an excess warm, giving no particular colour with $CoCl$ before the blowpipe, &c. This substance had all the characters of yttria, and exists here probably as phosphate. To separate the arsenic, lead, &c., it is necessary to allow the acid liquid saturated with HS to remain at least twenty-four hours, otherwise these substances escape, more or less. The gangue contains a little *rhodonite* (red silicate of manganese), quartz, silicates, and some sulphate of baryta.

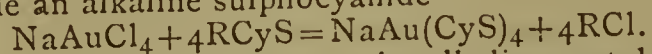
London, June 8, 1876.

ON THE ACTION OF SULPHOCYANIDES ON SODIO-GOLD CHLORIDES.

By SERGIUS KERN, St. Petersburg.

By adding to an aqueous solution of potassium sodium or ammonium sulphocyanides a solution of sodio-gold chloride ($NaAuCl_4$) a light orange-red precipitate is obtained, which easily dissolves in the liquid, giving a colourless solution. This reaction was proposed by me as a test for gold (CHEMICAL NEWS, vol. xxxii., p. 171). The orange-red precipitate is a sodio-gold sulpho-

cyanide; the presence of gold in solutions of this salt is easily detected, proving the instability of this double salt. The salt is obtained by adding to a solution of sodio-gold chloride an alkaline sulphocyanide—



In this equation R represents the alkaline metal.

A solution of $\text{NaAu}(\text{CyS})_4$ with sodium hyposulphite gives no precipitate; barium chloride produces a milky turbidity; mercuric chloride gives a white precipitate, which quickly blackens. A very peculiar reaction occurs with calcium oxide (CaO); if a small quantity of $\text{NaAu}(\text{CyS})_4$ is boiled for some time with CaO and water in a test-tube a blue precipitate is obtained.

It must be remarked that alkaline sulphocyanides give with ferrous salts a red colouration (Fe_3CyS), and as very diluted solutions of sodio-gold chloride also give with sulphocyanides, instead of an orange-red precipitate, only a red colouration, these two reactions may mislead analysts during analyses. The only difference between these two reactions is that red solutions containing a double gold sulphocyanide on being gently heated turn colourless, while the red solution due to the presence of e_3CyS on being boiled does not change its colour.

REPORT

ON THE

DEVELOPMENT OF THE CHEMICAL ARTS DURING THE LAST TEN YEARS.*

By Dr. A. W. HOFMANN.

(Continued from p. 236.)

Chlorine, Bromine, Iodine, and Fluorine.

By Dr. E. MYLIUS, of Ludwigshafen.

THE application of the three closely allied halogens, chlorine, bromine, and iodine in the chemical arts depends entirely on the energy with which they combine with electro-positive elements, more especially with hydrogen. In this respect chlorine enjoys the pre-eminence. Its most extensive application in the free state is, therefore, as a bleaching and disinfecting agent. Its efficacy depends here essentially on its remarkable affinity for hydrogen, which, under certain circumstances, even exceeds that of oxygen. Its more energetic affinities, in comparison with bromine and iodine, render it a convenient agent for obtaining the two latter. In fact the preparation of free bromine and iodine depends mainly on the decomposition of their hydrides by chlorine. A further chemical attribute of chlorine is its tendency to form with most metals soluble compounds. This behaviour is the more readily utilised, since hydrochloric acid, which may be regarded as an industrial by-product, affords a very cheap means of obtaining soluble chlorides of almost all the metals. Free chlorine is also employed as a solvent, *e.g.*, for separating and refining the precious metals, with the exception of silver.

Bromine and iodine are valuable to the chemist not so much on account of their energetic affinities as by the feeble power with which they maintain their position when combined with electro positive elements. This weaker affinity plays a part, as already mentioned, in their production, and is at the same time the foundation of their uses. Photography, in particular, is based upon the instability of the bromide, iodide (and chloride) of silver; scientific chemistry and tinctorial chemistry utilise it extensively on account of the readiness with which bromides and iodides of the hydrocarbons and of the metals are mutually decomposed.

Of less importance to the chemist is the property of bromine and iodine—like many of the rarer elements—of exercising a perturbing action upon the healthy animal

organism. The physician employs them, therefore, chiefly in combination with the alkaline metals, as valuable remedies.

Fluorine holds a distinct position from the remaining halogens, both in scientific chemistry and in technology. It is endowed with such powerful affinities as to be scarcely known in the free state, whence its energies, as a general rule, do not admit of application. Its affinity for silicium is alone utilised, hydrofluoric acid being employed for the decomposition of silicates, etching on glass, &c.

Chlorine and its Compounds.

Hydrochloric Acid.—As the initial point for the entire production of free chlorine and its compounds we still employ hydrochloric acid, which is obtained in the largest quantity as a by-product in the manufacture of alkali on Leblanc's process. The amount of hydrochloric acid liberated in this process is so enormous that if it were entirely converted into the transportable liquid acid the supply would far exceed the consumption, greatly as this has been recently extended. Hence, especially in England, little pains had been taken for the perfect condensation of the acid gas, so that vast quantities escaped into the air, and, becoming dissolved in atmospheric water, returned as rain in the neighbourhood of the works and effected manifold damage, giving thus rise to well-founded complaints on the part of the owners of the adjacent land. This rendered in England a law necessary known as the "Alkali Act" prohibiting the escape of more than 5 per cent of the total hydrochloric acid evolved. In consequence the soda manufacturers were compelled to pay increased attention to the condensation of the hydrochloric acid. The arrangements for this purpose have been improved, not only in England, but the question has been zealously taken up in France and Germany. The inducement was, on the one hand, the growing consumption of the acid and its consequent increasing value, and on the other, a wish to anticipate the complaints of the neighbouring residents and thus escape a law similar to that of England, the operation of which occasions the manufacturers decided inconvenience. In future it will be still more necessary to condense the hydrochloric acid due to Leblanc's process, as far as possible, if, as is probable, the manufacture of soda on the "ammonia" process should become more general.

Whilst formerly it was deemed sufficient to make use of the well-known *bombonnes* (groups of Woolf's bottles on a large scale) in which the gases escaping from the pan were absorbed, the method of coke-towers has become latterly universal, which enables the diluted gases to be arrested as they escape from the calcining furnace. This is greatly facilitated by the increasing use of muffle-furnaces instead of reverberatories.

The conditions to be observed in order to obtain the most perfect possible condensation of the hydrochloric acid gas, as ascertained by the exhaustive researches of E. Kopp,* G. Lunge,† and A. Smith, are as follows:—

A sufficient cooling of the gases before entering the absorbing apparatus, a sufficient volume of water, the largest possible surface of contact between the water and the gas, and the simplest possible construction of the apparatus for condensation.

(To be continued)

NOTE ON SULPHUR AS A MORDANT.

By ISIDOR WALZ, PH.D., and CHAS. M. STILWELL, A.M.

CHAS. LAUTH has recently published the contents of a sealed package, which he deposited in June, 1872, with the Société Industrielle de Mulhouse, and in which he announces the very interesting discovery that finely divided sulphur, in the form in which it is precipitated by acids

* "Berichte über die Entwicklung der Chemischen Industrie Während des Letzten Jahrzehends."

* E. Kopp, *Moniteur Scientifique*, 1866, p. 611.

† G. Lunge, *Dingl. Pol. Journ.*, clxxxviii., 322.

from solutions of hyposulphites, forms an excellent mordant for methyl green. The wool is mordanted with three grammes sodium hyposulphite and two grammes sulphuric acid, dissolved in six hundred grammes of water, and dyed in a bath containing 0.2 gramme methyl green, 0.6 gramme zinc acetate, 0.6 gramme sodium hyposulphite, and six hundred grammes water. To obtain a yellowish shade, 0.07 gramme picric acid may be added. The zinc acetate is used in order to counteract the effect of the sulphur mordant, which makes the wool soft and shrinking. A committee appointed by the Society, and consisting of Messrs. Schaeffer and Vaucher, was charged with the examination of the matter, and their report confirms the observations of M. Lauth.

It seemed to us interesting to determine whether sulphur would act in a similar manner with other dye-stuffs, and we selected eosine for the experiment. Some sodium hyposulphite was added to an aqueous solution of eosine in a test-tube, and after addition of a few drops of hydrochloric acid, the liquid was *neutralised* with ammonia. After standing a short time, a veritable lake of sulphur and eosine settled to the bottom as a rosy-pink precipitate. We next dyed a piece of woollen fabric, following the directions of M. Lauth as above, only substituting eosine for methyl green, and omitting the acetate of zinc. The result again showed that sulphur acts as a mordant for eosine, and the resulting shade is somewhat different to that obtained by omitting the sulphur, as shown by a simultaneous dyeing test.

This result induced us to test the behaviour of wool, mordanted with sulphur, towards madder. The bath was prepared in this and subsequent experiments in the proportions already described, only the dye-stuffs being changed, and the zinc acetate omitted. A "swatch" from the same piece, but not mordanted, was placed in the dye-bath at the same time, in order to facilitate comparisons.

In the bath prepared with French extract of madder, the mordanted wool took a full reddish-brown shade, while the non-mordanted cloth was but lightly stained. To decide whether this colour was due to the alizarin or to purpurin, samples of mordanted and clean wool were treated in baths mounted with artificial alizarin on the one hand, and with commercial purpurin on the other.

The samples from the alizarin bath were alike, and dyed a good yellow; those from the purpurin bath showed a light reddish-brown, the colour of the mordanted sample being deeper than the other. We, therefore, conclude that sulphur does not act as a mordant toward alizarin, but does act in that manner towards purpurin or the other colouring principles of madder.

We extended our experiments to cochineal, logwood, redwood, and fustic; but in the case of these dye-stuffs we discovered no difference between the colours produced on ordinary and sulphur-mordanted wool. — *American Chemist*.

ON THE ESTIMATION OF TANNIN, BY MUNTZ AND RAMSPACHER'S METHOD.*

By H. R. PROCTER.

As the estimation of tannin is a matter of great commercial importance, and also, chemically, one of extreme difficulty, and the methods in use are very imperfect, I venture to lay before the Society a short statement of some results which I have obtained, rather than delay to another session in order to render them more complete.

As is well known, the method named consists in forcing the liquid to be examined through a piece of raw hide by means of atmospheric or other pressure. Equal parts of the original and filtered liquor are then evaporated and the residues weighed, or the tannin is calculated from the

difference of specific gravities, it being assumed that the tannin and nothing else is removed by the hide filter. In support of this assumption, Messrs. Muntz and Ramspacher quote certain experiments in which solutions of gum, sugar, &c., were passed through the hide with but trifling loss. A solution of gallic acid, however, seems to have lost about 20 per cent, and the experimenters remark that the hide was swelled considerably, and that the filtration was extremely rapid.

I have repeated these experiments with gallic and with dilute hydrochloric acid.

In the first experiment the hide was freed from lime by soaking all night in water containing $\frac{1}{4}$ per cent fuming hydrochloric acid, and afterwards the water was removed by alcohol and the piece of hide was dried. A solution containing 0.1818 gram. dry gallic acid per 20 c.c., left on evaporation at 100° C, 0.1814 gram., and after filtration, only 0.1060 gram.—having lost 41 per cent.

In the second case the solution contained 0.094 gram. per 20 c.c., and left 0.069 gram., or a loss of 26 per cent. But in this case, say, 25 c.c. were rejected before taking the portion for evaporation. And we may assume that this had nearly saturated the hide. After this, dilute hydrochloric acid was passed through the same portion of hide. Before filtering, 20 c.c. = 6.1 c.c. normal soda, after filtering 20 c.c. = 4.8 c.c. soda, showing a loss of 1.3 c.c., or 21 per cent. By this process the hide was much swollen and rendered practically impervious to water, so that I completely failed in forcing a further portion of gallic acid through it.

A further portion of hide, of medium thickness, which had been preserved in a dilute solution of borax, was well washed first in water, then in dilute hydrochloric acid, and then again in water, and then a centinormal solution of HCl was forced through it. After, perhaps, even 100 c.c. had passed through, no trace of acid could be detected in the filtrate. A 5 per cent normal solution was then substituted, and the hide rapidly swelled and became impervious to water; but throughout no trace of acid passed through. I may here remark that my experience is quite contrary to the assertion of Muntz and Ramspacher, that swelling facilitates the passage of the fluids.

I must admit that my experiments are as yet too incomplete to allow me to speak with absolute certainty; but the conclusion which I draw from them is that the gelatinous tissue of raw hide enters into a sort of loose chemical or semi-chemical combination with acids, by which it has the power of withdrawing them from their solutions. It is probable that this combination is something of the nature of a solution of the acid in the gelatinous substance, which is very much swelled thereby. On steeping for some time in water, a certain amount of decomposition sets in, and the hide "falls," the acid probably being neutralised by the ammonia evolved. The same effect takes place rapidly in dilute ammoniacal solutions.

Since about 2 per cent solution of tannin is the strongest which is suitable for use in this process, it is obvious that the above cause must seriously interfere with its accuracy, even if it does not render it entirely fallacious. Another source of inaccuracy is that it is very difficult to ensure complete absorption of the tannin. Even with a 1 per cent solution only, a considerable quantity passed through a piece of hide 2 to 3 m.m. thick, as was shown by its forming a precipitate with gelatin and with ammonio-acetate of copper.

Action of Animal Charcoal on Ammoniacal Salts. — M. C. Birnbaum and A. Bomasch (*Dingl. Poly. Journ.*). Animal charcoal withdraws very little ammonia from a dilute aqueous solution of caustic ammonia. Ammoniacal salts also are only absorbed in small quantities, a small portion of the salt being decomposed. This decomposition is more considerable for the ammoniacal salts of bibasic acids than for those of the monobasic acids. — *Bull. de la Soc. Chim. de Paris*.

* Read before the Newcastle-on-Tyne Chemical Society, March 23, 1876.

PROCEEDINGS OF SOCIETIES.

PHYSICAL SOCIETY.

June 10th, 1876.

Professor G. C. FOSTER, F.R.S., President, in the Chair.

MR. W. J. WILSON exhibited and explained a Reflecting Tangent Galvanometer, which he has recently designed, for the purpose of exhibiting the indications of the instrument to an audience, and so arranged that the divisions on the scale show, without calculation, the relative strengths of different currents. It should be observed at the outset that this object cannot be attained by attaching a mirror to the needle, as in the ordinary galvanometer, as the angle passed over by the reflected ray is double that through which the needle is deflected. In the arrangement exhibited, the beam of light, after passing through a small orifice traversed by cross wires, is reflected vertically by a fixed mirror; the ray then passes through a lens, and is again reflected from a small plane mirror parallel to the first, which is rigidly fixed below a small magnetic needle. By this means the ray becomes again horizontal, and, since the light now falls on the second mirror always at the same angle, the extent of motion of the ray is identical with that of the needle, and, if the scale be one of equal parts placed in the magnetic meridian, the indications on it will be proportional to the tangents of the angles, and, therefore, to the strengths of the currents. The needle and mirror are suspended by a silk fibre, and a bent strip of aluminium, the ends of which dip into water in an annular trough, is attached to the needle in order to check its oscillations. A series of observations, taken with varying resistances introduced into the current, showed that the indications are very reliable.

Mr. S. P. THOMPSON then exhibited an Electromotor Clock, made by Mr. W. Hepworth, of York, and provided with a commutator of Mr. Thompson's design. This part of the instrument is very simple, and reverses the current at each single oscillation by means of two light springs resting on inclined planes. The motion of the pendulum drives the train of wheels by a modification of the gravity escapement, and a very small battery power is sufficient.

Prof. G. FULLER, C.E., exhibited and described his "Electric Multiplier," an instrument which may be looked upon as an automatic Electrophorus. An insulated plate of vulcanite is supported in a vertical position, and on each side of it is an insulated metallic plate, and these can be moved together to and from the vulcanite by rotating a handle. When these plates are far apart, two metallic arms, provided with points, are made to pass one on each side of the vulcanite plate. One of these is insulated, and is provided with a rod terminating in a nob, which at a certain point in its path almost touches the metallic plate on the opposite side of the sheet of vulcanite. The other arm is in connection with the earth. The action of the instrument is as follows: a charge of, say, negative electricity having been given to the insulated arm, it is passed over the face of the vulcanite, while positive is drawn up from the earth and thrown upon the opposite face by the uninsulated series of points. These arms are then removed, and the two metallic plates are brought into contact with the vulcanite. Call the side of the plate charged with negative electricity A, and the other B. The negative of A induces positive on the near face of its metallic plate, and repels the negative. This passes, by a strip of tinfoil joining the two faces of the vulcanite, to the other metallic plate, neutralising its free positive; and when the plates are moved away from the vulcanite, that from A is charged with positive, and that from B with negative. Before reaching its extreme position, this latter communicates its charge to the insulated

arm by the brass knob, and the electricity is then distributed over the face A. At the end of its path, B is momentarily connected to earth. It will be evident that the effect of again bringing the plates in contact is to increase the charge of positive electricity on the metallic plate opposite the face A. With the small model exhibited, Prof. Fuller has frequently obtained sparks an inch in length.

Prof. GUTHRIE then exhibited and employed Prof. Mach's apparatus for sound reflexion, which is one of an interesting series of appliances designed by him for the demonstration of certain fundamental principles in physics. It consists of a mathematically exact elliptical tray, which is highly polished, and provided with a close-fitting glass cover. The tray is covered with pulverised dry silicic acid, and a Leyden jar frequently discharged between two small knobs at one of the foci, when the silicic acid arranges itself in fine curves around the other focus.

CORRESPONDENCE.

THE CHEMICAL SOCIETY.

To the Editor of the Chemical News.

SIR,—The subjects which are agitating the minds of the members of the Chemical Society—namely, a somewhat indiscriminate process of blackballing, and a desire to give the profession of a chemist its due importance before the world—appear to be closely connected, and to need some disentanglement from the confusion into which they have fallen.

So long as the sole designation by which a professional chemist can be distinguished is that of the three letters F.C.S., an endeavour to make the Chemical Society an Institute like that of the Civil Engineers, or of the Architects, and unlike the sister Societies of Geology and Astronomy, or the Linnæan and the Geographical, is not unreasonable. On the other hand, so soon as some designation shall have been found which serves the desired purpose of assigning his due *status* to a qualified chemical practitioner, the position of the Chemical Society becomes in no sense different from that of either of these Societies.

The position assigned to our national scientific Societies by their charters, and established by their rules, is quite definite. A legal *status* has been awarded to them, and to certain of them the country gives a local habitation as well as a name, on the understanding that they are to promote their several sciences on a voluntary principle, and in a manner that shall not be tainted by a commercial spirit.

A man may be a member of the Astronomical or of the Geological Society who has never looked through a telescope or wielded a hammer. He need neither be able to read the stars, calculate a trajectory, or know the characters of a shell, a mineral, or a rock. And, notwithstanding that these Societies largely recruit their numbers from men who are purely amateurs—men who like to enlist among the more vigorous students of a science, to get copies of the Transactions as they are published, to attend the meetings for the instruction they afford and the advantage of talking to those who are more conversant with their subject—yet no one will say that these Societies have failed in honourably and most worthily holding aloft the standard of their sciences. And they have done so, notwithstanding that instrument makers, shell, fossil, and mineral dealers have been able to put F.R.A.S., F.L.S., F.G.S. after their names, and even flaunt them in advertisements.

Why should not the Chemical Society be equally large-minded and comprehensive in its admission of men who are not professionally chemists to its ranks? The answer is, of course, on everybody's lips. It is, in a word, that

Chemistry is a profession as well as a science, and some members of the Chemical Society think that the Society should be the guardian of the professional as well as of the scientific functions of chemistry. The Society will certainly have fallen on evil days when these two functions shall have been permanently confounded; the honourable and important profession of the chemist will not have emerged from its anomalous and unrecognised position until, with or without the help of the Society, it has separated these functions completely, and organised the means of distinguishing the skilled practical chemist from the amateur or the purely philosophical student of the science.

This organisation is now being arranged by its natural leaders, and surely it is no longer necessary or desirable to arrest the growth of the Chemical Society by a process of very indiscriminate blackballing. The Society discharges three important functions: it offers at once a library and a rallying point for all whose daily work or whose intellectual sympathies guide their steps to the shrine of chemistry; it publishes the memoirs in which a small proportion of its members contribute the greater part of what England adds to the archives of chemical science; and it now gives its members the best existing digest of the chemical work of the whole world.

To carry out these three objects thoroughly it must be a large Society, by no means limited to the actual working and practising chemists, and it must have the disposal of considerable funds.

And surely we are passing out of the days of exclusiveness. Why, for instance, should a man be blackballed and excluded from our Society because his calling and designation is that of a science teacher? Such a man, if he is to teach chemistry worthily, would be better, and the chemists of our Society would be none the worse, for his having the opportunities the rooms at Burlington House afford for his occasionally recruiting the knowledge he is to impart from the fountain-head. Surely it would be a miserable jealousy to push out such a man as not being "one of us," because he may not have fulfilled all the conditions properly demanded by an Institute of professional chemists from its members. It is really for the advantage of such men that the country supposes our national scientific societies in great part to exist. I will not dilate on the obvious fact that the recent blackballing has not been carried on entirely upon a high if mistaken principle. It has been singularly indiscriminate, and even the most honoured names subscribed as vouchers for the acquirements or fitness of a candidate have been contemptuously disregarded by the small minority into whose hands the secrecy of the ballot-box puts so much power.

Now, I argue from this, that there is probably a certain amount of personal feeling mixed up with the higher sentiment, and, since such feelings are transient, this part of the motive will ere long cease to exist; while, on the other hand, the impending organisation of the chemical profession will take away the only permanent source of the motive that is applying this drastic and indiscriminate purge to the Society. The object of my letter, then, is to suggest to the discontented few, first, that next to the interests of the science they would probably place those of the Society in their good wishes; that no great principle is any longer at stake in the election of candidates who, without being professionally chemists, bring on their papers the names of men generally respected in the Society; that, where the shafts fly so indiscriminately in the dark, good men will be scared, and will not let their names be the target for such random shooting; and, finally, that if there is any just ground for complaint as to the use made of the F.C.S. by a few people whose names are on the list, let the complaint be laid before the council, and action be taken on it by that body and by the Society at large. If this somewhat unseemly contention goes on, it will soon begin to be felt in the falling off of the Society, not only in numbers, but in the estimation of the world; for the exclusion of good men, whether by the

poinard of the ballot-box, or by the fear of its stroke, will soon produce its effects, and one of these effects will be a large diminution in the authority and in the efficiency of what till now has been one of the most efficient and well-sustained of our national Societies.—I am, &c.,

NEVIL STORY MASKELYNE.

To the Editor of the Chemical News.

SIR,—I am at a loss to understand the tactics of those Fellows of the Chemical Society who, by anonymous letters in your columns, uphold their action in blackballing candidates upon the grounds that these candidates have not given evidence of having done original work. Surely, Sir, this is a strange means of improving the Society. Would it not be a better plan if these anonymous gentlemen were to favour the Society with some more original work of their own?

I, Sir, not long ago proposed for election a worthy friend of mine, who is a hard-working, clear-headed, science teacher in the North of England. He is, in my humble judgment, just the kind of man whom the Society should delight to honour,—one who, withstanding the ordinary allurements of a business career, devotes his life to science teaching. Little pay, little social consideration, does the science teacher in England get; but recognition by a brotherhood of scientific men—or, at any rate, of persons forming a Society for the encouragement of Science—he surely might expect. But no! the anonymous gentlemen and their friends are so intent on *original* research that they see no merit in any other form of scientific work, and my friend was blackballed for the purpose of "improving the Society."

Sir, this state of things cannot go on. I do not know what action the President and Council intend to take in the matter. I write as an outsider. Of this, however, I feel confident,—that unless this meaningless, ill-natured (I can use no other word) blackballing of candidates simply because they have not done original work, by persons who do not show us that they have ever done anything themselves, is put a stop to, the Chemical Society will soon come to an end.—I am, &c.,

H. E. ROSCOE.

Athenæum Club, Pall Mall, S.W.,
June 13, 1876.

To the Editor of the Chemical News.

SIR,—It is only necessary to glance at your columns weekly to observe the wide and increasing sympathy of a large body of the Chemical Society with the so-called blackballing movement alleged to have been originated by certain younger members of the Society.

Ill-inspired by the conciliatory speech of the President some few meetings since, relative to this matter, which was received with such due courtesy by the Fellows present, some by no means elder members have presumed, in questioning their discretion, to rebuke these mysterious offenders; indeed, to use a colloquial but extremely apt expression of low comedy, have been playing the "heavy father" to them.

Paternal advice, however, requires support of a very substantial character, and necessarily falls somewhat flat and unheeded when given vicariously. Moreover, the "young men" alluded to are earnest and competent men, enthusiastic in the interests of their science, and strong of purpose. Having managed to get themselves admitted into the Society, they have nevertheless managed to raise its *status* by communicating original research; and seeing that the honour of the Society—and of a consequence their own honour—is threatened, in that that same learned Society is in danger of becoming a mere Club, the excuse for whose membership shall be a sham of chemical knowledge, they have made a stand against an infringement of their privileges, just as a certain young parliament—afterwards known as the Long Parliament—fought for its rights in days gone by; they have roused a storm of

opinion which no jocose banter can lull, and which, when spent, will be found to have considerably cleared the chemical atmosphere.

This, Sir, is one view of the subject; but there is a more neutral, less controvertible, point of view upon which I should like briefly to touch.

There can be no doubt that the Chemical Society counts among its Fellows all the leading chemists of England. Some of these, in addition to the communication of original research, take a justly appreciated interest in the actual welfare of the Society itself.

Now, Sir, such a Society should be a true republic of Science, where individual expression of opinion is met with attention and welcome by all, but more especially by those necessarily placed in the position of power by the great body of members.

It is evidently felt that in the Chemical Society this at present is not so,—that the opinions of many Fellows obtain no adequate representation, and can therefore only find immediate expression through the medium of the Press. Where perfect understanding does not exist, secret opposition has arisen. The case in point is the protest which has made itself heard through the ballot, by so large a section of the Society, against the admission into its ranks of men who have no place there. The arguments advanced in defence of this protest are strong; the increasing evil detracts from the *prestige* of the Society—it misleads the public; it produces, worse than all, discontent in the Society itself. But the strongest of all arguments that can be adduced is the fact that meetings have been, and are being, held with a view to bringing about so-called “organisation among chemists;” in other words, various plans have been put forward by means of which competent Fellows of the Chemical Society shall be distinguished from incompetent Fellows. Some propose the Utopian notion of a self-regeneration of the Society; we are to have Licentiated Fellows and Ordinary Fellows—perhaps the initials C.F. (Competent Fellow) and I.F. (Incompetent Fellow) are as good as any yet suggested.

It seems to me, Sir, therefore, that what is before all to be wished, with regard to the Chemical Society, is that openness of discussion should replace the present deplorably anonymous and restricted expression of opinion—that, especially with regard to the admission of Fellows, this would be conducive to a healthier state of things.

In conclusion, allow me to remark, Sir, that Fellows in voting by ballot act according to their own logical convictions, and that, this being so, the late severe black-balling must be looked upon as an expression of opinion on the part of certain Fellows whose names the ballot forbids us even to surmise. At the same time I would, with all deference, remind certain of your correspondents that it is as great an abuse of the privilege of ballot to accuse Fellows of indiscriminate blackballing as it would be to blackball from any other motive than conscientious conviction.—I am, &c.,

C.F.C.S.

To the Editor of the Chemical News.

SIR,—John Dalton, teacher of mathematics, must we not elect him a fellow of the Chemical Society? Wm. Herschel, organist; R. I. Murchison, retired officer, fox hunter; Lord Rowe, Irish Peer; Connop Thirlwall, Clerk in Holy Orders, afterwards Bishop; G. Grote, banker—are they not fit company for men of science? In our zeal to exclude mere dabblers in science from the Chemical Society do not let us exclude the *dilettanti* in the true sense of the word, the men who delight in science whether they live by it or no.

I cannot but fear that the views of some of your correspondents as to qualifications would, if adopted by the Society, practically exclude all those who do not make a profit out of chemistry, and make it easy for those to enter whose love is not for science but for what they can

get out of her, and who would be far more likely to injure the standing of the Society than any dabblers. On the other hand, the proposed plan of limiting election to those who have contributed papers of value would make the Society very select; but has the proposer counted how many of the present Fellows could claim election under such a rule; or if it were applied to the Royal Society how many even of that select body would find themselves shut out?

It is often impossible to particularise in two or three words the reasons that the proposers have, or should have, for knowing that a candidate is a really scientific man, and unless it is found safe to trust largely to the discretion of others who have signed papers, the only way to do real justice to the Society and to the candidates would be to alter the rules of election in such a manner as to ensure a fair investigation into the real merits of a candidate before presenting him for election to the whole body.

As far as I can recollect I am not acquainted with any of those gentlemen whose failure to secure election has caused so much comment, but I am strangely misinformed, if, in some cases, they would have been rightly excluded by the most rigid rules proposed by any; and, on the other hand, we must not forget that it was to the public spirit shown in openly warning us that we owed the certain rejection of one whose fate I have heard no one lament.

I hope that at least the attention called to this subject will have the effect of inciting all those who attend the meetings to take a real interest in the elections, so that the result, be it admission or rejection, may be the expression of the opinions of a sufficient number to give it real weight, as representing the general opinion of the Society.—I am, &c.,

DAVID HOWARD.

Stamford Hill, June 12, 1876.

To the Editor of the Chemical News.

SIR,—As some Fellows of the Chemical Society are suffering from obtuse vision of the state of that Society's affairs, perhaps I may be allowed to explain what the present discontent means.

Some time since the Chemical Society saddled itself with an expensive though valuable journal, the publication of which costs more money than can well be found. To meet this expense the portals of Burlington House were thrown open to all comers, and for a time there was witnessed a disgrace which, it is to be hoped, will never again be experienced. Tradesmen from pure conceit sought the Fellowship, and men who held bogus degrees rejoiced at the opportunity thus afforded them of swelling their bubble greatness. But there were men who watched and who felt the sting of this disgrace, and a stand was instituted to oppose this growing vice. It is now eighteen months since some few gentlemen, including the writer, became a nucleus for the movement, which has gradually absorbed into its vortex numerous followers and able leaders.

Mr. C. E. Groves is totally at sea when he attributes the movement to a “few young men,” for, although instituted by young men, it now embraces men of all shades of thought and position and certainly includes best part of the *working* men of the Society.

Mr. Groves will pardon the writer for regarding *him* as a young man, and for calling his attention to the fact that progress in every direction is always more or less the work of young men.

In war it is an object of care not to let the enemy have knowledge of your strength, but we can afford to neglect this precaution, for there can be no doubt that the movement under discussion is well supported, and in spite of all opposition it will accomplish its object.

Young chemists (and they are the men who carry on the work of these days) know too well how disposed

certain older Fellows of the Society are to stand still; they have secured their reputation; their position socially and professionally is determined. But it is to be feared that these same men often fail in that generosity of character which should prompt them to help younger men, as they themselves were helped in days when chemistry was not what it is. There is in England no University, no Corporation, no Organisation which stamps the chemist but the Chemical Society. Shall we, then, admit into our body men having no qualifications, and, in some instances, no principles? Are these men to become our compeers, our professional brethren?

If the Fellows of the Chemical Society will but look around they will at once perceive that those who hold aloof from the present movement are those older Fellows of whom we have spoken and some few younger men who follow in the paths illuminated by the light which they reflect as satellites to the greater planets. Astronomers know, however, that the common stars are, in many instances, of greater size than the planets which appear larger by reason of their greater proximity to the observer.

The meaning of the present outcry for organisation of chemists is but too evident. Its history is that of the movement under discussion, and it points to one and the same truth. The Chemical Society has suffered disgrace by the admission to its Fellowship of men who ought to have been elected only as Associates, or have been rejected altogether.

The men who are so indignant at the recent blackballings at the Society would have a difficulty in explaining why on the lists of the Society there appears the name of only *one* Associate: and greater difficulty still in explaining how men as Associates would fail to advance chemical science equally as when Fellows, if the only way in which they can do this is by subscriptions. As a matter of fact the scheme for the Organisation of Chemists, suggested by Prof. Frankland, amounts to a revival of the distinction of Fellows and Associates. That is to say, he proposes that whereas all existing members shall be still considered as Fellows, a higher distinction shall be given to those among them who can produce the necessary qualifications!

We sincerely trust that this scheme may be adopted, but until the future is provided for the present movement in the Chemical Society will be continued.

In conclusion, I would suggest to the younger Fellows of the Society henceforth to place their confidence in men elected from their own body, who will sustain the reputation of chemists and the Chemical Society. In like manner they should be well and largely represented in any scheme of organisation that may be developed, or, failing this, they should energetically resist it.—I am, &c.,
F.C.S.

To the Editor of the Chemical News.

SIR,—Your correspondent "Twig," and other gentlemen who have written to the CHEMICAL NEWS, are no doubt actuated by a desire to do what is right, and forward the interests of the Society of which they are members, but the opinions which they apparently entertain do not accord with my own. I do not approve of all candidates for election being "blackballed" because they have not pursued a strict course of study. But I do hold that a man must have some knowledge of and show interest in the subject before he can in any way contribute to "the advancement of Chemical Science." I cannot regard his scrip of two guineas per annum as a substitute for his want of such knowledge. A display of ignorance coupled with the letters F.C.S. is calculated to do more harm than the lack of many subscriptions. Your correspondent says—Is the Chemical Society to be an association of chemists or an assemblage of indescribables? And, again,—“As long as the Chemical Society is supposed to represent chemists,” &c. I may ask, What is a chemist? I have my own independent opinions on this as on most

other subjects, but I confess I am ignorant of any received definition of such a being. Another correspondent says—“It is useless to deny that the world looks upon these titles which Societies grant as a species of degree, and that their possession is a guarantee of the holder's proficiency in some branch of science,” and I may add that a recent article in the *World* made this very evident.

Collating the ideas and expressions of opinion, and reducing them to their simplest form, we arrive at the following conclusions:—

- (1.) That the public regard the Fellowship of the Chemical Society as a sort of degree, or stamp of considerable knowledge and skill in the Science of Chemistry.
- (2.) That the Society consists, mainly at least, of two classes of Fellows, *dilettanti* and workers.
- (3.) That there being no distinction between the two inconveniences arise therefrom.
- (4.) That the course of study for a chemist should be strictly defined.

A large amount of private correspondence with gentlemen in the country, which I have in my possession, calls for some organisation, and the facts here deduced show that this is not only desirable but necessary, because the Chemical Society appears to occupy a false position. My previous remarks on the care necessary in signing certificates arose from facts which have come within my ten years' experience. A certificate arrives by post, with a request to sign either from personal or general knowledge, the candidate being a stranger to the receiver of the letter, and unknown by any published papers. Occasionally a similar request is made in the lecture room, where one's only acquaintance with the candidate is the general contour of the back of his head. Other cases I will not enter into.—I am, &c.,

W. N. HARTLEY.

King's College, London,
June 13, 1876.

To the Editor of the Chemical News.

SIR,—The discussion now going on in your columns on the subject of the qualifications for admission to the Chemical Society cannot fail to be of the greatest moment to all interested in chemistry, both as a science and as a profession in which to gain a livelihood. It is not only to Fellows of the Society that this question is of interest. There are many, very many, who, like your correspondent "P. H.," have never yet offered themselves as candidates for election, simply because they knew how utterly worthless is the "distinction" F.C.S., and because they have seen it granted to so many whose claims to any knowledge of chemistry were perfectly absurd; also because they see how shamelessly the F.C.S. may be used as an advertisement without calling forth any notice from those at the head of the Society. Yet all those who keep aloof on these grounds would gladly have entered the Society if it had been what it should be, and what many people who do not know it still imagine it to be; and they will be very glad if Messrs. "Twig" and "Nuntius," and the other blackballers succeed in forcing the governing body of the Chemical Society to alter the rules in some way so as gradually to rescue it from the absurd and undignified position in which it now is, when so many of its own Fellows have to speak of it with contempt.

The letter from Mr. Pettengill is also of interest, inasmuch as it probably sets forth the views of a body of chemists as to the constitution and working of the much talked-of proposed Institute of Chemists. If such a body comes into real and active existence, nobody is likely to object to its rules being so framed as to completely exclude all who have not had a proper course of original training in a recognised manner and of sufficient duration, followed by a good period of practical occupation in the profession. We do not know as yet who are Mr. Pettengill's "clients," but it seems as if they were going to place themselves in a

very ambiguous and somewhat ridiculous position in one respect. Why on earth should Fellowship of the Chemical Society have anything to do with admission to membership of the proposed Institute? Surely this latter ought to be perfectly independent in every respect; and it is, at any rate, a very ill-chosen moment to propose any such rule as requiring candidates for the Institute to first be F.C.S., when the Fellows are disputing among themselves whether or not any *chemical* qualification is to be demanded before election to their Society, and when their President has distinctly stated that none such should be required. Now, if any kind of amateurs, and people with a "general interest" in chemistry are to be held eligible for the F.C.S., what possible qualification can the F.C.S. confer upon anybody as regards membership of a guild of purely professional chemists? It seems to me, and to others to whom I have spoken, that this proposed rule, and also that of giving the nomination of several members of the Council of the proposed Institute to the Council of the Chemical Society, can lead to but one inference. It looks as if the "clients" of Mr. Pettengill were afraid of treading on the toes of the Chemical Society, and had cooked up this very singular proposed rule as a kind of "sop" to that body—to compliment them, and perhaps indirectly add to the number of Fellows and consequent revenue.

Let the proposed Institute come into being as soon as possible; but let it have enough backbone to stand by itself for its own specific aims and ends, and not stultify itself by any such queer arrangement as that alluded to. If such a rule were really confirmed, one qualification for membership would be held by many "chemists" who never did a bit of chemical work in their lives. It is, indeed, high time that something was done to raise the position of chemists. All well-wishers of the Chemical Society must be glad to see an agitation which will, perhaps, put an end to this bad state of things. All well-wishers of the proposed Institute will desire that it may in no way connect itself with the Chemical Society, unless the latter is very much reformed in some respects.—I am, &c.,

London, June 13.

ANALYST.

UNITY AMONGST CHEMISTS.

To the Editor of the Chemical News.

SIR,—The discussion which is going on in your pages must have convinced the most sceptical that we are rapidly approaching a period when it will be wise and expedient to found a corporation which shall be the representative alike of the chemists and chemistry of England. We may therefore expect that questions, deeply concerning all engaged in chemistry, will soon be raised, when, let us hope, they will be freely discussed.

At present chemistry is in some sort of way represented by the Chemical Society; analytical chemistry by the Society of Analysts; and technical chemistry by the Chemical Section of the Society of Arts. But it is apparent that not one of these commands the full respect of chemists, and this for the obvious reason that they are not associations of chemists. Had it been otherwise, one or other of them might have been taken without hesitation as the nucleus of the desired institution.

It is now proposed to found what will be the fourth chemical organisation, whose primary function shall be "the general advancement of technical chemistry in its application to the arts, manufacturers, agriculture, and public health." The object of the promoters of this undertaking is laudable, and doubtless would command the general support of chemists—at least as to its principle—were it not for existing bodies which profess to do much the same thing. If organisation is to mean disintegration, it appears to me that we have had enough and to spare, and what we really want now is unity. It is the first essential to a country's greatness, and is absolutely

necessary for chemistry to attain to its maximum as a source of power.

Naturally we seek for the uniter of chemists—in fact the Bismarck of chemistry—in the Chemical Society. Grant that this, the oldest of chemical organisations, has striven to do its work through one small channel only, and has neglected altogether the interests of chemists, yet it contains within its ranks all the chemists of whom Great Britain has reason to be proud.

It would be more gratifying to me were I to hear—instead of a proposal to found an institute of professional chemists, or one for technical or scientific chemists—a proposal to found simply an institute of chemistry, whose doors should be open to all (after, of course, giving proof of knowledge and skill) engaged in chemistry whether technological or scientific. Chemistry, being so vast, makes it absolutely impossible for one mind to grapple or do justice to it as a whole, which leads me to think that, in the event of an institute or society of chemistry being founded, or the Chemical Society reformed, it would be well were it divided into sections. Let the president of the institute or society, say, be the chairman of the scientific section, and other distinguished men appointed to the chairmanship of the technological, analytical, or other sections.

The entrance to such a body should be made sufficiently difficult to ensure respect, and should be obtainable through (1) original work in any branch of chemistry, or (2) examination in the practice and principles of chemistry.—I am, &c.,

ALFRED TRIBE.

Dulwich College, June 13, 1876.

ORGANISATION AMONG CHEMISTS.

To the Editor of the Chemical News.

SIR,—The appearance of Mr. Pettengill's letter in the CHEMICAL NEWS (vol. xxxiii., p. 240) makes it desirable that gentlemen interested in the question of "Organisation amongst Chemists," should be informed that energetic steps towards the attainment of this object have been taken, and that the following brief report of what has been done should be laid before them.

After a large amount of private correspondence with most of the leading chemists in the United Kingdom for the purpose of eliciting views and opinions, the Council of the Chemical Society was asked for and kindly granted the loan of their meeting-room for the purpose of holding a *private* meeting to consider the question generally. The President of the Chemical Society kindly consented to take the chair at this meeting, which was duly held on April 27, 1876, about one hundred and thirty of the best-known chemists having been asked by circular to be present, and about fifty being actually present.

Resolutions were finally passed as follows:—

I. That it is desirable that an Organisation of Professional Chemists be effected.

Carried unanimously.

II. That in order to effect this organisation it is desirable that a body be formed for the purpose of selecting as members of the organisation such persons as may be found to be competent chemists.

Carried unanimously.

Propositions were brought forward for the establishment of an "Institute of Professional Chemists" as the "body" referred to, this institute being unconnected with any other existing corporation or society; and also for the alteration of the constitution of the Chemical Society so as to adapt it to fulfil the duties of the new "body." After a good deal of discussion a committee was appointed for the purpose of conferring with the Council of the Chemical Society, with a view of ascertaining how far that Society is able and willing to carry out a scheme for the

organisation of professional chemists, and the meeting was adjourned until such time as this committee should be able to report the results of their conference.

This committee consisted of the following gentlemen:—Mr. M. Carteighe, Mr. Dugald Campbell, Dr. E. Frankland, Mr. F. A. Manning, Dr. Redwood, Dr. Stevenson, Mr. Tuson, Dr. Voelcker, Mr. J. A. Wanklyn, Dr. C. R. Alder Wright, with Mr. W. N. Hartley as secretary.

This committee has met several times, and has conferred with the Council of the Chemical Society, which is now carefully considering the matter; as yet, however, this committee has been unable to report, because very great consideration on the part of the Council of the Chemical Society is requisite before a definite answer can be given. But it is believed that the Council of the Chemical Society will very shortly be able to give at least a provisional answer to the application made to them, and that this answer will take the form of a definite proposition for selecting from the general body of Fellows of the Chemical Society such as are found to possess skill and attainments sufficient to render them worthy of such a distinction, and conferring upon these some additional title, such as "Practising Fellow," or the like.

Probably before this proposition can be definitely made to the Organisation Committee, a general meeting of the Fellows of the Chemical Society must be convened, for the purpose of considering the desirability of such a course. Manifestly it may be that a majority of those present at such a meeting may think this proposition either objectionable on its own score, or less likely to effect the desired result than would be an institution starting *de novo*, and not fettered by a charter granted for purposes not altogether the same as those in view, nor by its government being necessarily either identical with or subordinate to the Council of the Chemical Society. In such a case, the letter of Mr. Pettengill clearly shows that the desired results can readily be obtained even without the concurrence and co-operation of the Chemical Society as a body. If, however, at such a general meeting of the Chemical Society the Fellows should give their assent to such alterations in the bye-laws of the Society, and in the constitution and mode of election of its Council, as may be necessary to render fairly practicable the scheme of uniting under one charter a purely scientific Society with a professional Association, there can be little doubt that this plan will commend itself to the acceptance of those present at the adjourned meeting of the promoters of the organisation movement.—I am, &c.,

C. R. ALDER WRIGHT, D.Sc.

Chemical Laboratory, St. Mary's Hospital, W.
June 11, 1876.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Seances, de l'Academie des Sciences. No. 21, May 21, 1876.

Fixation of Atmospheric Nitrogen by Vegetable Soil.—M. Th. Schloesing.—All the experiments prove evolution rather than absorption. Neither sealed tubes, nor the passage of nitrogen through liquids, nor variation in the proportion of the reagents, nor the exposure of the earth in an atmosphere deprived of oxygen, have presented the phenomenon of the fixation of nitrogen.

Action of the Organic Acids upon the Tungstates of Soda and Potassa.—M. J. Lefort.—Of all the mineral salts regarded as neutral in accordance with their composition, there is none which shows so decidedly alkaline a reaction with litmus as the neutral tungstate of soda. In the diluted solutions of the neutral tungstates

of potassa and soda the addition of certain organic acids such as the acetic, oxalic, tartaric, and citric, give rise to no apparent reaction. Under these circumstances there is a formation of acid tungstates, but in one special case these organic acids may behave like mineral acids, producing yellow tungstic acid.

Second Paper on the Theoretic and Experimental Determination of the Relation of Two Specific Heats in Perfect Gases whose Molecules are Monatomic.—M. Y. von Villarceau.—This paper consists chiefly of mathematical formulæ.

Lead Present in certain Platinum Points employed in Lightning Conductors.—M. S. de Luca.—It appears that the supposed platinum points of the lightning-rods attached to the Observatory on Mount Vesuvius has been adulterated with lead to the extent of 10 to 12 per cent.

Modification in Electric Batteries, rendering their Construction more easy and more economical.—M. Onimus.—The author substitutes parchment-paper for porous earthenware.

New Experiments on the Flexibility of Ice.—M. J. J. Bianconi.—The author finds that ice possesses a compressibility or plasticity, decided, but sluggish and very limited.

Nitrides and Carbides of Niobium and Tantalum.—M. A. Joly.—After referring to the researches of Wœhler and Ste.-Claire Deville, the author states that he has found that carbon combines with niobium, and that mixtures may thus be obtained containing variable proportions of nitride and carbide, the latter increasing with the temperature. Titanium also, like niobium and tantalum, combines with carbon and nitrogen, forming mixtures in various proportions.

Normal Pyrotartaric Acid.—M. Reboul.—This acid crystallises in triangular laminae belonging to the clinorhombic and hemihedral type. Its analysis gives C=45.2, H=6.0, theory requiring C=45.4, H=6.0. It distils without alteration at 299°. Its point of solidification and fusion is 96°. It is very soluble in cold water, and in boiling water it dissolves in all proportions. It is also soluble in absolute ether and alcohol. The author has examined its neutral barytic, $C_5H_6O_4Ba + 5H_2O$, and calcic salt, $C_5H_6O_4Ca + 4H_2O$.

Electrolysis of the Derivatives of Aniline, Phenol, Naphthylamin, and Anthraquinon.—M. F. Goppelsröder.—Reserved for insertion in full.

Nature of the Mineral Substances Assimilated by Fungi.—M. L. Cailletet.—There are differences between the composition of the ash of these plants and of such as contain chlorophyll. Chlorophyllous plants draw from the atmosphere under the influence of light carbon, oxygen, and hydrogen, whilst the lower plants—and in particular the fungi—seek the same nutritive elements in unstable compounds corresponding to what we call explosive bodies. This essentially different mode of nutrition may explain the notable differences found in the respective analysis of fungi and of chlorophyllaceous plants. Iron, one of the most important inorganic elements of the latter group, does not appear to be present in the fungi, which are poor in lime and magnesia, but very rich in alkalies and phosphoric acid.

Bulletin de la Societe Chimique de Paris,
No. 9, May 5, 1876.

Normal Pyrotartaric Acid.—M. Reboul.—Not adapted for abstraction.

Products of the Addition of Hypochlorous Acid to Non-saturated Compounds, and especially to Propylen.—M. Louis Henry.—Already noticed.

Source of Carbonic Acid, Characteristic of the Formines and Polyatomic Alcohols.—M. Lorin.

Correspondence from St. Petersburg, Jan. 15, 1876.—M. W. Louguinine.—M. Menschoutkine, on behalf of M.

Bunge, communicated the results of experiments on the electrolysis of aqueous solutions of oxalic acid.

M. Boutleroff, on behalf of M. Saytzeff, described certain researches on diallyl-oxalic acid and on allyl-dimethyl-carbinol.

On behalf of M. Kolly he also communicated certain hypothetical views on the composition of glucose.

He also announced that pseudo-butylen obtained from secondary butylic alcohol dissolves in sulphuric acid less easily than isobutylen.

M. Dobroslavine described the results of experiments made with saliva upon different kinds of starch, and also laid before the Society a note on the application of caoutchouc stoppers for closing tubes in which it is intended to heat aqueous liquids above their boiling-points.

M. Goldstein has studied the action of chloride of benzoyl on dinitro-diphenol.

M. Mendeleeff finds that the application of Dalton's law to the determination of the composition of different layers of the atmosphere is confirmed by the analysis of air collected by Boussingault on the summits of the Andes, and by Miller's analysis of air taken in a balloon ascent.

The same part of the *Journal of the Russian Chemical Society* contains a memoir on the synthetic pyrotartaric acid of Simpson, by M. A. Lebedeff; a paper on the presence of aceton in the urine of diabetic patients, and one on the laws governing the combination of non-saturated organic molecules, by V. Markovnikoff; a memoir on the transformation of certain hydrocarbides of the ethylenic series into corresponding alcohols, by M. A. Boutleroff; a paper by the same chemist on the milky juice of *Cynanchum acutum*; and one by E. Wagner on the action of zinc ethyl upon acetic aldehyd.

Revue Universelle des Mines, de la Metallurgie, de Travaux Publics, des Sciences et des Arts Appliqués à l'Industrie, March and April, 1876.

Commercial Assay of Sulphate of Soda.—L. L. de Koninck.—The author after referring to his former paper (*Revue Universelle*, xxv., March and April, 1874) criticises the methods given by Mr. Tate (*CHEMICAL NEWS*, vol. xxix., p. 144), Mr. R. J. Tinniswood (pp. 164 and 205), and Mr. W. Simmonds (pp. 185 and 216). The author makes the curious mistake of rendering "high analysts" as "*les essayeurs en renom*!" He concludes:—"I still hold the method which I formerly described to be the best. The only modification which I introduce consists in the simultaneous determination of the alumina, ferric oxide, and sulphate of lime. For this purpose after having precipitated the alumina and iron with ammonia I add oxalate of ammonia to throw down the lime. The precipitate is collected upon a filter and washed, calcined, treated with a few drops of dilute sulphuric acid, and heated again to redness in order to decompose the sulphates of iron and alumina."

Ammonia Process of Manufacturing Soda.—M. P. Hanrey.—A history of the earlier attempts at the manufacture of soda in the manner in question from the first experiments of Dyar and Hemming to the successful results of M. E. Solvay, whose soda-ash is said to contain 99.4385 per cent of pure carbonate of soda.

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June 1st, 1876.

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ROYAL POLYTECHNIC.—Alice in Won-

derland; or, MORE WONDERS IN WONDERLAND, New Songs, Views, and Illustrations, by Mr. GEORGE BUCKLAND. A WALK THROUGH THE TOWER, and WONDERS OF THE MICROSCOPE, by Mr. J. L. KING. Mr. Taylor's WONDERFUL BOY; Clairvoyance and Plate Dancing extraordinary. SOLOS ON THE VIOLIN by Miss C. A. BROUSIL, of the Celebrated Brousil Family. Admission to the whole, 1s.; Schools and Children under 10, half-price.

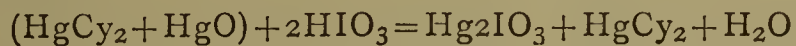
THE CHEMICAL NEWS.

VOL. XXXIII. No. 865.

ON MERCURIC IODATE: ITS PREPARATION AND REACTIONS.*

By C. A. CAMERON, M.D., F.R.C.S.,
Prof. Chemistry R.C.S.I., Medical Officer of Health and Analyst
for Dublin.

THE author finds that the statements made by Rammelsberg and Pleischl that mercuric iodate cannot be obtained by precipitation, is erroneous, except in reference to mercuric chloride. Iodic acid added to hot solution of oxycyanide of mercury, in the ratio indicated by the equation—



gives a white amorphous precipitate, almost insoluble in water, attacked with difficulty by nitric acid, but soluble readily in hydrochloric acid. Iodic acid and alkaline iodates give the same kind of precipitate with nitrate and acetate, but not with chloride of dyad mercury. However obtained, mercuric iodate dissolves very readily in solution of alkaline chlorides, bromides, iodides, cyanides, and cyanates, in disodic hyposulphite, and in hydrochloric acid. It is also soluble in dilute solutions of manganese and zinc chlorides. It is insoluble in solutions of potash, soda, ammonia, sodium sulphite, borax, corrosive sublimate, hydric disodic phosphate, alkaline iodates, chlorates, and bromates, and in acetic, fluoric, and silicofluoric acids. Treated with hydriodic acid or hydrobromic acid, mercurate iodate is dissolved with liberation of bromine and iodine. Mercuric bromate does not dissolve when treated with alkaline iodides, chlorides, &c., but they turn it brown.

When mercuric iodate is dissolved in chloride of potassium, chloride of ammonium, iodide of sodium, &c., and the solution evaporated, the salt which crystallises out first is an alkaline iodate, whilst the mercury converted into chloride forms, with excess of the solvent, a great variety of crystallisable double salts. In the cold 4 atoms of an alkaline iodide, &c., dissolve 1 atom of mercuric iodate; at a boiling temperature 2 atoms suffice.

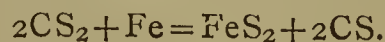
From solution of Hg_2IO_3 in $\text{Na}_2\text{S}_2\text{O}_3$ hydrochloric acid throws down a red precipitate soluble in excess of acid, and Hg_2IO_3 dissolved in NH_4Cl gives with ammonia a white precipitate insoluble in ammonia. These precipitates have yet to be examined. A mixture of potassium chlorate, mercuric iodide, and water heated in a sealed tube up to 170°C . undergoes no change, but at 200°C ., and especially with excess of chlorate, the scarlet colour of the iodide soon vanishes; on cooling, the tube is found filled with crystalline tufts, and its contents dissolve completely in water. On evaporating the solution, potassium iodate crystallises out, leaving HgCl_2 in solution.

ON THE PRODUCTION OF CARBON PROTOSULPHIDE.

By SERGIUS KERN, St. Petersburg.

THE discoverer of this compound, M. Sidot, described a year ago the results of his researches on the decomposition of carbon disulphide under the influence of solar light (*Comptes Rendus*, 1875, tome lxxxi., No. 1). The light was acting on sealed tubes containing carbon disulphide for

about two months; the compound was then decomposed without the production of gaseous matter, sulphur was obtained in solution, and a reddish-brown matter was precipitated, which Sidot found by analysis to be a new compound, viz., carbon protosulphide (CS). The same compound has been obtained by me in the following manner:—In a test-tube ordinary carbon disulphide (crude) was poured, and some pieces of clean and pure iron-wire were placed in the tube, which was next well corked and left for about one month and a half in my laboratory. On then opening the tube, the liquor in it was found to be water, which had been present as an impurity. Reagents showed in the liquor not the least presence of carbon disulphide. The test-tube contained a reddish-brown precipitate; the pieces of iron-wire were partly covered with the same precipitate, and had diminished in weight. The precipitate was found to be a mixture of iron disulphide (marcasite) and carbon protosulphide, which was produced by the reaction:—



As carbon protosulphide is nearly insoluble in hydrochloric acid, it is easily separated from the iron disulphide by means of this reagent; the CS is then liberated in the form of reddish-brown flocks.

The compound obtained, as described above, is an odorous powder, insoluble in water, alcohol, and also nearly insoluble in carbon disulphide. Sulphuric and hydrochloric acids have no action on it.

I have now prepared some grms. of carbon protosulphide, and the reactions of it with some other compounds will be communicated.

REPORT

ON THE

DEVELOPMENT OF THE CHEMICAL ARTS DURING THE LAST TEN YEARS.*

By Dr. A. W. HOFMANN.

(Continued from p. 244.)

Chlorine, Bromine, Iodine, and Fluorine.

By Dr. E. MYLIUS, of Ludwigshafen.

THE refrigeration of the gases, especially those from the calcination spaces, is most conveniently effected by means of cold air. Cold water is, indeed, employed as a refrigerant in some establishments in Germany and France, but the difficulties involved can only be successfully combatted on the small scale. In larger works cold air is preferable and is universally employed in England. Refrigeration becomes absolutely necessary where reverberatories are used for calcination, and the gases pass at once into the coke-towers. In this case the heat is so considerable that the coke may take fire, a result which has actually happened.

The apparatus employed in English works for cooling the gases consists chiefly of pipes which are either conducted straight onwards with a slight inclination, or where it is desirable to economise space, are arranged descending and ascending in the form of a U. These pipes are made of fire-clay of from 0.4 to 0.6 metre internal diameter, and fit into each other by means of contracted ends. The joints are made good with a mixture of fire-clay and coal-tar. If possible this series of pipes is carried with a fall of 0.05 to 1 metre for 35 to 70 metres to the condensers, which, to obtain the strongest possible acid, may be either Woolf's bottles or, as in many cases preferable, stone troughs. The latter have the great advantage that the many elbows of the connecting pipes, which impede the movement of the gases, may be more easily dispensed with. They are best made of sand-stone

* Abstract of a paper read before the Society of Public Analysts, 14th June, 1876. Communicated by the Author.

* "Berichte über die Entwicklung der Chemischen Industrie Während des Letzten Jahrzehends."

slabs let into each other and held together with tie-rods which pass through the projecting ends of the side slabs. Or the slabs are fitted together with their edges bevelled to an angle of 45°, tightened by the interposition of strips of caoutchouc, and the whole clamped together with a somewhat expensive iron frame work. Lunge decidedly recommends the former plan. Troughs cut out of a solid block are sometimes used in Germany, especially in the neighbourhood of sand-stone quarries. Such, however, if of large size are very costly, and if once damaged can scarcely be repaired. The sand-stone used in Germany after it has been shaped out requires to be saturated with coal-tar, as it is otherwise unable to resist the action of the acid. Very hard stone, such as the carbonaceous sandstone (Kohlen sandstein) quarried at Herdecke and Wetter, in Westphalia (Hasenclever), does not require this preparation. Many English sandstones are sufficiently compact to resist the muriatic acid without any preparation (see the section on bromine below).

In these stone troughs a great part of the muriatic acid is condensed. The gases not condensed pass into the base of a coke-tower divided into two sections. They ascend in one of these and descend in a pipe fixed outside the tower, ascend again in the second compartment of the tower, and are thence led through a descending pipe into the chimney. The exit pipe is fitted with a damper to regulate the draught. The second compartment of the tower serves especially to free the gases which pass through it entirely from muriatic acid. According to the arrangement described the gas traverses both sections of the tower in a direction opposite to the descending current of water. It would be possible to dispense with the two external descending earthenware pipes if the two towers were connected above, and the gas were allowed to pass in the second tower in the same direction as the water. Thereby, however, a much less perfect condensation would ensue in the second tower, and a larger descending flow of water would be required.

(To be continued)

ADDITIONAL NOTE ON BALANCES.*

By Prof. ALDIS.

AN examination made by Prof. Herschel of Bunge's balance since the last meeting gives the following dimensions, with the notation of the former note:—

$a = 64$ m.m. $h = 0$ (probably). $k = 0.175$ m.m. $W = 40$ grms.

A similar examination of a balance by Kleiner, which has been used in the laboratory for some time, gives for that balance—

$a = 198$ m.m. $h = 0.5$ m.m. $k = 1.25$ m.m. $W = 110$ grms.

The weights in the two cases are thus nearly proportional to the lengths, Bunge's being slightly heavier in proportion. The cause suggested at the last meeting of the superior sensitiveness of Bunge's balance, namely, that the beam was lighter than that of other balances out of proportion to its length is, therefore, not the correct explanation.

The real reason is that the values of h and k are so enormously smaller in Bunge's balance than in the balances in ordinary use. The formula for the sensibility, viz.:—

$$\frac{a}{Wk + 2Ph}$$

gives for Bunge's balance with the above data a sensibility ten or twelve times as large as that of the Kleiner balance, owing to this great decrease in the value of k .

The phrase used in the prospectus of Bunge's balance, that, *other things being equal*, the sensibility of a balance

varies inversely as its length, has been supposed to contradict the ordinary view. The apparent contradiction lies in the fact that in the ordinary theory the phrase, "other things," means the values of W , P , h , k , while in the prospectus of Bunge's balance it means merely that the times of oscillation of the two balances compared are the same.

Referring to the formula for the time of oscillation of the balance which ought to have been given in the former note as—

$$\sqrt{\frac{2Ph + Wk}{Wk'^2 + 2Pa^2}}$$

where k' is nearly proportional to a , it is clear that the times of oscillation of any two balances are nearly proportional to the values of $\sqrt{\frac{k}{a^2}}$ for those two balances.

Consequently, if the beam of one be three times as long as that of the other, in order that the two balances may swing in the same time, the value of k for the longer balance will have to be nine times its value for the smaller; and if the weight of the larger be assumed to be three times that of the smaller, the sensibility of the larger

will, by the formula $\frac{a}{Wk}$ assuming h in both cases to

vanish, be one-ninth only of that of the smaller.

The writer regrets that in the former note the vertical lines of the diagram were not engraved perpendicularly to the horizontal ones, and that by an error, for which he is solely responsible, the same letter D was used to represent two points in the diagram. This will, he believes, cause no confusion if the attention of the reader is drawn to it at once.

SOME NOTES ON ANILINE-BLACK.

By S. CABOT, Jun.

THERE has been much discussion, in late years, upon the subject of aniline-black, both as to its technical application, and with regard to its chemical properties and formula.

My experiments upon the latter part of the subject lead me to believe that, as ordinarily seen upon cloth, aniline-black consists of a salt, or perhaps a mixture of salts, of a number of organic acids of a red colour, combined with a base of a greenish colour.

This base I believe to be quite similar to rosanilin in its chemical constitution, but not so strong a base. Like rosanilin, however, it contains (if freed from the acids mentioned above), I believe, no oxygen. It can combine weakly with mineral acids, and I may at some future time be able to discover its atomicity through means of this fact.

The red and purple-coloured acids, on the other hand, appear to be oxidised products; some of them, like indigo, may be reduced by the action of grape sugar and alkali, and are reoxidised by contact with the air.

In the ordinary methods of making aniline-black, i.e., by the action of chloric acid upon aniline oil, large quantities of ammonia salts are found which remain in the filtrate, as may be easily seen by boiling the same with an alkali. This fact proves distinctly that the black itself must contain less nitrogen and less hydrogen than the aniline from which it is made.

And now with regard to the practical part. It has long been a great "desideratum" to obtain a steam aniline-black that was sufficiently good and cheap, and did no injury to the fibre. Many printers, knowing that acetic acid was a volatile acid, and not of great use in steam colours, have attempted to substitute it for other acids in the printing of this black, with absolutely negative results. I have shown by experiment that the cause of

* Read before the Newcastle-upon-Tyne Chemical Society, February 24, 1876.

this ill success is the formation of *acetanilide*, upon which the oxidising agents have not the effect desired. I believe in the case of the use of tartaric acid the same difficulty is sometimes to be experienced through the formation of a similar compound in which one of the hydrogen atoms of the aniline is replaced by the acid radicle of tartaric acid. Where chlorate of potash is used, however, this tendency is partly counteracted by the great tendency to form the insoluble bitartrate of potash.

The only steam aniline-blacks that appear as yet to have any great technical value are those made with ferro- or ferricyanide of aniline in connection with a chlorate.

The great step which should next be made in the preparation of this colour, is the introduction of the use of more soluble chlorates than that of potash—for instance, that of soda. Much inconvenience would thus be avoided, from the crystallisation of the chlorate of potash in the colour while printing, and the consequent destruction of both "doctors" and "rollers." It would also render possible the use of much stronger solutions of chlorate, which will be of great use in steam colours.—*American Chemist*.

ON THE ESTIMATION OF PHOSPHORIC ACID IN FERTILISERS.

By ALBERT H. CHESTER, E.M.

In testing some samples of raw bone for phosphoric acid, I tried a method by R. Warrington, given in the second supplement to "Watts's Dictionary," with such good results that I thought it worth while to make a comparison between it and the method by molybdate of ammonia. To distinguish it, I shall speak of it here as the citric acid method.

Three different samples were tried by each method, giving the following results:—

No.	Weight taken.	Molybdate method, per cent P_2O_5 .	Citric Acid method, per cent P_2O_5 .
No. 1	1 grm.	22.00	22.05
" 2	$\frac{1}{2}$ "	3.68	3.74
" 3	1 "	18.73	18.77

The first method was worked with extreme care, every precaution being taken to insure accuracy, so we may unquestionably use the citric acid method for all commercial analyses, as the results, though uniformly higher, are certainly not enough out of the way to impair the value of the work. To insure such accuracy certain precautions must be taken, so the method is here given in detail.

The ground bone is first dissolved in hydrochloric acid, and the solution nearly, but not quite, neutralised with ammonia. Lime is then precipitated hot with ammoniac oxalate, and allowed to stand for several hours. The supernatant liquid is then decanted through a filter, and the precipitate washed once, by decantation with hot water. It is then dissolved in as little hydrochloric acid as possible, and re-precipitated by carefully adding ammonia nearly to neutralisation, when it can be filtered and washed as usual. This final precipitate of calcic oxalate is perfectly free from phosphoric acid, and the filtrate from lime. A sufficient amount of citric acid to keep in solution any iron and alumina that may be present is then added, and afterward ammonia is added to alkaline reaction. For bone, the amount of citric acid needed is very small. Magnesia mixture may then be added to the ammoniacal liquid, and the phosphoric acid determined as magnesian pyrophosphate. In case the ammonia produced a precipitate, it should be carefully examined to see whether it is the result of an insufficient amount of citric acid, or on account of the presence of magnesia in the sample; which last, of course, would make no difference in the final result.—*American Chemist*.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, June 15th, 1876.

Professor GLADSTONE, F.R.S., Vice-President, in the Chair.

THE names of the visitors having been announced and the numbers of the previous meeting confirmed, the names of Messrs. E. H. W. Swete and C. Law were read for the first time. Messrs. Arthur Brownhill Cortis, George F. Thomson, John Heron, Charles George Matthews, George Evans, and Dr. Otto N. Witt were balloted for and elected after their names had been read for the third time.

The first communication, "*Chemical Studies*," was given by Prof. DEWAR. The first note was on a method of producing a vacuum by the absorption of gases or vapours. In one instance he had tried to remove the residual hydrogen by palladium, but as the tension of hydrogen-palladium at the ordinary temperature is 2 or 3 m.m., it was evidently inapplicable. He had met with better success by using bromine, and absorbing the residual vapour by means of carbon; it was merely necessary to boil off a small quantity of bromine in a tube so as to expel the air, seal it, and allow it to cool; the carbon, which is confined to a small portion of the tube by a constriction, then absorbs the residual vapour so completely that the electric discharge gives very broad striæ, which are symmetrical at the two poles, a sign of a very good vacuum. It is curious that at ordinary temperatures the spectrum of the discharge gives no bromine lines, but only carbon lines, due to a trace of carbonic anhydride; when the carbon is heated, however, the bromine lines make their appearance. Chlorine or carbon bisulphide may be substituted for bromine. The author had found the perfection of the vacuum proportional to the heat developed by the absorption of the vapour by the carbon. He also exhibited some exhausted tubes, containing phosphorus, which had been partly exposed to the light; here the phosphorus vapours had condensed and crystallised on the glass in the polymerised state. After some remarks on the latent heat of dissociation of ammonium carbamate, which he had found by experiment to correspond very closely to that required by the dynamical theory of heat, he described a method by which he had determined the latent heat of the formation of ozone from oxygen; this was done by passing it through a solution of hydriodic acid, observing the heat developed, and making a correction for that due to the decomposition of the hydriodic acid. It was found to be between 5000 and 6000 units.

The Chairman having thanked the author,

Dr. ARMSTRONG gave a short account of his "*Researches on the Reduction of Nitric Acid and on the Oxides of Nitrogen* (Part I. *On the Gas Evolved by the Action of Metals on Nitric Acid*)," made in conjunction with Mr. Ackworth. After referring to the statements in the textbooks, and reading extracts from Odling and from Gmelin, as representing the present state of our knowledge of the subject, which was very imperfect, from the fact that the experimenters had merely considered the qualitative composition of the gas, and not the relation of the amount of gas to the metal taken, he stated that it was now universally admitted that the first action of the metal on nitric acid was that represented by the equation—

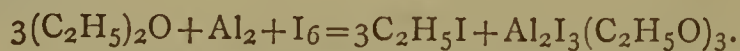


and that the hydrogen at the moment of its liberation acted on the excess of nitric acid, producing either nitric or nitrous oxide or free nitrogen. The theoretical amount of gas per unit of metal, R'' , dissolved being 14920 c.c. for NO, 5595 for N_2O , and 4475 for N. The method of experimenting adopted consisted in dissolving a known weight of the metal in excess of acid, *in vacuo*, carefully

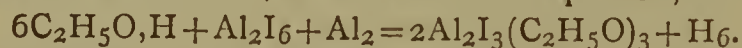
collecting the evolved gas by means of a Sprengel pump and then measuring it. The results were exhibited in a series of tables showing the temperature, strength of acid, total amount of gas, percentage of NO, N₂O, and N, also the c.c. of gas per unit of metal, which never exceeds that required by theory. With copper the amount of NO is comparatively large, although the presence of cupric nitrate in large quantity exerts a considerable influence on the result, increasing the amount of nitrous oxide. With zinc the N₂O obtained is comparatively large, and, as with copper, the presence of much zincic nitrate increases it. In the case of cadmium and magnesium the former yields less N₂O than zinc, whilst the latter gives more, showing that cadmium is less active and magnesium more active than zinc. Tin yields chiefly N₂O. Nickel differs totally from iron in its action on the acid; the result being chiefly N₂O with the latter, NO with the former. It was found that chemically pure nickel obtained by reduction in a current of hydrogen, gave off hydrogen when dissolved in nitric acid; this was most probably occluded hydrogen. Silver, lead, and thallium were also tried. The authors especially call attention to the fact, clearly shown by these results, that the action of the hydrogen molecule differs greatly in its action on the excess of nitric acid according to the metal by which it is liberated.

Mr. C. T. KINGZETT then read a paper "*On an Alkaloid Obtained from Jaborandi, its Platinic Compound, and their Formulae.*" The alkaloid was obtained from the aqueous extract of the plant by precipitation with phospho-molybdic acid and the precipitate decomposed by baryta, or the extract was concentrated by evaporation, mixed with alcohol to throw down albuminous matters, &c., and after treatment with ether and ammonia extracted with chloroform. Although no crystalline hydrochloride of the alkaloid could be obtained, the platinic compound forms distinct reddish yellow octahedral crystals of the formula C₂₃H₃₅N₄O₄.2HCl.PtCl₄.

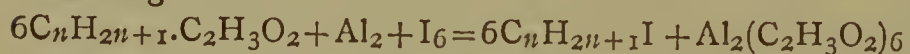
The CHAIRMAN having thanked the author, gave a short abstract of a paper by himself and Mr. A. TRIBE, on "*The Simultaneous Action of Iodine and Aluminium on Ether and Compound Ethers.*" The authors find that although ether is not affected when boiled with aluminium and aluminic iodide, yet when treated simultaneously with iodine and aluminium a powerful action takes place, the products being ethylic iodide and aluminic iodoethylate:—



The iodo-ethylate may readily be obtained in a state of comparative purity by treating alcohol with aluminic iodide and aluminium, as shown in the equation,—



It is a brown non-crystalline substance. The authors have also examined the action of iodine and aluminium on ethylic and amylic acetate, and find that the products are ethylic and amylic iodide and aluminic acetate, the reaction being—



The next paper was "*On some Compounds of Antimony Pentachloride with Alcohols and with Ether,*" by Mr. W. CARLETON WILLIAMS. In order to prepare these compounds the pentachloride is cautiously mixed with the alcohol, taking care to avoid rise of temperature, and the product purified by crystallisation from alcohol or ether. They are hygroscopic, readily soluble in alcohol or ether, and are decomposed by distillation or by the action of water. The methyl alcoholate, SbCl₅.CH₄O, forms pale yellow plates melting at 81° C.; the ethyl alcoholate, SbCl₅.C₂H₆O, crystallises in long colourless needles, which fuse at 66° C.; the amyl alcoholate is also a white crystalline body, whilst the ether compound, SbCl₅.C₄H₁₀O, exists as a finely divided greyish white crystalline powder, which melts at 68° C., and decomposes slowly at the ordinary temperature, rapidly at 70° C.

A paper by Prof. J. W. MALLET "*On the Volatility of Barium, Strontium, and Calcium*" was then read. The author has ascertained by a series of carefully made experiments that when lime, baryta, or strontia are heated in contact with metallic aluminium to a very high temperature in a carbon crucible, it suffers an appreciable loss of weight, in some instances to the extent of more than 3 per cent, indicating that the alkaline earth must have been partly reduced and the metal volatilised. This supposition is confirmed by the observation of the flame of the carbon monoxide which is given off, the characteristic lines of the metals being distinctly visible when it is examined with the spectroscope.

The next communication was "*On the Action of Chlorine on Acetamide,*" by Dr. E. W. PREVOST. It gives rise to two substances, one of which melts at 68° C. and the other at 129.5° C. They both crystallise in colourless needles, but the author was not able to deduce any satisfactory formula from the analytical results.

In a "*Note on the Perbromates*" Mr. M. P. MUIR corrected his former statement that perbromic acid may be easily prepared by the action of bromine on an aqueous solution of perchloric acid (Kammerer's process). His subsequent attempts to prepare perbromic acid in this manner having been invariably unsuccessful.

The last paper was by Dr. J. G. BLACKLEY "*On a New and Convenient Form of Ureometer.*" In it he gives a description of a simple form of apparatus analogous to Russell and West's ureometer which is exceedingly convenient for clinical purposes, the method consisting in measuring the amount of nitrogen evolved on decomposing urea by sodium hypobromite. After making the correction, pointed out by Russell and West, the experiments made with a view of testing the apparatus gave very concordant and satisfactory results.

The CHAIRMAN having thanked the authors in the name of the Society adjourned the meeting.

NEWCASTLE-UPON-TYNE CHEMICAL SOCIETY General Meeting, March 23rd, 1876.

JOHN PATTINSON, President, in the Chair.

THE minutes of the last meeting were read and confirmed.

Messrs. S. B. Malcolm and H. M. Habinshaw were unanimously elected members.

The name of Mr. N. H. Martin was read for the first time.

Mr. B. S. PROCTOR read a paper on "*The Effect of Flexibility on the Working of Chemical Balances.*"

Departure of Dr. Lunge.

The PRESIDENT said they were all aware that a prominent member of the Society was going to take his departure from amongst them, having been appointed to a Professor's Chair in the Polytechnic School of Zurich—he meant Dr. Lunge. Dr. Lunge had been a most active member of the Society from its commencement. He acted at one time as its Secretary, and for many years furnished very interesting abstracts from foreign journals of chemical matters of interest to members in this district. Dr. Lunge had also contributed a great number of very interesting papers, and had filled with great distinction the office of President of the Society. Under these circumstances, it would be wrong to allow him to pass away from their midst without expressing to him their sense of the obligation they were under for his many useful papers and duties in connection with the Society, and wishing him God speed in his new undertaking. He believed Mr. Swan had prepared a resolution on the subject.

Mr. JOSEPH SWAN said he thought it would probably meet the wishes of the Society if it were practicable, and in accordance with its rules, to elect Dr. Lunge an honorary member of that Society.

The PRESIDENT believed there was nothing bearing on it in the rules, but they could make a rule.

The SECRETARY—We can make a precedent.

Mr. SWAN said, believing this would be the feeling of the meeting, he moved the following resolution:—"That this meeting congratulates Dr. Lunge on his election to the Professorship of Chemistry in the Polytechnic School of Zurich; and in view of the consequent severance of his personal connection with the Newcastle Chemical Society, deems this a fitting occasion for acknowledging and placing on record its high appreciation of the services rendered by Dr. Lunge both as a principal member of the Society during the entire period of its existence, and as a frequent and most able contributor to its proceedings." He felt sure that he need not add to the resolution a single word to commend it to the members; for he was uttering what was generally felt when he said they were all deeply indebted to Dr. Lunge for his long, painstaking, and valuable services rendered to the Society.

Mr. GLOVER rose with great pleasure to second the resolution. He could bear testimony to all Mr. Swan had said in relation to Dr. Lunge's efficiency both as Secretary and President, and as an ordinary member of the Society. They had reason not only to respect Dr. Lunge's talents and ability, but those who knew him best could bear testimony to his worth as a man. On these grounds he begged leave to second the motion.

The motion was then put and carried unanimously.

On the motion of the same gentlemen, Dr. Lunge was unanimously elected an honorary member of the Society.

Dr. LUNGE said he should not attempt to express the feelings which at that moment were uppermost in his mind, because he would have to make a long speech, and he ought not to inflict it upon them, looking at the business before them, and also because he could not say all he felt for that very unexpected honour. He had never had any other idea than to remain an ordinary member of the Society for the rest of his life. That he should have been honoured by being elected the first honorary member of the Society had never for a moment entered his mind, and he could assure them he appreciated it to its full extent; and he appreciated almost more the kind words which the mover and seconder of the resolution had spoken about him. He could say with the utmost sincerity that many of the pleasantest hours he had spent during the nearly eleven years he had been in this part of the country had been spent in that Society—in that very room—and in the intercourse he had had with many of his scientific brethren. He was going abroad, as they knew, but he should never cease to be an Englishman; because, as they knew, he was an Englishman by adoption. Nor should he ever cease to be a member of their Society—not only in name, but, he hoped, also in sympathy. He hoped his contributions of papers were not at an end, but that he would now and then still contribute. He begged to tend them his sincerest thanks, and more especially to the mover and seconder, for the great honour which had been bestowed upon him.

The Explosion in Grey Street.

Mr. FREIRE-MARRECO—As a good many versions of the accident in Grey Street have been circulated, some of them exceedingly wild ones, and as in some respects the explosion itself was rather interesting, I thought it might, perhaps, not be altogether amiss that we should have some short account of it recorded in our "Transactions." Without going into detail as to all the evidence that has been collected on the subject, I need not remind anybody that there was an explosion in Grey Street, close to the Bank of England, and a tolerably severe one. With regard to the explosion itself, the only point of special interest was the direction the sound-wave took, which was very well marked. A certain amount of damage was done on the ground floor of the Bank; the sound-wave seems to have crossed the street, and to have damaged the first-floor windows on the opposite side. It

was then reflected very clearly and distinctly, so as entirely to miss the *first-floor* windows of the Bank, and scarcely to leave a single window unbroken in the top storey. That is all the interest attaching to the explosion itself. As to the cause, it seems perfectly clear that on the morning of the explosion a gas bag—which was supposed by the sender to be an oxygen bag, but which in reality was that which had been used to contain coal gas for the lime light—was sent to be filled, and was returned with a message that it was the wrong bag. The sender had in the meantime, probably supposing he had still got hold of the hydrogen bag, put coal gas into the real oxygen bag. We can hardly say very accurately what quantity he put in; but I should say, putting the different accounts together, the probabilities are that he had somewhere about 2 cubic feet of explosive mixture in the bag. Of course, whether it was mixed at all accurately in the proportions necessary for explosion, we can only speculate. The accounts of the principal actors in the filling of the bag are very discrepant; in point of fact, they contradict each other flatly in one or two respects; but on the balance of evidence, these are the conclusions one can hardly help coming to.* Now we have this boy started with the bag—and it seems to have been rather a leaky one—down Grey Street; and, as we all know, he had got as far as Messrs. Reid's shop when the explosion occurred. I was at first inclined to think that the boy, being possibly an amateur chemist, and knowing there was gas in the bag, had been experimenting with a lucifer match on his own account, the more so as the police found lucifer matches in his pocket. But one witness, who picked him up immediately after the explosion, says he watched him for some little distance down the street, and is quite certain that he did not touch the bag in any way except carrying it, and did not light it, and did not seem playing any tricks with it. Then the difficulty arises—how did it go off? Because, of course, the suggestion as to concussion by its falling on the pavement—the boy says he dropped it on the pavement to rest himself, being tired—is a perfectly absurd one; and so are one or two others which have been suggested. But, luckily, we have another witness, who says he saw a man come up just behind the boy with his pipe lighted, and throw away the match with which he had apparently just lighted it. This witness says he saw the match fall, still red, towards the bag, and the explosion immediately followed. So that, after all, it was no very extraordinary circumstance chemically which brought about the explosion. But the point of interest seems to be the rather long train of coincidences which was required. First, you get a man who does not know much about the management of lime lights to make an explosive mixture in a gas-bag; then, that gas-bag has to be a leaky one; then the leaky bag has to be placed on the pavement; and last, not least, some one has to light his pipe and throw away the match. The evidence seems to show very clearly that all this did occur, and I think the conclusion to be drawn from it is that it is very unlikely the same train of circumstances will ever come together again—at least in Grey Street. The violence of the explosion can be judged of from the specimens on the table; these are the only two pieces of the bag which we have been able to get hold of, and this little piece, I believe, was projected through one of the top windows of the Bank, so that the explosion must have been a pretty violent one. There arises naturally the question of how far the accident could have been prevented. I don't know that it could have been prevented as long as you have gentlemen dealing with lime-lights, one of whom says his usual practice is, "I first turn on my jet of oxygen and light it; when that has warmed the lime sufficiently, I turn on the common gas. I use my own judgment;" and how far his own judgment is to be relied upon I think we can all of us see. Then we have the other manipulator of the gas-

* I am indebted to Captain Nicholls for an opportunity of examining the evidence taken by the police authority.

bag, who says he filled it partly with coal gas; "that I sent down to the dealer; I thought he would put the oxygen into it and mix it. I am not acquainted with the management of experimental gases;" and I think we shall all endorse his description of himself. These are all the points of interest in the explosion, but I thought it as well that we should have some short account of it in our "Transactions."

The PRESIDENT said he was sure they would all join in thanking Mr. Freire-Marreco for his interesting account of this explosion.

Mr. GLOVER thought the Society ought to bear its testimony to the fact that the authorities of the town should have some control over the retailing of such a dangerous mixture as that of oxygen and hydrogen.

Mr. JOSEPH SWAN—I think it ought in fairness to be said that such a mixture of gas was probably never in Grey Street before, and that the likelihood of its being in Grey Street again is extremely remote. It is entirely unusual to make a mixture of oxygen and hydrogen. Formerly, as every chemist in this room knows, it was a common practice to make a mixture of hydrogen and oxygen, and burn the two gases from one jet; but it is long since that practice was abandoned, and now the regular course is either to use two bags—one containing coal gas and the other oxygen—or to use only one bag, oxygen, the other gas being taken direct from the main; and this latter is the general practice.

(To be continued.)

CORRESPONDENCE.

ORGANISATION AMONGST CHEMISTS.

To the Editor of the Chemical News.

SIR,—In an article in *Nature* of the 8th inst., "On the Organisation of the Profession of Chemistry," I find the following:—"But examining bodies, and bodies that issue certificates to those who pass their examinations, are ready to hand."

I have been surprised, in following the correspondence that has of late appeared in the *CHEMICAL NEWS* on the above subject, to observe that attention has not been turned to what I should have considered the most natural source from which aid might be expected. I refer to the Royal College of Chemistry, South Kensington—an institution established and supported by Government for the furtherance of chemical science. One of the ablest chemists in the country presides over this Institution, aided by an able staff of assistants, and with every facility at his disposal either for the preparation or examination of candidates for the chemical profession.

Here is the establishment for which we are casting about ready formed to our hand, wanting but the addition of a few more Professors to form an Examining Board, and which could have no more legitimate object than the present.

Being a Government and not a private institution, we might reasonably look for an impartial exercise of the responsible duties required in the present case; and it has been a matter of wonder to me that it has not come forward voluntarily to offer its services.

I can quite go with all that has been said by your correspondents as to the crying need there is to have the profession placed on a proper footing, and could recount many anecdotes of the low estimate set on our services far surpassing those given by your correspondents. The two following will, however, suffice:—

The manager of a large iron works, lately turned into a company, sent for me to ask my terms for complete analyses of iron ores, as some members of the board had actually ventured the hitherto unheard-of suggestion that it might be well to have the ores, which were being bought

in thousands of tons on the faith of the seller's analysis analysed for a check on the quality. As a large number of analysed were promised, I abated my usual charge of £3 3s. to £2 2s., knowing that a neighbouring analyst was ready to make *single* analyses at this unremunerative rate. The enlightened manager seemed much surprised, as he had thought £1 rs. would have been ample remuneration.

But even this is surpassed by a story lately told me by an eye-witness. A man walked into a well-known laboratory one day with a sample of gold quartz from North Wales. There being something suspicious in his manner, he was told that the fee would be so much; when he replied, "Oh, I thought you would have done it for the value of the gold in it."

What between ignorance on the part of employers, and competition on that of unqualified practitioners, it is high time that those who are properly qualified should protect themselves; and I trust that the matter will not now be allowed to drop until such protection is obtained. With this view I have made the above suggestion.—I am, &c.,

A. R. S. M.

Chemical Laboratory, Birmingham and Edgbaston
Proprietary School, June 15, 1876.

THE CHEMICAL SOCIETY.

To the Editor of the Chemical News.

SIR,—It would appear that the Chemical Society, like almost every other similar scientific society, has a large number—perhaps nine-tenths—of inactive members, who contribute nothing but their subscriptions; and we may fairly infer that their subscriptions are of considerable importance when taken collectively. It appears to me that the qualification necessary for Fellowship is simply that the candidate shall be a "decent fellow," and one who is likely to put out his hand to help the general objects of the Society as far as he is able.

If greater restrictions had been enforced for admission to the Fellowship from the first, the power for active good to the public would, I think, have been cramped seriously for want of funds, although the scientific position of the Society would doubtless have been higher than it is.

I would suggest a slight alteration in the rules, which would, no doubt, eventually raise the position of the Fellows to what they would wish it to be without throwing any check on the funds, which would at present possibly cause serious trouble.

That all candidates shall be, when first proposed, elected only as Associates, having every privilege equal to the Fellows except as regards official service. That any Associate shall, after contributing to the Society a paper containing the results of his own original research, be at once eligible for election to the Fellowship, and that his election shall be valid if three-fourths of the members present vote in his favour.

Fellowship of any scientific society to be obtained only by examination, would, in my opinion, be a farce, and would immediately reduce the admissions to about zero; and any examination for professional chemists ought to be conducted by a separate examining body having no official connection with the Chemical Society. I would suggest that all professional chemists shall pass any examination necessary at the College or University at which they were educated.

The Chemical Society certainly has nothing to do with this matter; its meeting-room is a rallying point for friends, mutually interested in the same subject, to meet and exchange ideas. If any member has no ideas to exchange, but gives help other ways to the best of his ability, there is no reason why he should be badly treated; and the contribution of original scientific work might, without causing any ill feeling or difficulty, make a strong mark between the two matters which go to make success—*brains and money*.—I am, &c.

T. F.

To the Editor of the Chemical News.

SIR,—When lions enter the arena, the work should be worthy of kings. You may therefore expect, Sir, it was with eagerness that I read in your pages the letters on the Chemical Society by Profs. Maskelyne and Roscoe. But conceive my disappointment in finding Prof. Maskelyne merely reiterating, with somewhat more of detail, the story told to us a few weeks since by Mr. Warrington, and so strongly condemned by the majority of your correspondents. Instead of likening our Society to what these gentlemen term the sister societies of Geology and Astronomy, it would have been more to the purpose had they reminded us of the fierce battle which raged, not so very long ago, in the oldest of learned Societies, between money and merit; in which the latter, to the Society's honour, came off to a great extent the victor. The reformers in those days were doubtless sneered at as much as Prof. Roscoe has considered it his duty to sneer at the men who are striving to drag the Chemical Society out of its present miserable condition. But let us hear the charge made against us. Prof. Roscoe has a worthy friend—a science teacher—whom, he tells us, was rejected for the purpose of “improving the Society.” The words between inverted commas I take for an attempt at irony. If not, we are on this point in perfect agreement. Now it must be known that the title “science teacher” may be used by any one who proves, to the satisfaction of the Science and Art Department, that he possesses a third rate book knowledge of any one of about twenty subjects, more or less scientific. “Science teacher,” therefore, not necessarily affording any evidence of chemical knowledge, should not be taken as a qualification for the Fellowship of the Chemical Society. But the Professor may aver, “I certified to my friend's eligibility when I signed his paper.” Personally I entertain a just respect for Prof. Roscoe, but since he considers a chemical qualification unnecessary, and I do, for the Fellowship of the Chemical Society, I can attach no value to his signature when I find it on a candidate's certificate. I trust this will be taken as a sufficient explanation of our “tactics.”

Mr. Hartley has once more done good service to the cause in collating some of our ideas and expressions of opinion, and in giving us such a graphic description of the process by which candidates get signed up. We earnestly recommend the unconverted to read this gentleman's simple confession.—I am, &c.,

TWIG.

To the Editor of the Chemical News.

SIR,—The correspondence which has for some weeks been raging on the above subject in the columns of your paper will, I trust, have a practical result in the adoption of the only course which can, in my opinion, prevent anything like improper admission to the Chemical Society.

It is much to be regretted that indiscriminate black-balling was ever resorted to; it can in the end do no real good, and will certainly prevent application for membership on the part of many men who would be an acquisition. It is an illogical and underhand way of attempting to purify the Society, and success on the part of the infallible clique who have thus set themselves up as the judges of candidates of whom they know, in most cases, absolutely nothing, and whom they reject simply because they are not professional chemists, probably means the disruption of the Society. At no time have we been looked upon as simply a body of professional chemists, and I most sincerely trust that no such time will ever come. As Mr. David Howard and other gentlemen have pointed out, much good work has been done by “the dilettanti in the true sense of the word,” and any attempt to exclude them and convert the Society into a trades union of chemists, cannot but prejudicially affect the advancement of chemical science, and the welfare of the Chemical Society.

No one doubts that some unfit men have gained admis-

sion for the sake of thrusting the F.C.S. before the eyes of the public in advertisements, &c., but if every Fellow would faithfully fulfil the promise he makes on entering the Society, to do all in his power to promote its interests and the good of the science, and would systematically refuse to give his name to a would-be candidate without knowing that he really was entitled to become a member, it is my firm conviction that this most unfortunate state of things would never have been brought about. Here it seems to me is the proper and legitimate means of keeping the Society as it should be, and any course such as that lately followed by the misguided few, will certainly cause the Society to afford a new illustration to the old saying concerning a house which is divided against itself.—I am, &c.,

G. W. R.

Liverpool, June, 20, 1876.

To the Editor of the Chemical News.

SIR,—It behoves all readers of your paper to have an appreciation of the reasoning by which the rejection of certain candidates for the Fellowship of the Chemical Society is defended. I notice, in your last issue, Mr. Howard asking whether such men as Dalton (mathematician), Herschel (organist), Murchison (fox-hunter), Thirlwall (clerk in holy orders), Grote (banker) are to be rejected. In reply, I may say I feel certain they would and ought to be rejected if nothing more were known of them than what is conveyed in these words; and also that these men would not have been so foolish as to put these things down as *qualifications* for the Chemical Society.—I am, &c.,

BEE.

To the Editor of the Chemical News.

SIR,—After the summing-up of Lord Chief Justice Maskelyne (begging his pardon for the liberty), it is difficult to see what more can be said upon the matter. His letter is an example of the come-and-let-us-reason-together method, that is eminently worthy of copying; conciliatory and gentlemanly in a high degree, but calm, clear, and practical.

But will Masters “Twig” and “Nuntius” take a word in a quiet way from a youngster like themselves? The Chemical Society is *not* a *close* Society, and is *not* an examining board, and was never made for such; and any effort to take an established and respected society of its dignity and position, and make it such by force, *after* they themselves have entered it, is very like the manner and custom of a bird that says a good deal and doesn't do much, called the cuckoo. If the nest doesn't suit them, it is possibly because it was not made for them—solely. So let the young Fellows take heart; there are plenty of “twigs” to build another nest—not, perhaps, so good as the old one, but as strictly exclusive as an oyster's home.—I am, &c.,

F.N.S.A.

June 19, 1876.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Bulletin de la Societe Chimique de Paris,
No. 10, May 20, 1876.

Composition of Native Peroxides of Manganese.—T. L. Phipson.—The determination of the peroxide alone (as ordinarily performed for commercial purposes) does not suffice to ascertain the value of this substance. Certain samples contain fluorine and phosphoric acid in

very considerable proportion, and also a small quantity of arsenic acid. A peroxide of manganese much used in England, if digested for twenty-four hours in very dilute nitric acid, yields to the solvent a large quantity of phosphate of lime, a little arsenic acid, traces of nickel and cobalt, much iron, and traces of indium. From this liquid sulphuretted hydrogen separates a little lead with arsenic acid and traces of copper. The filtrate mixed with excess of acetate of soda and sulphide of ammonium deposits much phosphate of iron with traces of indium, nickel, and cobalt, and the filtrate from this precipitate contains much phosphate of lime. Boiling hydrochloric acid leaves a reddish residue containing quartz and silicate of manganese (Rhodonite).

Fraudulent Colouration of Wines.—M. Arm. Gautier.—An exhaustive and voluminous paper of which we have here only the first portion. The fraudulent colouring matters taken in the order of their importance are magenta—arseniferous drugs being sometimes employed—cochineal, the Chinese mallow (*Althæa rosea*), elder-berries, and extract of indigo. The berries of *Phytolacca decandra* have been disused in the South of France on account of their violent and irritating purgative action.

Atomicity as a Principle of Classification.—M. Edme Bourgoïn.—Reserved for insertion in full.

Imidosulphonic Acid.—M. E. Berglund.—The author describes the free acid $\text{NH}_2\text{SO}_2\text{H}$, and its neutral ammoniacal, potassic, and barytic salts; its basic potassic, sodic, barytic, strontic, calcic, argentic, and plumbic salts, and a series of double compounds in which mercury is present.

Preparation of Hæmatinon.—M. R. Kayser (*Poly. Centralblatt*).—Hæmatinon is a flux of a fine red colour formerly employed in mosaic work. The following proportions give the best results:—

Pure quartz sand	60	parts
Oxide of copper	10	"
Ferrosulfuric acid	3	"
Calcined borax	10	"
„ soda	10	"

The mixture must be brought to a very high temperature, and then allowed to cool to dull redness, at which it is kept for some time. The red mass is generally covered with a green vitreous layer.

Use of Stassfurtite in the Preparation of Boracic Acid and of Borax.—M. G. Krause (*Berg. and Hüttenm. zeitung*).—Stassfurtite, which is a variety of boracite composed of $2\text{Mg}_3\text{B}_2\text{O}_5 + \text{MgCl}_2$, and containing 62.57 per cent of boracic acid before being delivered to the market, is washed so as to remove the foreign salts, the proportion of which may reach 50 per cent. The ground ore is introduced into leaden troughs and reduced to a paste with water, heated, and the solution of the residual foreign salts drawn off through an opening fitted with wire gauze. It is again slightly washed with water, and the remaining mineral is heated with 300 kilos. of water and 150 kilos. of hydrochloric acid at 1.16 to 105 kilos. of the mineral. The acid solution on decantation and cooling deposits crystalline boracic acid, which is pressed in cloths, washed with water, and dried.

New Process for the Manufacture of Dextrin.—M. Anthon (*Dingl. Pol. Journ.*)—At present fecula in the separate state is exclusively employed in the preparation of dextrin, all that portion being wasted which is retained by the fibrous part of the potato or of the bran. To prevent this loss the author recommends the use of the entire potato dried and ground after being freed from its soluble principles by washing in acidulated or alkaline water. The fecula thus obtained is sprinkled with hydrofluosilicic acid (0.5 to 1 per cent of the weight of the fecula); it is then placed in a drying-room at 38° to 44° till there is no further loss of weight; the temperature is then raised to 70° to 75° till no loss occurs, and finally to 90°. The desiccation being thus complete the fecula, whilst still

hot, is placed in large flat sheet-iron troughs which are introduced into large stoves heated to 100° to 125°. The transformation into dextrin is known to be complete when a sample taken out, on being cooled and moistened with cold water, adheres together in little glassy globules.

Characteristics of Eosine upon Stuffs.—M. R. Wagner (*Deutsche Industrie-zeitung*).—To distinguish eosin from saffranin and other colouring matters the most striking reaction is its inverse transformation by debromination into fluorescein. Under the influence of sodium amalgam fluorescein is very readily recognised by its splendid uranium-green fluorescence of an extreme sensibility. But this reaction, indicated by M. Baeyer, requires a skilful hand. The following procedure is easy and certain. Collodion is coloured by all the dyes derived from aniline, by Magdala red and alizarin; eosin, on the contrary, is immediately decolourised by collodion. To detect it upon a tissue a drop of collodion is sufficient; if the tissue is dyed with eosin or methyl-eosin a white spot is produced.

Resorcin Black.—M. R. Wagner.—If to an aqueous solution of resorcin there is added sulphate of copper and then ammonia, enough to re-dissolve the precipitate first formed, a deep black liquid is obtained which dyes wool and silk black and which may possibly be used as ink.

Medicinische Jahrbücher Herausgegeben von der K.K. Society of Physicians, Jahrgang 1876, Heft ii.

This serial appears to be mainly devoted to anatomical and surgical subjects. There is, however, a paper by Dr. M. Abeles, of Carlsbad, on the "Saccharific Ferments in the Animal Organism." From a number of carefully conducted experiments the author concludes, in opposition to Dr. Pavy, that "A solution of glycogen injected into the circulation produces no greater secretion of sugar than does pure water. The subcutaneous injection of glycogen also occasions no excretion of sugar. Hence the blood has no saccharific power."

Reimann's Farber Zeitung, No. 17, 1876.

Aniline Colours in the Spectroscope.—By Dr. M. Reimann.—I have examined a series of aniline colours with the spectroscope in the hope of utilising this valuable instrument both for the determination of sophistications and for distinguishing shades. After some attempts I decided in favour of an instrument in which the image of a photographed scale is thrown upon that of the spectrum, which renders it possible to read off the darkening of the spectrum in degrees. The two coincident images were examined as usual through a telescope. In order to render it possible for anyone to repeat the following experiments I have determined some of the best known metallic lines in their position upon my spectral scale, which reaches from 1.0 to 15.0. Of the metallic lines examined the following took up the spaces given:—

Yellow sodium line	5.5	to	5.9
Red potassium line	4.9		5.0
No. 1, lithium line	3.9		4.2
No. 2, " "	5.5		5.8
Green thallium line	7.2		7.5

The position of the spectral colours of the scale was as follows:—

The spectrum appeared black	}						1.0	to	3.5
from			
Red occupied	3.5		4.5
Orange	4.5		5.0
Yellow	5.0		5.8
Green	5.8		8.6
Blue	8.6		10.5
Indigo	10.5		12.6
Violet	12.5		15.0

The solutions of colouring matters under examination were diluted to 1 part in 10,000. In order to facilitate the repetition and control of the experiments as regards shades of colour, I have given, where it appeared needful, the position which the colour in question occupies in the first of Chevreul's circles (*couleurs franches*). The width of the slit of the apparatus was arranged so that the darkening of the spectrum was just distinct, which, with a little practice, is easily measured. It may be remarked that when coloured solutions are introduced that part of the spectrum appears dark—in most cases black—which shows the complementary colour of the dye under examination. Which this is may be ascertained in the well-known manner from the first circle of Chevreul's system. The three colouring matters first examined were magenta, eosin, and safranin:—

Magenta; spectrum black 1 to 3.5, darkened 6.0 to 9.0.

Eosin; black 1 to 3.8, darkened 7.2 to 9.0.

Safranin; black 1 to 3.1, darkened 6.0 to 8.0.

A mixture of equal parts magenta and eosin showed the spectrum black 1 to 3.1 and darkened 7.3 to 8.5. A mixture of equal parts eosin and safranin gave a black spectrum 1 to 3.2, darkened 6.8 to 8.3. A mixture of equal parts eosin and safranin black 1 to 3.2 and darkened 6.5 to 8.0. The following colours were also determined:—

Colour.		Chevreul's name.	Black.	Darkening.
Nicholson blue, 4 B ..	B 2	—	—	1.0 to 4.7
" " 5 B ..	B 4	—	—	1.0 4.7
Methyl violet, 3 B ..	B V 1	1.0 to 3.6	4.5	5.1
" " 2 B ..	B V 3	1.0 3.5	4.7	5.4
" " B ..	V	1.0 3.9	5.0	5.5
" " R ..	V 2	1.0 4.7	5.7	6.0
" " 2 R ..	V 3	1.0 4.4	5.3	5.9
" " 3 R ..	V 5	1.0 4.0	5.8	6.0
Scarlet ruby, II. O O R	R O	1.0 3.3	5.6	6.7
Ruby, II. O R yellowish	R 4	1.0 3.9	5.9	7.0
Ponceau ruby, II. R R O	V R 3	1.0 3.6	6.2	7.0
Methyl green	B Ve 2	—	1.0	5.3
Iodine green	B Ve 4	—	1.0	5.9

No. 18, 1876.

In a leading article on the necessity of technological schools (*Fachschulen*) taken from the *Volks Zeitung* the remark is made that Germany is in the industrial arts far behind other nations, and does little but imitate. As a remedy Dr. Reimann suggests improved patent-laws and a new tariff. (It is very remarkable that at the very time when an attempt is being made to destroy our system of patent laws it should be recommended for introduction into Germany.) Coloured yarns, especially those dyed with magenta and other coal-tar colours, are often found partially discoloured or even bleached. This is due to their being wrapped in white paper containing an excess of chlorine. It would be preferable to wrap them in coloured papers.

Justus Liebig's Annalen der Chemie,
Band 181, Heft 1.

Dinitro Compounds of the Fatty Series.—Dr. Edm. ter Meer.—A lengthy essay; unfit for abstraction.

Certain Bromo-sulpho-benzolic Acids.—Dr. W. Lenz.—A long paper, presenting no points of particular interest, and abounding in hypothetical formulæ.

Certain Compounds of Hydro-sulpho-cyanic Acid with the more important Cinchona Alkaloids.—O. Hesse.—The compounds described are those of quinine, cinchonidin, conchinin, and cinchonin.

Phenol Compounds.—O. Hesse.—The author points out that whilst phenol is an excellent test for ascertaining the quality of the cinchona alkaloids the latter may inversely serve as reagents for phenol. Certain salts of these alkaloids may find a similar application to that of certain bisulphites in the recognition and separation of aldehyds, and acetons.

Aricin and Kindred Substances.—O. Hesse.—Aricin, cinchovatin, and de Vriz's lævo-rotatory alkaloid when purified are identical with cinchonidin. Cusconin is the sulphate of cinchonidin.

Re-conversion of Triacetamin into Diacetamin and a Fifth Aceton Base.—W. Heintz.—Not adapted for abstraction.

Nitrogen and Albumen Present in the Milk of Women and Cows.—Dr. Leo Liebermann.—The author concludes that both in Brunner's and in Hoppe-Seyler's method a considerable part of the albuminoids escapes precipitation: Haidler's method, on the other hand, gives the total amount of the lacteal albuminoids, which may also be entirely thrown down by means of tannin. In addition to casein and albumen a third and distinct albuminoid body is present, but there is no nitrogenous body found in milk except such as belong to the albuminoid class.

Determination of Nitrogen in Albuminates.—Dr. L. Liebermann.—The author agrees with Seegen and Nowak that where an exact determination of nitrogen is required the method of Dumas must be adopted to the exclusion of that of Will and Varrentrapp.

Researches on the Biliary Pigments.—Dr. R. Maly. (Fifth part.)—Incapable of abstraction.

Remarkable Transformation of Normal Butyric Acid into Isobutyric Acid.—If a solution of normal butyrate of lime in water is subjected to the prolonged action of heat in a sealed tube it is converted into the corresponding isobutyrate.

Band 181, Heft 2.

Contributions to the Theory of Luminous Flames.—Dr. Karl Heumann.—The first portion of an extensive treatise. The author examines critically the various existing theories, and maintains that the views of Davy require to be enlarged and developed rather than rejected.

Milky Juice of Plumiera Acutifolia, and on Plumieric Acid.—A. C. Oudemans.—Not capable of useful abstraction.

Determination of the Atomic Weights of Cæsium and Rubidium.—R. Godeffroy.—The atomic weight found for cæsium is 132.557, or, in round numbers, 132.6. Bunsen, Johnson, Allen, and Mercer give in round numbers 133. The atomic weight of rubidium is 85.476, or, in round numbers, 85.5. The author describes in detail the methods adopted for obtaining the two bodies in a state of purity.

Preparation of Platinum-Black by means of Glycerin.—Milau R. Zdrawkowitch.—The author takes 15 c.c. glycerin at 25° to 27° B. and 10 c.c. potash lye at 1.08 specific gravity. The mixture is heated to a boil, and then 3 to 5 c.c. of solution of chloride of platinum are added drop by drop of the strength commonly used as a reagent.

Communications from the Laboratory of the University of Greifswald.—These communications consist of a paper on the action of bromine upon ortho-amido-sulpho-benzolic acid, by H. Limpricht; one on ortho-bromo-sulpho-benzolic acid, by A. Bahlmann; and one on met-amido-sulpho-benzolic acid and bromo-sulpho-benzolic acid, by H. Beckurts.

Observations on Zirconia.—R. Hornberger.—The analogy of zirconium with silicium to be traced in many inorganic compounds does not extend to organic bodies. Oxide of zirconia cannot combine with alcohol radicals so as to form compound ethers in which it plays the part of an acid, as does silica in the silicic ethers. In contradistinction to silicium zirconium is electro-positive with organic groups of atoms.

A Criticism on the Researches of R. Maly on the Biliary Pigments.—J. L. W. Thudichum.—A controversial paper.

Action of Zinc Ethyl upon Acetaldehyd.—G. Wagner.—Not adapted for abstraction.

Moniteur Scientifique, du Dr. Quesneville,
June, 1876.

Tempered Glass.—MM. Perry-Nursey, de Luynes, Tell, &c.—A long account of the various processes used for toughening glass. M. de la Bastie has proved that contrary to the opinion generally received tempering gives to glass a solidity far superior to that of annealed glass.

Ferments and Fermentations.—M. Ch. Blondeau. (Continuation.)—This treatise does not admit of useful abstraction.

Essence of Roses.—An account of the production of this perfume at Kezanlik in the Balkan chain. 16 kilos. or about 130,000 roses are required to produce 30 grms. of the essential oil. The yearly yield of the district is estimated at 2000 kilos. of oil, worth on an average 1000 francs. per kilo.

History of the Turkey Red Manufacture.—M. T. Chateau.—Unfit for abstraction.

Preparation of Pure Nickel from the Nickel of Commerce.—M. A. Terreil.—The author dissolves in 7 parts of aqua regia, evaporates almost to dryness, re-dissolves in water, separating the insoluble matter by filtration, precipitates the copper with metallic iron, peroxidises the iron, transforms the metals into sulphates, and lastly, precipitates the iron with carbonate of baryta and crystallises the sulphate of nickel.

Citric and Tartaric Acids.—Taken from the *Journal of the Society of Arts*.

Industrial Exhibition of Mulhouse.—An account of the opening ceremonials, festivities, speeches, &c.

Les Mondes, Revue Hebdomadaire des Sciences,
No. 1, May 4, 1876.

Temperature of the Interior of the Earth.—From observations made on the Well of Sperenberg, near Berlin, M. Mohr concludes that at the depth of 5170 feet the increment of heat must be nil. A similar decrease of the increment of heat has been observed in the Artesian Well of Grenelle. Hence M. Mohr draws conclusions unfavourable to the Plutonian theory.

Nos. 2, 3, and 4, May 11, 18, and 25, 1876.

These issues contain no chemical matter.

No. 5, June 1, 1876.

Putrefaction Produced by Bacteria in Presence of Alkaline Nitrates.—M. Meusel.—The presence of nitrites in common water is due to bacteria when it contains nitrates, and organic bodies such as sugar, starch, cellulose, &c. Bacteria are the agents of the transmission of oxygen even when in chemical combination. It is probably by reason of the consumption of oxygen which they effect that these animalculæ are so dangerous to man. Nitrates are useful as manure, not merely in virtue of the nitrogen which they contain, but also by their oxygen, by means of which the bacteria destroy the cellulose.

The Radiometer of Mr. Crookes is already employed in photography to judge of the time necessary for the exposure of an object.

The manufacture of sea-weed charcoal is carried on on a large scale at Noirmoutiers, where more than 200 furnaces are constantly at work. The value of the charcoal is from 80 centimes to 1 franc per hectolitre. 100,000 kilos. of fresh weed give 20,000 kilos. of dry material, or 5000 kilos. of charcoal, which, when incinerated, yield from 3500 to 4000 kilos. of saline matter. Weeds which abound in potash, such as the *Laminaria*, contain more iodine than bromine. In *Fucus nodosus*, *vesiculosus*, *fruticosus*, &c., soda predominates, and bromine is much more abundant than iodine.

No. 6, June 8, 1876.

The Industrial Society of Mulhouse has celebrated its fiftieth anniversary. Delegates were present from the Academy of Sciences, from the Conservatory of Arts and Manufactures at Paris, the Industrial Societies of Rouen, Reims, Amiens, &c.

MEETINGS FOR THE WEEK.

SATURDAY, 24th.—Physical, 3. The following Apparatus from the Loan Collection will be exhibited and explained—Electric and Magnetic Apparatus by Prof. Petrochovsky, Mach's Apparatus for Lenses, Kerr's Apparatus for showing Effect of Tension on Polarised Light, &c.

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PHYSICAL SOCIETY.

The Last Meeting of the Session will be held on Saturday, June 24, at 3 p.m., in the Physical Laboratory, Science Schools, South Kensington.

By Special Permission of the Lords of the Committee of Council on Education, Certain Instruments from the Loan Collection of Scientific Apparatus will be Exhibited and Explained.

Visitors will be admitted on giving their names at the entrance.

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THE CHEMICAL NEWS.

VOL. XXXIII. No. 866.

THE CHEMICAL SOCIETY.

HISTORY is said to repeat itself. The correspondence which has been going on for the last few weeks in our columns shows that a somewhat widespread dissatisfaction exists in regard to the election of Fellows of the Chemical Society, the malcontents using the ballot-box to force matters to a crisis.

It will be remembered by many of our readers that nine years ago* we had to draw attention to a parallel state of affairs in the Chemical Society. Then, as now, certain members felt that the Fellowship of the Society was somewhat too easily acquired; then, as now, these dissentients thought it desirable that something should be done to restore to the initials F.C.S. the honourable prestige which they feared was departing from them; and then, as now, an anonymous minority of the Fellows adopted the expedient of indiscriminate blackballing in order to coerce the Council of the Society to follow a particular line of action.

Considering how exactly parallel the two cases are, it has been thought advisable to reprint the following report which was circulated among the Fellows of the Society in November, 1867: and it is hoped that the fair and impartial summing-up of the various arguments for and against may tend to allay the anxiety which some of the younger Fellows of the Society appear to feel that their interests and scientific status are less thought of now than they were a few years ago.

"CHEMICAL SOCIETY, BURLINGTON HOUSE,
"PICCADILLY, W.

"November 11, 1867.

"SIR,

"At a meeting of the Council, held on May 16th, 1867, it was resolved, 'That a Committee of five be appointed to consider the by-laws relating to the election of Fellows, Honorary Members, and Associates, and to report to the Council.' It was further resolved, 'That the Committee consist of Mr. Crookes, Dr. Miller, Dr. Odling, Mr. Wanklyn, and Dr. Williamson.'

"Upon the presentation of the Committee's report at a meeting of the Council, held on November 7th, it was resolved, 'That this report be approved, and that a copy of it be sent to each Fellow of the Society.'

"We beg to append the report in question, and have the honour to remain,

"Your obedient Servants,

"W. ODLING,

"A. VERNON HARCOURT.

"Hon. Secretaries.

"Your Committee were appointed by a resolution, passed at a Meeting of Council, held on May 16th, 1867, in fulfilment of the intention which the Council announced to the Society in its anniversary report.

"As bearing upon the standard of qualification for admission to the Fellowship of the Chemical Society, your Committee, from replies they have received to a circular which they addressed to all the Fellows, and from conversations they have held with different Fellows whom they chanced to encounter, have ascertained the existence among the Fellows of the Society of two very distinct views as to its nature and purposes.

"Many Fellows appear to regard the Society as being by rights an association of eminent scientific men; and they accordingly look upon the Fellowship of the Society as a distinction which should be conferred only upon those who have given evidence of marked chemical proficiency, as, for example, by the production of some original memoir; so that the election of anyone as a Fellow of the Society should stamp him at once as being a well-trained chemist and competent investigator.

"In favour of this view it is urged that the initials F.C.S., appended to the name of any gentleman, seem to imply that his attainments have won for him a public recognition somewhat in the character of a degree; and that these initials ought to signify, in reality, that which they seem to imply, and which is indeed their proper signification.

"It is further urged that the Fellowship of the Chemical Society is essentially an honorary distinction, although from the ease with which it can be obtained, practically by any who choose, it is a distinction but little valued by the better sort. It is, however, eagerly sought after and obtained by men who are not perhaps altogether desirable—who certainly have no claim to the title of scientific chemists—and who, in some cases, do not even join the Society from any interest they take in chemical science, but solely with the view of parading a distinction to which their merits do not really entitle them.

"Moreover, from the circumstance that chemistry is pursued, not only as a science but also as a profession and trade, the right to append the initials F.C.S. possesses a sort of trade value, exceeding its cost, to mere trading or professional chemists; as suggesting that those who have the privilege of using these initials are better qualified men than their brethren who are not thus distinguished.

"From these causes, it is said, the Fellowship of the Chemical Society has gradually sunk in public estimation; and accordingly it is very desirable that something should now be done to restore, if possible, its original prestige.

"On the other hand, many Fellows are of opinion that the Society is merely an association of individuals, having joint but various interests in the progress of both pure and applied chemistry; that the object for which the Society exists is not to confer honour upon any individual whatever, but to promote the general advancement, distribution, and application of chemical knowledge; and that, as a general rule, men engaged in pursuits more or less dependent on or connected with chemistry, and taking a sufficient interest in chemistry to wish to join the Society should, unless personally objectionable, have every facility afforded them for joining it.

"In favour of this view, the preamble to the charter is adduced, and especially the following paragraph; whereas certain of our subjects 'did establish and are now members of a society known by the name of the Chemical Society, for the general advancement of chemical science, as intimately connected with the prosperity of the manufactures of the United Kingdom and for a more extended and economical application of the industrial resources and sanatory condition of the community,' &c.

"It is further maintained that the Society, from its origin until the present time, has always been of a mixed rather than of an exclusively scientific character—that the present Fellows form quite as distinguished a body as have ever constituted the Society—and that many, at any rate, of the most distinguished individual Fellows do not feel themselves at all discredited by being associated as joint Fellows of the Society with men who are engaged or interested in chemical pursuits, but whose scientific or social position is inferior to their own.

"Moreover, of scientific as distinguished from purely professional societies, the Royal Society, it is urged, is the only one of which the Fellowship is conferred in recognition of eminent scientific merit—the special science societies being practically open to all students of and workers at their respective subjects, who may wish to be elected to their respective Fellowships. To limit the

* Vide CHEMICAL NEWS, vol. xv., pp. 229, 243.

Chemical Society then to eminent scientific chemists would be tantamount to making it the chemical section of the Royal Society, instead of allowing it to have a distinct function and character of its own.

"It is further urged that the circumstance of chemistry being to some extent a profession, so far from indicating the propriety of making the Fellowship of the Chemical Society an honorary distinction, rather contra-indicates it. For, independently of the difficulty, or rather impossibility, of withholding or conferring the honour without doing much injustice to individuals, the Society, by professing to choose out the most worthy, would naturally be held responsible for its choice, and identified more or less with the acts of each and all of its Fellows.

"Your Committee having given these different views their best consideration, are not prepared to recommend any alteration in the bye-law relating to the election of Fellows, which would have the effect of confining the Fellowship of the Society to strictly scientific men.

"But they think it may be advisable, although they have failed to elicit evidence of the admission of any significant proportion of unsuitable persons into the Society, to make some modification in the present bye-law, with a view to increase the security against the accidental election of undesirable candidates.

"They accordingly suggest that in future, or after a certain interval of time, the form of recommendation of a candidate, referred to in the first paragraph of the bye-law in question, shall be required to be signed by five instead of by only three Fellows of the Society, of whom three at least instead of only one shall be required to sign from personal knowledge; and further, that in the second line of the printed form of recommendation, the words 'Qualification or Occupation' shall be substituted for the words 'Position, Profession, or Occupation.'

"At present your Committee are not disposed to advise any alteration in the second paragraph of the bye-law, which requires three-fourths of the votes given to be in favour of the candidate, in order to effect his election. If, however, contrary to the anticipations of the Committee, any section of the Fellows should be found to make an improper use of this requirement, your Committee would then recommend that one or other of two courses should be proposed by the Council and adopted by the Society; that is to say, that the bye-law should be so altered as to render valid the election by a mere majority, or else that the bye-law should be temporarily abrogated, and during its abrogation the election of Fellows be delegated by the Society at large to a Committee appointed for the purpose.

THE NEW BILL FOR THE PREVENTION OF THE POLLUTION OF RIVERS.

THE Ministry has at last fulfilled its promise, and this eagerly awaited measure has been actually introduced. In what shape it will become law, if at all, lies beyond the scope even of an editor's prescience. But we may safely say that it deserves to pass; not that it fulfils all the reasonable expectations of the friends of sanitary reform, but it attempts quite as much as in face of the powerful interests to be encountered there is any probability of being accomplished. We accept it, therefore, in the spirit of the homely old adage which tells us that "half a loaf is better than no bread."

The first part of the Bill is directed against the very common practice of using the streams as carriers not merely for liquid but for solid refuse. This is an evil about which there can be no dispute, since the rubbish in question, even if it has no tendency to pollute the waters, interferes with their due flow, and renders floods both more frequent and more destructive. It may, perhaps, be doubted in how far the wording of Section 3 will apply to

the trick of depositing cinders, tank-waste, refuse from mines, &c., not exactly in a river but close to its margin, so that the next fresh may carry it in. The language employed is that:—"Every person who puts or causes to be put or to fall, or knowingly permits to be put or to fall, or to be carried into any stream the solid refuse, &c." How these words will be construed by the Courts it would be very rash to predict. But we cannot help thinking that it would have been wiser to enact that no refuse of the kinds referred to should be laid down within twenty yards of the margin of any river.

The provisions relating to the discharge of sewage into streams are free from all unnecessary complications. The sanitary authorities are very fairly required to use "the best practical and available means to render harmless the sewage matter so falling or flowing." What such means may be is not specified, but it is left to the local boards, town councils, &c., to select from amongst the known processes whatever is best adapted to their individual case. We are also glad to find that there is no hard and fast standard laid down as to what may and what may not be discharged into any river. We have so often insisted on the futility of any arbitrary standard as applied to the polluting influents, that we must be pardoned for rejoicing over Mr. Sclater-Booth's measure as a triumph of common sense over official pedantry.

For the liquid refuse of manufacturing establishments the provisions are similar. The perplexing distinctions drawn in the Bill, as introduced last year between nuisance of old and of comparatively recent standing, no longer find a place. No poisonous ingredient likely to occur among manufacturing or mining refuse, such as arsenic, chrome, lead, &c., is made the subject of any especial prohibition, which may be regarded as great, if not too great, leniency. But the local authorities if receiving dangerous or poisonous materials into their sewers are to be empowered to require compensation for the additional outlay required in their purification. We cannot see any other possible solution of the difficulty. The law cannot fairly require every dyer, fuller, tanner, &c., to purify his own waste waters since many manufacturing establishments are built in the heart of towns, where there is no room for precipitation-tanks, filter-beds, or any of the various appliances required. We trust, therefore, that manufacturers will not oppose this clause, and that they will gradually see the way to render their liquid refuse both less plentiful and less offensive than at present. We dare not expect that in every case their success will be as signal as that of the alkali manufacturers in the condensation of their hydrochloric acid gas, and we cannot forget that even this boasted condensation, when carried to the perfection now deemed requisite, is found not a source of profit but a decided burden. It is stipulated that the local authorities shall not be bound to receive into the sewers any matter which might render the sewage or its deposit unfit for application to the land.

The two "saving clauses"—so-called, perhaps, in irony—are most objectionable. The one declares that "the powers given by this Act shall be deemed to be in addition to and not in derogation of any other powers conferred by Act of Parliament, law, or custom." This is most unfortunate. It is not fair that a quarrelsome or fanciful neighbour shall have in his reach a whole armoury of offensive weapons to use against either manufacturers or local authorities. Every one should know exactly his position with respect to the pollution of rivers. For this purpose there should be but one way of proceeding against offenders. So long as the multiplicity of remedies exists no one can tell when he is safe. The second "saving clause" is, if possible, still more amiss. By it the doings of the Metropolitan Board of Works are formally excluded from the operations of the Act. It is a common saying the greater the offender the more lenient is his treatment, and here we find it fully exemplified. Perhaps, however, had the said Board of Works been placed upon the same footing as the Town Councils of Leeds, Birmingham, or

Manchester, the representatives of the Metropolitan boroughs would have combined in opposition.

These defects apart we consider the measure useful and practicable and wish it a safe passage through the Houses.

NOTE ON PICTROTOXIN.

By RICHARD APJOHN, F.C.S.,

Prælector of Chemistry Gonville and Caius College, Cambridge.

SOME time since I prepared a large quantity of picrotoxin from the seeds of the *Cocculus indicus* with a view of studying more closely the properties of this remarkable substance. After making a number of experiments on this body I have no hesitation in saying that the determination of its melting-point furnishes an easy and certain criterion of its identity.

The method I have employed for determining its melting-point claims no novelty. About half a milligram of the substance was introduced into a capillary tube sealed at one end. The tube was then attached to a thermometer by means of a few turns of fine platinum wire, and the whole was gradually heated in a beaker of oil of vitriol. In four experiments the picrotoxin was observed to melt at 192°C . A specimen of picrotoxin which I obtained from Messrs. Hopkin and Williams exhibited exactly the same melting-point.

"As far as I know, the melting-point of picrotoxin has never been used as a character for its detection, and in "Watts's Dictionary of Chemistry" (vol. iv., page 643), it is asserted that picrotoxin decomposes without fusion when strongly heated. In order to test the truth of this latter statement about a grain of picrotoxin was introduced into a test-tube, and heated in an oil of vitriol bath to a temperature of 192°C ., when it melted to an amber liquid. The contents of the tube when cold were dissolved in warm water, and set aside to cool. The well known stellate tufts of prismatic crystals soon appeared, and these crystals, when dried, were found to possess the normal melting-point (192°C .) of picrotoxin. From these experiments it is, I think, sufficiently demonstrated that picrotoxin has a definite melting-point, and that it does not undergo decomposition at the heat at which it melts.

I may also state that crystals of picrotoxin deposited from an aqueous solution contain no water of crystallisation, for having heated them to a temperature slightly below their fusing-point I found that they had lost no weight.

PROCEEDINGS OF SOCIETIES.

PHYSICAL SOCIETY.

June 24th, 1876.

Professor G. C. FOSTER, F.R.S., President, in the Chair.

THE following candidates were elected members of the Society:—Prof. James Dewar, M.A., F.R.S.E., and the Hon. F. A. Rollo Russell.

Prof. GUTHRIE showed the action of Prof. Mach's apparatus for exhibiting to an audience the effect of lenses on a beam of light passed through them. It consists of a long rectangular box with glass sides, in which are several movable lenses. A parallel beam of light falls on a grating at one end of this box, and is thus split up into a number of small beams, which are rendered visible by filling the box with smoke. After passing through the first lens the rays fall on a movable white rod, which may be placed to indicate the focus. The light then falls on another lens partly covered with red and partly with blue

glass, in order to more precisely exhibit the paths of the rays.

Baron WRANGELL exhibited the apparatus employed by Petrochovsky in his magnetic experiments. These experiments had reference to (1) normal magnetisation; (2) the measurement of the distance of the poles of a magnet from its ends; and (3) a thermo-electric apparatus. The determinations were very much simplified by employing a unipolar magnetic needle formed by bending a small bar magnet at right angles at about a quarter of its length from one end. The needle is then suspended by a fibre attached to the end of the short arm, and the longer arm is maintained horizontal by a brass counterpoise weight. It will be evident that as one pole is in the axis of rotation it cannot have any effect on the motion of the needle. By turning up each end in this manner the moment of the magnet may be ascertained without knowing the exact positions of the poles. If a magnetic needle be so placed that a bar magnet parallel to it has no effect in deflecting it from the meridian, and the bar be then struck with a brass hammer, the state of equilibrium will be disturbed, as is shown by the motion of the needle. This, however, is not the case with a piece of soft iron round which an electric current is passing. The apparatus employed in the experiments on "normal magnetisation" consisted of an arrangement for passing a current round rods of soft iron of varying lengths, so constructed that any number of the surrounding coils can be removed in the manner of an ordinary rheostat. After the current has been passed round the bar it is moved until its residual magnetism has no effect in deflecting a delicate unipolar needle from the meridian. The current is then passed round it, and the coils are adjusted until the magnetised bar has still no effect on the needle. The effect of the coils themselves is eliminated by means of a subsidiary coil. When the current is thus adjusted, the bar is said to be "normally" magnetised, and M. Petrochovsky has ascertained that this condition is satisfied when the length of the coil is 0.8 times that of the bar, and this is independent of the strength of current. This, then, is the only case in which the position of the poles is the same as when the bar is charged with residual magnetism. For the determination of the positions of the poles of a bar magnet a somewhat complicated apparatus was employed. A large unipolar magnet, about 8 inches in length, provided with a bifilar suspension, was enclosed in a glass box. A fine silver wire was stretched parallel to the axis of the needle between two projections on it, and it also carried a fine index at the horizontal end. The wire is focussed in a telescope which can be made to travel along rails parallel to the magnet, and the index at the end can be observed by another telescope. A small magnet at right angles to the large magnet can be moved with the first telescope, and the point at which its effect in deflecting the unipolar is the greatest is ascertained by varying its position parallel to itself along a graduated scale, and then observing the space through which a subsidiary magnet must be moved in order to restore the unipolar to its initial position, as observed in the second telescope. When this point is reached it must be exactly opposite the pole of the large magnet. It was thus found that the poles are at a distance of one-tenth of the length of the magnet from its ends. To determine the position of the poles of a horseshoe magnet a delicate magnetic needle is placed below a fine wire in the meridian, and a horseshoe magnet is brought so that its two ends are immediately below the wire and near the needle. In the case of an electro-magnet the point at which its effect is greatest is found to vary when the coils are moved towards the ends, and is nearest to the ends when the coils project slightly beyond them. The third series of researches referred to was on the influence of an electric current on the thermo-electric action of soft iron. A number of strips of iron are connected by means of copper studs, and when currents are passed round the alternate strips it is found that the system acts as an ordinary thermo-pile. This question is,

however, still under investigation. In reply to a question of the President, BARON WRANGELL stated that the effect of increasing the number of coils in the horseshoe magnet on the position of the poles is also still under investigation.

Prof. BARRETT then made a brief communication on the magnetisation of cobalt and nickel. He has recently made some experiments on these metals with a view to ascertain whether they undergo any elongation or contraction similar to that experienced by iron during magnetisation. From his first experiments he concluded that cobalt elongates slightly, but that there is no effect on nickel, but this latter result may have been due to the fact that the metal was not absolutely pure. He has, however, obtained through Mr. Gore a fine bar of pure nickel about two feet in length, and now finds that it contracts, and that the amount of this contraction is about the same as the expansion of a like iron bar when similarly treated.

Prof. GUTHRIE then described some experiments on the freezing of aqueous solutions of colloid substances, which he has been studying in connection with his recent investigations on Cryohydrates, &c. If a solution of sugar be gradually cooled the temperature at which ice separates out is always below 0°C ., and the extent below increases with the amount of sugar in solution. But he finds that in a solution of gum, having exactly the same chemical formula, the ice always separates at 0°C . whatever be the amount of gum present. Thus, while every crystalline substance forms a freezing-mixture when mixed with ice or snow, colloids are incapable of doing so. The gum and the water do not recognise each other, and similar results were obtained in the case of gelatin and albumen. These facts are strictly in accordance with the results of Prof. Graham's classical researches. It almost follows that when heated similar effects are observed, and Prof. Guthrie has found that solutions of gum in varying proportions always boil at 100°C .

Mr. W. CHANDLER ROBERTS said that this important discovery was one that his late distinguished master would have welcomed, and he expressed a hope that Dr. Guthrie would continue his experiments with the series of colloids actually prepared by Graham.

Prof. GUTHRIE then showed the experiment by which Dr. Kerr has recently proved that glass, resin, and certain other substances exhibit a depolarising effect when under the influence of a powerful electrical tension. With the help of Mr. Lodge, Dr. Guthrie has succeeded in repeating these exceedingly delicate observations, but the effect is very slight, and ill suited for the lecture-room. A beam of polarised light traverses a thick plate of glass in which two holes have been drilled, nearly meeting in the centre, and two wires are fixed in these, and connected with the terminals of a powerful coil. The light after passing through the analyser falls on the screen. If now the analyser be so turned that the illumination is least before the current is turned on, the brightness of the field will be seen to increase as soon as the circuit is closed, and this brightness will gradually increase up to a certain limit. The effect is greatest when the light is polarised at an angle of 45° to the line joining the terminals.

The PRESIDENT then adjourned the meetings of the Society until November.

NEWCASTLE-UPON-TYNE CHEMICAL SOCIETY.

General Meeting, March 23rd, 1876.

JOHN PATTINSON, President, in the Chair.

(Continued from p. 258.)

Mr. R. C. CLAPHAM read the following paper:—"On a New Decomposing Furnace Patented by Messrs. Jones and Walsh." In the first place I must apologise to the members for bringing before them a paper which would have been better placed in the hands of those more conversant with the new decomposing furnace, but my excuse must be that it was those very gentlemen who could have

done it so well who first pressed the matter on my attention.

The best methods of decomposing salt with sulphuric acid have long attracted the attention of manufacturers, for it is admitted on all sides that those adopted hitherto in practice have been unsatisfactory, leading to expenses from partial stoppage of the work, breakage of pans, and loss of sulphuric acid.

When the present system of making soda was commenced on the Tyne in 1820 (at that time called the French system), the chemical trade was very limited, and small lead pans lined with brick work were used in decomposing salt. A charge of 2 cwts. of salt was taken, the sulphuric acid was slowly poured upon it from a carboy, through a hole in the top of the furnace, and it took three hours to complete the operation. But as these lead pans were so liable to be injured, a fire-brick furnace was substituted in 1828, which was looked upon then as an improvement and was well known locally as the "Dandy" furnace. At that time no attempt was made at the condensation of the hydrochloric acid fumes, and they passed directly into the atmosphere. A great improvement upon the above was introduced in 1840, by the late Mr. John Lee, who applied a metal pan about the same size as we now use, and its adoption by the trade has led, amongst other advantages, to a large saving in sulphuric acid. This pan has, however, been always liable to frequent breakage from negligence of workmen and other causes, and to get over this difficulty various substitutes have at times been tried. In 1860 I had some fire-clay pans made at the Scotswood Brick Works, which were used at the Walker Alkali Works. They were found to work pretty well, but were given up from the difficulty of getting them properly heated.

If we allow that the life of an ordinary decomposing pan is long enough to turn out 2000 tons of salt (in a few cases it may be more, but many of those present will know that in some cases 1000 tons is a fair quantity), I calculate that on the Tyne alone not less than £6000 per annum is spent in renewal of decomposing pans; and if we take into account the loss of time caused by the breakages, the extra labour required, and the waste of acid, the money loss under these heads will be very considerable. In the face of these facts there would then appear to be ample room for improvement. Messrs. William Jones and John Walsh, of Middlesbro', have contrived and erected a new class of decomposing furnace, which completes the whole charge of salt in one furnace. This furnace has now been at work several months, and from an examination of the plates forming the dish of a furnace which was laid off a few days ago, I found the plates to be as fresh and good as when erected.

The furnace, which is now in actual work, and from which regular results have been obtained, consists of a metal dish divided into six segments, all tightly fixed together. The metal is $2\frac{1}{2}$ inches thick, and the whole rests on solid brick work. Its inside diameter is 15 feet, and the charge of salt is about 12 tons each twenty-four hours, so that a furnace of this capacity (allowance being made for draughts and local matters) will turn out about 80 tons of sulphate in six days. The batch is kept in constant motion by means of two metal arms, worked from a centre shaft, to which are fixed paddles and rakes, and the whole mass is much more completely mixed than in a hand-worked furnace, and the sulphate produced is of a fine and uniform character. Mr. Walsh, who is present, has kindly undertaken to explain the furnace more fully afterwards, from the working model on the table and from the plans exhibited. The motion given to the crown wheel is got from a donkey engine, and an engine with a 6-inch cylinder is found to be sufficient to work the furnace. At present the batch is both charged into the furnace and discharged by hand, but Mr. Goodman expects to perfect a plan to do this by machinery.

The advantages to be derived from this new furnace may be described as a more regular daily plan of working;

freedom from sudden stoppages by the breakage of the present pans; a direct saving in wages, as only one workman is required on each shift for charging and working the furnace; a saving of fuel, as the heat required is less than in the old furnace, and 3 cwts. of coke per ton of sulphates is used in the place of 9 cwts. of coals ($=5\frac{1}{2}$ cwts. coke) by the old furnace; a saving also of sulphuric acid calculated by Mr. Walsh to be equal to 2 per cent. Then again, owing to the complete mixing and better working of the batch by machinery, a great inconvenience felt by the public living in the neighbourhood of chemical works, by the batches being sometimes drawn by the workmen in a partly finished state, and containing free hydrochloric acid, is entirely avoided. The fumes are emitted in a gradual and uniform manner throughout the working of the batch, and the condensation is, therefore, more perfect; and as the whole of the gas is passed through one coke tower, it is condensed into hydrochloric acid of 26° to 28° T.

It therefore appears that both in an economical and sanitary point of view, this furnace is an improvement on its predecessors. In the description I have given I have purposely confined myself to a furnace in actual daily work, and from which known results have been obtained; but it is not unreasonable to expect that experience may show that a much larger weekly turn out may be possible. There are several members present who are interested in perfecting the successful working of this furnace, amongst whom I may mention my friend, Mr. Jones, one of the patentees, Mr. Walsh, and Mr. Goodman; and I shall feel greatly indebted to them if they will give the members the benefit of their experience, and fill up the blanks which I may unintentionally have left.

Mr. JONES said he did not know that there was very much which he could add to what Mr. Clapham had already said, but he might perhaps give them a short account of how they came to adopt this furnace. He might say they were absolutely driven to it by the misconduct of that very low class of workmen, the decomposing men. They had their troubles at Middlesbrough, as well as we on the Tyne had ours; and with the men constantly getting drunk and coming in and breaking the pots, it became at last a serious question whether they could not dispense with them altogether; and he made up his mind that they would endeavour to do so, even if it cost them more to do the work by mechanical means than by manual labour. That was their motive in trying this plan. Now, they found at first that continuing a few of these men to work the furnace, they had very great difficulty in getting it to work at all. By some mysterious means which they had not the means of ascertaining, the furnace perpetually went wrong at night: a cog broke, or the crown wheel gave way, or something or other went wrong; and they did not get to the end of that trouble until they had discharged every decomposing man from the place and got common labourers to do the work. Since then they had got on very much better with the labour department. He might say that now the labour was done by one man—a strong, serviceable fellow who had been a cab driver, and he drove the machine by day, and his mate by night; so that by means of one man each shift they were now doing the work which formerly required three men—that was to say, they had a decomposing man at the pot, and of course they had a man at the roaster, and a man wheeling to the pot. Now, their one man does the whole of this work with the present machine. He wheels his own charges, he charges the machine, he minds it during the operation, and he discharges it; so that they would see that in those points there was a very considerable saving of labour; and the fact was that at the present time they were simply paying this man—or rather these two men—who work the furnace at the rate of 1d. per cwt.; for that they did the whole of this work, and were earning nearly £3 a week each, so that they were liberally paid—too liberally paid. There was no doubt that by quickening the process a

little they could do better. They had not yet got quite all the satisfactory results they wished for, but by shortening the times of the charges, and getting a larger output, they would work the furnace considerably cheaper than they were doing. Their Mr. Walsh said he would not be satisfied until he got it done for $\frac{1}{2}$ d. a cwt., or 10d. a ton. He had not got it to that yet, but he had accomplished so much that he (Mr. Jones) did not doubt he would do it. As to the time of working the charges, they were at present working the furnace under a little want of draught, and therefore it had not yet quite fulfilled Mr. Walsh's expectations as to the amount of output; it was doing very little more than a smaller furnace which they had at work previously, and which had a better draught. But this was a mere mechanical matter, which could be put right in a few days. Then, it would be a great point to save time and save fuel by getting the charges put in and withdrawn by mechanical means, and a considerable saving could be effected in that way. Gentlemen would easily understand that a man could not throw five tons of stuff into anything with a shovel in a very short time; it took fully an hour—with a very good man indeed—to throw in five tons with a shovel; so that that hour is practically lost in charging a machine; and it took a still larger time to draw that charge out by hand. These were the two points which Mr. Goodman thought he would be able to improve upon very materially by means of the contrivance he showed upon that plan, and he (Mr. Jones) did not doubt it would be so. When these were added to the existing furnace, he was strongly of opinion that it would turn out fully what Mr. Clapham had stated in his paper that night, and perhaps more. He was not aware there was any other point which it was necessary to mention except this—that in Middlesbrough, as many were here, they were formerly always in hot water with regard to nuisance. This was always a very delicate point with them at Middlesbrough; and being isolated chemical works, they were more easily "spotted" than we were here on the Tyne; they could not throw it upon a neighbour as we could here. It was always known to be the chemical works; whenever there was a stink in the town, either from brickworks or anything else, the unfortunate chemical works had always to bear the blame. Well, now, fortunately with this furnace they had no trouble whatever in that respect. Many of the gentlemen present had been at the works during the time the furnace was working and while the charges were being withdrawn and laid on the floor, and he thought they would bear him out in saying there was no nuisance from escaping gas worth speaking about. Then, as regards the condensing, he thought this was a satisfactory point, viz., the whole of the hydrochloric fumes as well as the products of combustion pass from the furnace into the condensers. They had an arrangement for cooling the gases before they entered the condensers. Gentlemen had been afraid the heat would break the flags, but they need not have been nervous on that point; the heat was not so great as to damage any of the flags, and they cooled the gas a little before it entered the condenser by means of a jet of water. But the result of this arrangement was, they had the whole of their hydrochloric acid at a suitable strength: It runs from the condenser usually at about 26° T., and of course they got rid of the two towers. They had no roaster tower, but all the gas went through one, and was obtained from the condenser as muriatic acid at a strength of about 26° warm.

Mr. MOND asked if Mr. Jones would state what quantity of muriatic acid 26° he obtained from a cwt. of salt. It looked to him to be a wonderful result for one tower getting heated acids to complete the condensing. He knew they had not been able to do so in Lancashire, where they had worked hard to improve their furnaces.

Dr. LUNGE thought it only fair to state as an unbiassed witness that Mr. Mond had assured him that the condensation at Messrs Jones and Co.'s works was excellent; and when he (Dr. Lunge) was there, a gentlemen who

was there along with him assisted Messrs Jones's chemist in taking a chimney test; he was present at the test, and they then found the escape only to be 0·1 of a grain per cubic foot—which of course was an excellent result. He thought the explanation of the difference between the results of Messrs. Jones's furnace and that used at the Lancashire works—those, for instance, of Messrs. Hutchinson—where both the oven and roaster gases are going along into the condensers mixed with fire gases was this—that, in the first case, the evolution of the gases was over quickly, while here it was given off very gradually, and of course the condensation was comparatively easy in that respect; and, secondly, the temperature of the furnace was evidently far below that of an ordinary open fire-grate. He might say that the cooling arrangement at Messrs. Jones's works, as Mr. Jones himself had mentioned, was only of a temporary nature, and might, in his opinion, be improved greatly. He went to the works with this question in his mind—how the condensation of the acid might be effected; whether the condensation of the acid could be effected completely, and a strong acid? and he came away entirely satisfied on that point. As he was on his legs he would like to ask another question—that was with regard to the output. The output of the furnace was stated at 80 tons. When he was at Middlesbrough the charges were not so much as that; he thought it would be only about 50 or 60 tons they would be getting there. Of course the pan was not so large as the pan which Mr. Goodman had designed; and it was also perfectly clear that the mechanical arrangement for charging and discharging would lessen very largely the time in which the process could be completed, and possibly enable the three charges to be made into four—the charge to be completed in six hours instead of eight, which of course would very largely increase the production. On this point it might perhaps be interesting to many members to receive some more specific information from the patentees.

Mr. GLOVER asked if Mr. Jones would also state the superficial area and cubic capacity of his condenser?

Mr. JONES said, Mr. Walsh told him that their condenser was 6 feet square inside and 40 feet high, and the meeting could easily work that out. In reply to Mr. Mond, he could not say exactly the proportion of hydrochloric acid obtained per cwt. or ton of salt—they never worked exactly to that scientific accuracy. They had, however, got a good name for condensing, as Dr. Lunge had said, and Mr. Todd (the inspector) thought it very good. He had before him a few of the tests, from the condenser itself, taken by Mr. Todd in the chimney:—Feb. 4, it was 0·1; Feb. 7, 0·09; Feb. 10, 0·08; Feb. 14, 0·11, &c., &c. None of them exceeds 0·2.

Mr. GLOVER—I infer that your draught is from the top of the condenser into the chimney.

Mr. JONES—Yes; we have a pipe from the top of the condenser down to a flue leading to the chimney. Then with regard to the wash tower, to which Dr. Lunge had referred, there must, of course, be some small amount of gas condensed by the spray there, but he had tested it himself personally several times, and had never found it stand higher than 1°, so that it did not amount to much, and water of that strength could easily be pumped up if necessary, and used in the condenser. They did not do that at Middlesbrough; they had no arrangement for it. The very gradual way in which the gas was evolved was, as Dr. Lunge had stated, in favour of a perfect condensation. They did not get it off in a great rush, as they used to do in charging a decomposing pot. The condenser used to be overpowered, and there was a back draught into the faces of the men. Now, the salt was gradually charged into the furnace, and came gradually into contact with the acid. Another little matter that occurred to him was, the question of the wear of the iron work of the furnace. Whilst the arms of the agitator were of wrought-iron there was a tendency in them to bend considerably, so that they had to stop the furnace for the purpose of

renewing these arms. In the furnace now at work they had no wrought-iron work, the arms and scrapers were made of metal. There was on the table a sample of a scraper, which had been working in the present furnace for nearly three weeks. That was the original form and size of it, and, as gentlemen would see, it had worn down only three-quarters of an inch during that time. That was the whole of the wear of the iron work during that time; there had been no visible or perceptible wear in the pan itself. They had had the pan, which was erected last November, very carefully examined (it had been working very steadily up to now), and there was no appreciable wear of the pan. The rest was closed-in brick work, and the heat, not excessive, passing over the surface of the stuff did not seem to affect the metal work of the pan at all.

(To be continued.)

CORRESPONDENCE.

THE CHEMICAL SOCIETY.

To the Editor of the Chemical News.

SIR,—Of all the aggrieved who have lately occupied so much of your valuable space not one has clearly stated the reason of the systematic and indiscriminate blackballing resorted to by some of the members. A kind of undefined vague feeling seems to prevail amongst them that by a proceeding of this kind the status of the Society will be raised: the motive is an excellent one, but the results of the movement have been anything but satisfactory. Mr. "Twig" writes to say, "We want unity and vitality," and he then proceeds to use his utmost endeavours to destroy the unity and injure the vitality of the Society. Some of them, again, compare the Chemical with the Royal Society, forgetting that the latter is a close society, and therefore differing entirely from the former. The real question is—Does the Chemical Society adequately carry out the object for which it was founded, viz., "The general advancement of Chemical Science?" No one who has watched the progress of the Society during the last six or seven years could conscientiously say that it does not. It follows, therefore, either that the grievance of the blackballers is fictitious, or that they have a design to alter the constitution of the Society.

The character of a large Society like ours depends, not so much on the qualifications possessed by individual Fellows, as what they do collectively to advance science. It is simply ridiculous to speak of the Society "in words of open contempt" because some of the Members are not "chemists by profession." In face of the fact that some of the Officers of the Society are *dilettanti* who have earned a European reputation by their labours in chemical research, the blackballers would exclude a man merely because he is a *dilettante*—because he does not practise chemistry as a profession. It is possible there may be some Fellows of the Society who abuse its privileges and make use of the letters F.C.S. to impose upon the unscientific public. These, however, are not *dilettanti* but "professional chemists," whom it would be vain to attempt to exclude by the requirement of a statement of qualifications, so much insisted on by the blackballers: as in the case of the vendors of spurious degrees or promoters of bogus companies *et hoc genus omne* they would no doubt find it an easy matter to prepare a certificate which would appear very satisfactory in this respect. For such men, however, to obtain the signature of our President, or any member of like standing, would be an impossibility.

Prof. Roscoe calls the blackballing meaningless. It is to be feared, however, that it is not so in all cases, but that self is really at the bottom of the movement; how otherwise should we have some of your correspondents complaining that it is not an honour to belong to the

Chemical Society, that the letters F.C.S. are not "something of definite value," which in plain English means that the mere fact of being a Fellow of the Society does not materially help them in their professional career. It is quite true F.C.S. put after a name does not signify that the possessor is a good chemist, and, what is more, it was never intended it should do so.

A word on a personal matter. One of your correspondents say the writer of the present letter is himself a "younger brother." It is true. I have scarcely attained my chemical majority; for although I had some knowledge of the science at a comparatively early period, it is but little more than nineteen years ago since I began to study it systematically under Dr. Hofmann, and it is not yet quite sixteen years since my first paper was read before the Chemical Society. I suppose your correspondent thinks all young men ought to join the pseudo-reforming clique. Blackballing, like measles, hooping-cough, and other infantile diseases, is no doubt "catching," but it is sincerely to be hoped that it will not become epidemic, or at all events that some of the younger members may escape. That the blackballers at present are but few in number, notwithstanding some of them have said to the contrary, is a fact which admits of easy verification. Any one who was present at the meeting on the 15th, and took the trouble to count the voters and divide the total by four, would have found considerable difficulty in adjusting the small quotient so as to include "numerous followers and able leaders." Some of the blackballers seem to think the Fellows are ignorant of the ordinary rules of arithmetic.—I am, &c.,

CHARLES E. GROVES.

PS.—Allow me more particularly to draw the attention of your readers to the circumstance, already alluded to by Professor Roscoe, that not one of the partisans of blackballing has had the courage to append his name to his letter.

ORGANISATION AMONG CHEMISTS.

To the Editor of the Chemical News.

SIR,—Feeling considerable interest in the scheme for an organisation amongst chemists, and noticing that most of your correspondents appear to be gentlemen who have already obtained a fair position as chemists, I venture to address a few words to you from a point of view that has not, as far as I have seen, been taken up in any of the numerous letters upon the subject, namely, the great advantage that would accrue to those who are desirous of making chemistry their profession if the proposals of an organisation were fairly and fully carried out.

At present there are no schools of chemistry the course of instruction in which qualifies a student for the profession, neither are there any examining bodies to which he can apply to show his practical capability of undertaking any kind of chemical work.

Those students who have obtained their knowledge by private study or by attending the smaller science schools find their path beset with difficulties. It is useless for them to refer to their teachers for recommendations, for these would carry no weight, and there being no society from which they can obtain qualifications their efforts to obtain employment would probably be in vain. Of course those students who have been assistants to or studied under well-known Chemists will not have these difficulties, but then these are not always the best adapted to the profession. There are men who have never had these advantages who would be likely to make great progress if they could obtain a fair beginning.

Then, again, an organised society with power to confer distinct qualifications to its members would raise Chemistry to the level of the other professions, a position which it certainly does not at present occupy. The outside world has scarcely the shadow of an idea what is implied by the term "chemist," and even those positively engaged in the

application of chemistry do not know with much greater certainty the range and limits of their profession. Of course they all have some kind of an idea, but these differ considerably.

There is one point, however, which, for my own part—and in which all persons who have spoken upon the subject agree with me—I must strongly object to in the proposed regulations, and that is excluding everyone who is under twenty-six years of age. With any of the other professions the full qualifications can be obtained by the age of twenty-one, and it is unreasonable to make it more difficult to enter the chemical than the other professions.

However, that an organisation of some kind be formed is of more importance than it seems on the surface. It is of small import whether this be connected with the Chemical Society, the Chemical Department at South Kensington, or whether it exists upon a distinct foundation. Trusting that the desired end will be obtained by those gentlemen who are working to place chemistry upon a fair basis as a profession.—I am, &c.,

F. SEXTON, Assistant D.C.

Dulwich College, Dulwich, S.E.

THE RELATIONS BETWEEN THE CHEMICAL SOCIETY AND THE ORGANISATION MOVEMENT.

To the Editor of the Chemical News.

SIR,—The Council of the Chemical Society having now definitely decided on the course of action which they are prepared to recommend to the Society (subject to the acceptance thereof by the promoters of the organisation movement), the Members of the Council are now free to do what they were hitherto debarred from doing, viz., to discuss publicly the various aspects of the question, and to promulgate their own views on the subject. I would therefore beg space for the insertion of the following remarks on the general aspect of the question.

I may as well premise that since the publication of my first communication on the subject (CHEMICAL NEWS, vol. xxxiii., p. 40) my own views have undergone more than one change. At first, I was disposed to think that the objects of the desired association or guild of chemical practitioners were entirely different from the ground hitherto covered by the Chemical Society; that the policy of that Society hitherto adopted rendered it unsuitable as an origin of the desired association; and that the Society and the Professional Association were entirely separate in their nature, and incapable of fusion together without injury to both. I subsequently was led to think that there were numerous difficulties in the way of establishing such a separate association: and that although a scheme for organisation through the Chemical Society would not by any means do all that seemed necessary and desirable to be done, it would nevertheless enable a distinction to be made between competent and incompetent men, and that it might be preferable to sacrifice a portion of the ends to be aimed at in order to facilitate the attainment of some of them. Finally, an attentive study of the nature of the steps which appear to be possible for the Society to take under the existing Charter has induced me entirely to revert to my former opinions. So few of the results aimed at by organisation appear to be likely to be effectively attained if this be attempted to be carried out through the Society that I am reluctantly compelled to believe that in proposing to do anything of the kind the Society will only weaken itself by uselessly stirring up dissention amongst its members.

Collating the opinions of a large number of chemists of high repute from all parts of the United Kingdom, who have been privately communicated with by one or other of the gentlemen who, with myself, have endeavoured to bring this subject prominently before the chemical public, I conclude that what is generally desired is to put the

occupation of a consulting and analytical chemist on a par with other recognised professions, *i.e.*, to give it a legal status; to do as is done in the clerical, legal, and medical professions, *viz.*, to prevent as far as possible persons who are not properly qualified to do certain work from being able to impose on the public generally, by obtaining (or at least endeavouring to obtain) suitable enactments preventing such persons from being able to give certificates admissible as evidence, and to *enforce* payment for their services: to establish by the general consent of those practising the profession a Board controlling professional malpractices, so as to take away the stigma which the existence of "high" and "low" analysts and the like have brought on chemists generally, and to establish an association which shall have the power to take such steps, as circumstances may from time to time render desirable, for the public welfare and the efficient discharge of the duties of the sanitary officer, the manufacturer, the analyst, and all the other citizens whose occupation necessitates a thorough knowledge of chemistry in some form or other.

It has been objected that this is a "Trades' Union." So it is, in the same sense that the legal and medical organisations of this country, the Established Church, and many other institutions of repute are "Trades' Unions." An association of professional men (*i.e.*, brain-workers) for the mutual advantage of the community at large, and of themselves, does in truth bear considerable resemblance in many respects to an organisation for the same purposes amongst handicrafts'-men: in fact, the chief distinction lies in the nature of the work done, mental *versus* bodily. These correlations and differences are embodied in the very term "Professional Association."

The establishment of a Professional Association of this kind, then, being the object to be aimed at, the question arises, What are the steps that should be taken in order to effect this object in the best way? I only propose here to deal generally with the broad question, "Should the Chemical Society, if practicable, be made the starting-point for such an Association." As for the details, either of the plans proposed by the Council of the Society, or such other scheme as the Fellows generally may think fit to propound, they may be safely left to after-consideration when the subject comes (if it should come) before a general meeting of the Fellows.

The principal arguments on behalf of and against this broad question I take to be as follow:—

Firstly for the Proposition.—It may be argued (1) that unity in the chemical world is a desideratum, and therefore *a priori* the Society should be the centre of the movement. The truth of this is incontestable, provided the nature of the Society permits of its being the centre of the movement, which is the point at issue.

(2.) Many persons think that the Society would be very likely to suffer in numbers, funds, and influence were an Association to be formed entirely disconnected from it, whilst the contrary would be the case were the Society intimately connected with the movement. This is a matter of probabilities and opinion. Many persons think, on the other hand, that no injury to the Society would accrue were the two entirely disconnected. Whilst many doubt if the Society, as a society of a *scientific* character, would be really permanently benefitted by being connected with a Professional Association.

(3.) Were an entirely separate "Institute" started there is at least a possibility of its opponents and rejected candidates endeavouring to form a rival association; and as the general public would not for a long time distinguish between the association of competent men and that of inferior ones, such a proceeding would to some extent hamper the movement: whereas, if the movement have its origin in the Society, successful rivalry on the part of an opposition would be, it is alleged, well nigh out of the question. It cannot be denied that there would be a great advantage if rival associations of imperfectly qualified men could be practically rendered impossible of formation; but as to whether the kind of development of the Chemical

Society which alone seems practicable under its Charter would be more or less efficacious in this direction than an independent association there is clearly room for much difference of opinion. Many with whom I am acquainted think that the imperfect character that any scheme working through the Society must necessarily possess would rather render opposition schemes more probable of initiation than would be the case were a more thorough independent association formed.

(4.) As a matter of policy, various influential men are supposed to be willing to forward a plan for organisation which might add to the power and influence of the Society, and more particularly of its Council, whilst they would either actively oppose, or at least hold aloof, from a movement not springing therefrom. Unfortunately, there appears to be considerable truth in this argument. Still, in the event of the majority of those interested in the question deciding that an independent association is on the whole preferable, even the active opposition of a few, however highly placed, could only delay and retard the development and progress of such a scheme, and would not suffice to frustrate it.

Secondly, against the Proposition.—It is alleged that (1), whether rightly or wrongly, a by no means inconsiderable number of Fellows of the Chemical Society are dissatisfied with its government and policy generally; the constitution and mode of election of its Council does not meet with universal approval, and there is, in consequence, a possibility of internal storms arising at no distant period. Under these circumstances, to connect itself with a movement of a most reforming character is a dangerous action on the part of the Society, its stability being thereby somewhat imperilled; whilst for the movement to ally itself with an institution the members of which are not in harmony is unwise, as it would seriously handicap the movement.

(2.) Under the Charter of the Chemical Society Fellows voting at the election of Officers and Council must be personally present at the annual election; proxy-voting and voting by letter are not possible. In the election of a Board of Management of a Professional Association it is manifest that every qualified man ought to have a right to vote whether he be bodily present or not.

(3.) The Charter of the Society, although enabling (in the opinion of eminent lawyers) more than one kind of Fellow to exist, would not admit of any other title being applied to the body of professionally competent men selected from the general body; so that these selected Fellows must be distinguished as "Professional Fellows" or "Practising Fellows," or in some analogous way. Now, firstly, it seems not at all unlikely that the general body of Fellows would refuse their assent to the establishment of such a selected class consisting of only a fraction of the total number of Fellows, on the ground that the others would thus be degraded to a lower level, and that this was not precisely the object they had in view in joining the Society. Secondly, it is open to question whether such a title as "Practising Fellow" would be sufficient to distinguish thoroughly the competent selected men from less thoroughly qualified individuals who might happen to possess the F.C.S.; in short, whether the chief practical object in view—*viz.*, distinguishing sharply good chemists from indifferent ones—would be adequately effected in this way.

(4.) Certain clauses in the Charter of the Society render it impossible that this Society should be developed into a Professional Association of the desired kind without altogether destroying its character as a Scientific Society, if indeed such development be at all possible. The Council of the Society (subject to the direction and control of the general meetings of Fellows at which every Fellow has a right to be present and vote) is the only committee or body which can "direct and manage the concerns" of the Society. Any sub-committee or Board appointed by the Fellows (or by the Council subject to their consent) must either be directly subordinate to the Council or to

the general body of Fellows, or both. New regulations and bye-laws can only be made and established at General Meetings of the entire body of Fellows, and are only valid provided they are not inconsistent with the Charter (which, like most documents of the kind, is in various places susceptible of more than one construction). Consequently, supposing that a Committee or Board were appointed, either by the whole body of Fellows or by the Council subject to their consent, for the purpose of taking charge of the matters which would form the business of a Professional Association, the decisions of the Board could only be made *subject to the approval of the Council, or of the whole body of Fellows, or both*. The power and authority which such a Board *must* possess if it is to be at all efficacious as the executive of a Professional Association would not be possessed by the Board appointed through the Chemical Society. Such a Board would have no power over the funds subscribed by professional men for professional purposes. It could only recommend to the Council that the funds should be employed in such and such ways, and could not enforce their employment in these ways should the Council not see fit to act on their suggestions; and it could not prevent the Council (or at any rate a general meeting of Fellows) from appropriating these funds to wholly different purposes should a majority decide on abolishing or modifying the then existent bye-laws relative to these funds. Such a Board could not make regulations which *must* be obeyed by every professional man belonging to the Association. It could only recommend to the Council certain new bye-laws, and the Council could not ratify these without calling together a general meeting of the whole body of Fellows, so that *the regulations of the Association would finally be subject to the approval of a body, the majority of which could not belong to the Association*. Such a Board, indeed, could never have any permanent character, as its very existence would be entirely dependent on the assumption that at every future annual meeting the general body of Fellows were pleased to approve of its action, and to reappoint it for such a period as some special bye-law should state; and even the bye-laws to which the Board owed its existence might be revoked and altered at a future general meeting, and the whole class of Practising Fellows abolished.

One natural result which would spring from the establishment of such a Board would be that the Professional Fellows of the Chemical Society would combine together so as to alter the mode of electing the Council, and so to influence the elections as always would ensure the decisions of the Board being of necessity ratified so far as the Council could ratify them; in other words, the Council would become not the managing committee of a purely scientific society, but that of a professional association pure and simple. Alterations in bye-laws other than those relating to the constitution of the Council would be gradually introduced; intestine squabbles in the Society would be rendered imminent; and, finally, neither the Society nor the Association would be an institution of stability. From the point of view of the interests of the Society, it seems extremely undesirable that such a course should be adopted as will inevitably lead to disputes and revolutionary measures. The tendency towards the latter is already strongly marked, but is as yet only taking the direction of useful reforms. The importation into the question of other considerations, however, might tend to pervert reform into total anarchy.

The consideration of the foregoing arguments, Sir, has finally induced me to conclude that whilst it is possible for the Chemical Society, under its present charter, to make distinctions between those of its Fellows who are well qualified professional men and those who are not, it is impossible to carry out through the medium of the Society a scheme for the thorough organisation of the chemical profession, whilst the attempt at drawing such distinctions is a dangerous experiment for the Society to make, inasmuch as it is by no means certain that such a

plan would be assented to by the Fellows generally, and discord and want of harmony would certainly be engendered and promoted. There still, however, remains the possibility of the Society giving up its present charter and applying for a new one empowering the Society to split itself up into two sections, Professional and Scientific, each section having its own Council, the one elected by the professional members only, the other by the general body of Fellows; and each section having supreme and independent control over the funds and general business pertaining to it. To this plan, however, there are the objections that almost perfect unanimity of opinion as to the desirableness of such a course must exist amongst the Fellows, and it is not very likely that this would be the case; whilst even if every existing Fellow desired it, it is very improbable that such a new charter would be granted.

On the whole, therefore, it is now tolerably clear to me that if a Professional Association of an effective character is to be originated, it must be apart from the Chemical Society. There then arises the question, Is it preferable that this Association should be wholly independent of any existing institution, or is it feasible to develop either the Chemical Section of the Society of Arts or the Society of Public Analysts into the required organisation? Many of the objections which apply to the general proposition of development of the Chemical Society would apply in these cases also. The Society of Public Analysts, moreover, has as yet hardly attained to such dimensions and favour in the eyes of the chemical profession as to render it a specially desirable nucleus, although in the event of its being clearly shown that an entirely independent institute would not best meet the views of the majority, it is quite possible that this Society might be rendered a more effective starting point than the Chemical Society promises to be.

In view of the approaching vacation and of the great interest which this subject has excited, it seems hopeless to expect that any definite conclusions will be come to by those interested in the subject before several months have passed: and indeed, precipitation in such a matter would be very unadvisable. But after those concerned have duly reflected on the various bearings of the subject, and have fairly weighed the different pros and cons so that each one may have looked well at the matter from as many points of view as possible, it is to be hoped that a definite scheme for the organisation and consolidation of the chemical profession will be arrived at, and that those who find themselves in the minority when a final division may be called for, will yield their own convictions for the general good, and that all will co-operate together to advance and perfect that plan which meets with the approval of the majority of those qualified to give an opinion on the subject.—I am, &c.,

CHARLES R. ALDER WRIGHT.

Scientific Club, 7, Savile Row,
June 25, 1876.

“PRELIMINARY NOTES” OF CHEMICAL RESEARCHES.

To the Editor of the Chemical News.

SIR,—On page 324 of the *Journal of the Chemical Society* for 1874, there appears a “Preliminary note on Perbromates,” by Mr. M. P. Muir, F.R.S.E., in which he states that an aqueous solution of perbromic acid may be easily obtained by the action of bromine on the hydrate of perchloric acid dissolved in water, and that this solution neutralised with potash deposits crystals of potassium perbromate. He also describes the formation of barium and copper perbromate, but in the communication there is not a single numerical quantity, reaction, or result of an analysis given. In the report of the last meeting of the Chemical Society in your journal of June 23, another preliminary note on the same subject

was communicated by the same author, in which, if the report in your journal be correct, he says that he has not been able to prepare the salts in the manner he before stated, in fact a contradiction of the results published in 1874. Now credit must be given to the author for having at once acknowledged the inefficiency of the method, but at the same time this case is typical of certain others which have occurred previously, in which statements founded on insufficient experimental data have been communicated under the form of preliminary notes, and I think it is a matter deserving of consideration as to whether notes of this kind should be published in the *Journal of the Chemical Society*. It is not as if we had only one record of chemical progress, but there are both the *Journal of the Society* and the *CHEMICAL NEWS*, and if I may be allowed to express an opinion I certainly think that notes of the kind alluded to should be published in the abstract of the proceedings of the society given in your journal, but not in the *Journal of the Society* itself, especially when they contain no numerical result, and may be found necessary of correction at some future date. This matter would have passed without comment had it not been that an investigation of the perbromates was being commenced in the laboratory here; but this, in deference to the preliminary note in 1874 having been given up, much valuable time is lost.—I am, &c.,

J. M. THOMSON.

King's College, W.C.

PATENTS.

ABRIDGMENTS OF PROVISIONAL AND COMPLETE SPECIFICATIONS.

Improvements in the manufacture of chromates of potash and soda and their bichromates. J. White, Fleet Street, London. March 1, 1875.—No. 746. This consists in slaking lime with sulphate of potash in solution, and adding chrome ore to this mixture.

A new and useful improvement or process of smelting and refining copper ores and metallic copper. S. L. Crocker, Massachusetts, United States of America. March 2, 1875.—No. 768. This complete Specification describes a process which consists in adding zinc in small quantity in a furnace to the copper ore or ores, or to the partially refined metallic copper therein containing arsenic and antimony, whereby these impurities can be readily removed.

Improvements in the method of and apparatus for the manufacture of glucose. S. H. Johnson, Lea Bank Works, Warton Road, Stratford, Essex. March 4, 1875.—No. 810. According to this Provisional Specification grain suitably impregnated with acid is treated with steam in such a manner that the steam may permeate amongst the grain and so rapidly bring it to the temperature at which the conversion takes place.

A new or improved metallic alloy. A. M. Clark, Chancery Lane, Middlesex. (A communication from A. Le Marquand, Paris.) March 4, 1875.—No. 815. The invention relates to a new white metallic alloy or white metal, which is not liable to oxidation, composed of the following ingredients:—

Pure copper	750	parts.
Pure nickel	140	"
Oxide of cobalt	20	"
Tin	18	"
Pure zinc	72	"

1000

Improvements in the manufacture of cast-steel ingots, and in apparatus for casting and rolling the same, parts of which are applicable to rolling iron. M. Scott, Sardinia Terrace, Glasgow, Lanark. March 5, 1875.—No. 816. This invention relates to the casting of steel ingots, and to rolling the same or wrought iron. For casting the ingots, a number of moulds are set round a central pit on a wheeled platform with a spare mould for receiving overflow metal from the pit. The pit and moulds are divided longitudinally, the halves secured together by elastic rings driven into their taper exterior. The metal is run by a spout from the furnace into the pit, whence it flows by fire-brick runner pipes to the bottoms of the moulds. A slag box hinged on a truck with means of tipping it receives the cinder from the furnace after the moulds are filled, the spout being turned aside. The air-holes in the tops of the moulds are bellied, so that the ingot has a projecting head by which it can be conveniently lifted and moved. For consolidating ingots grooved rollers are used to give local squeezing by indenting the ingots with furrows, and then plain rollers for flattening down the ridges. To avoid the inconvenience of reversing or two-storied rolls, two sets of rolls are arranged end to end, one set a little in advance of the other, and are driven in opposite directions. The ingot or bar rolled in the one set of rolls is received on a cradle and transferred to the other set of rolls. The grooved rolls and arrangement and mode of operating with the two pairs of rolls are applicable in rolling wrought-iron.

An improved system of and apparatus for extracting mercury and other volatile metals from their ores. W. E. Newton, Chancery Lane, Middlesex. (A communication from H. Berrens, Paris.) March 10, 1875.—No. 894. The object of this invention is to precipitate or condense all the mercurial vapour that the uncondensable vapours (which accompany it from the furnace) alone shall escape into the outer air, while the vapour of mercury, together with such substances as water, tar, sulphurous acid, pyroligneous acid, or other condensable vapours, shall be found precipitated in the condenser. The sulphurous acid combines with the oxygen of the water, and is precipitated in the form of sulphuric acid.

NOTES AND QUERIES.

** Our Notes and Queries column was opened for the purpose of giving and obtaining information likely to be of use to our readers generally. We cannot undertake to let this column be the means of transmitting merely private information, or such trade notices as should legitimately come in the advertising columns.

Mercuric Iodate.—Allow me to add to the abstract of my paper on the above which is in last week's *CHEMICAL NEWS* the following:—Mercuric iodate dissolves in 2 molecules of KBrNaCl and NH_4Cl if the solution be boiling, and in 3 molecules in the cold. 4 molecules of KI dissolve 2 of Hg_2IO_3 .—C. A. CAMERON.

TO CORRESPONDENTS.

Nuntius, Cantab, F.C.S., Chemicus, Beta, R. G., Chemist, J. B. W., and others.—Owing to the extreme length of the Correspondence on the Chemical Society it has become necessary to exclude all letters which do not bear the names and addresses of the writers.

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